THE AMERICAN

ECONOMIC REVIEW

VOLUME XLVIII

P14411

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(In May 1958 number)

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THE AMERICAN ECONOMIC REVIEW

Volume XLVIII	MARCH 1958	Number 1

OTHER DEPARTMENTS

Number 59 of a series of photographs of past presidents of the Association

Morris A. Copeland

INSTITUTIONALISM AND WELFARE ECONOMICS

By Morris A. Copeland*

We in the West are proud—and justly proud—of our free enterprise economic system. Nonetheless, if you look for the kind of rounded description and explanation of this type of system that the proverbial man-from-Mars would need in order to understand in broad terms what it is and how it works, you will not find one. I know of no published explanation that does not leave important gaps and does not badly need to have a number of qualifications attached to it.

Such descriptions and explanations of the free enterprise way of organizing economic activity as we have are to be found chiefly in connection with statements of principles of economics in general introductory texts and texts on intermediate theory. These statements commonly tell only a part of the story of free enterprise and a rather misleading part at that. The aspects of a free-enterprise economy that they typically select for consideration can fairly be characterized as mechanisms, aspects that lend themselves to the techniques of model analysis. And they slight what may be called the organic aspects of a private-enterprise economy, particularly those aspects that reflect changes in legal and economic institutions, and those like the master-servant relationship that are not easily treated in the terms of an individualistic ideology. I do not mean to say that most introductory and intermediate theory texts neglect these organic aspects entirely; only that consideration of them is often very scanty and is quite commonly put in a separate chapter rather than made a part of the explanation of the private enterprise way of organizing economic activity. But such scantiness and separateness of treatment can be an extremely serious fault, particularly when one is concerned with the public-policy implications of a statement of principles of economics.

For a time I had planned to try this evening to indicate in some detail the kind of statement we ought to have for our free enterprise way of organizing economic activity. But even an illustrative formulation would have taken far too long and would have required my making many points that are commonplaces for all of you along with less familiar ones. Instead, therefore, I have decided to concentrate on some of the leading public-policy implications that such a statement would entail.

I

When we speak of a nation or community as having a free enterprise economic system, we imply by the word system that while there is no general manager managing its economic activities, the people that make up the nation or community in large measure act as if there were. An adequate descriptive and explanatory statement for this kind of economic system would call for taking at least the five following steps: First, one should specify what general or central management functions under such an arrangement are performed without a general manager. Second, one should identify and describe the institutions that perform these central management functions, for it is human institutions—not an Unseen Hand—that serve in lieu of a general manager. Third, one should explain how these institutions work, indicating the part played by each institution in each function. Fourth, one should consider any deficiencies in the way the institutions perform the functions. And fifth, one should note the public-policy implications of such deficiencies and consider what institutional changes have improved or might improve the performance of the functions. This fifth step would, of course, give us only a part of the answer to the question, What public economic policies should we adopt? But it is a significant part, because the functions provide something like an objective basis for policy judgments. Much of what I want to say relates to this part.

But what central management functions do institutions perform in our economy? In the interests of brevity I will not attempt to enumerate them systematically. There are four that are our immediate concern; two others I will take note of presently. The four are: first, determining how much of each good the economy is to produce; second, determining who is to do what, and seeing that the work gets done; third, determining how and where the available nonhuman resources are used; and fourth, determining the distribution of the economy's product. These central management functions are all quite familiar. In fact they may be said to be a primary concern of the principles-of-economics or model-analysis type of statement of our free-enterprise system.

The list of institutions that participate in the performance of these

four functions is fairly long, so I will mention only two of them to which I propose to give particular attention: the profit system and competition.

Before considering these two institutions let me say a few words about some shortcomings of the principles-of-economics or model-analysis type of statement of our free enterprise system. For one thing it largely ignores one of the main central-management functions, that of establishing the master-servant relationships that make a business organization possible. I want presently to comment on this function. Another shortcoming of the model-analysis type of statement is that it almost inevitably plays down historical changes and intercountry differences in economic organization. The same "principles of economics" are assumed to apply before and after the industrial revolution; the same "principles" to apply to all free enterprise countries. A third major shortcoming is that a very inadequate basis is provided for analyzing deficiencies in the way human institutions perform central-management functions.

It is true the model-analysis type of statement of a free-enterprise system gives full recognition to the imperfections in our economic organization arising from private monopoly. And at least since Keynes' *General Theory* such statements have fully recognized the undesirable consequences of an inadequate level of aggregate demand. But there have been other serious deficiencies in the way our institutions have performed central-management functions. One of the principal amendments needed in the model-analysis type of statement is that the discussion of the four central-management functions with which it is primarily concerned should include a more comprehensive and systematic treatment of these deficiencies and of any changes in institutional arrangements that have served or might serve to remedy them.

More than a century ago John Stuart Mill in the last chapter of his *Principles* incorporated a systematic treatment of the problems he saw arising from the institutional deficiencies in the British economy of his day and the measures that had been or could be taken to remedy them.[1] We very much need an amended and modernized vision of Mill's treatment, for there are significant differences between that economy and ours today. Also we have acquired in the meantime a good many significant bits of economic knowledge that would be appropriate for bringing his last chapter down to date either for the British economy or for ours. Nonetheless no really comprehensive modern formulation along these lines has been put together. It is true that in the decade or two before

[1] J. S. Mill, *Principles of Political Economy*, Ashley ed. (New York, 1923), Book V, Ch. 11.

the great depression a good deal of work was done on this phase of economic theory. But unfortunately since the early 1930's little attention has been paid to the subject. Surely it is time economists took it up again.

I want therefore to indicate what a modernized version of Mill's last chapter would entail. I propose to do so by noting some of the main points—in addition to those relating to monopoly and to inadequate aggregate demand—that it should deal with. These points can conveniently be grouped under two heads, those that relate particularly to the working of the profit system and those that relate primarily to the way competition works.

Let us take the profit-system points first. The working of the profit system is inevitably conditioned by the legal rules that govern the conduct of business and by the business practices that have developed under those rules. Satisfactory operation of the profit system assumes that what is profitable is also good public policy and that what is good public policy is also profitable. Of course this assumption is true only to the extent that we have developed legal rules and practices under those rules that make it true. And it is scarcely conceivable that the rules and practices that have developed could be so perfect that what is profitable and what is good public policy would completely coincide. But to the extent that they do not, there are deficiencies in the way our economic institutions perform basic central-management functions, deficiencies in the way the profit system works, most of which are not suggested by the model-analysis type of statement of the free-enterprise way of organizing economic activity.

Let me cite a number of illustrations or ways in which our legal system and practices within the law have been responsible historically for significant divergences between what is profitable and what is good public policy. I will cite them but for the sake of brevity I will not stop here to consider, case by case, what has been done toward correcting these defects in our scheme of pecuniary incentives.

1. Petroleum and natural gas resources. As has been pointed out by various students of these resources, notably Joseph E. Pogue, the application of the so-called wild animal theory of property to our subsoil mineral rights has led to competitive drilling and a consequent waste both in the excessive number of wells drilled and in the needlessly incomplete extraction and utilization of the oil and gas.[2]

2. Short-term leases. As A. C. Pigou has so clearly explained in his *Economics of Welfare*, it is nearly impossible to draw a short-term lease

[2] J. E. Pogue, *Economics of Petroleum* (New York, 1921), Ch. 3 and 28. See also C. G. Gilbert and J. E. Pogue, *The Energy Resources of the United States: A Field for Reconstruction*, U.S. National Museum Bull. 102, Vol. I, 1919, esp. pp. 84 ff.

for a parcel of real estate that will give the lessee the same incentive to take care of the property that an owner would have.[3]

3. The individual labor bargain. If what is profitable is to be in accord with the public interest, no one should ever be able to do anything that imposes any cost on the community beyond the cost he himself has to bear. But there have been various circumstances under which an individual could do something without paying the full cost. It has frequently been said of such a situation that the individual had to pay only the money cost, not the human cost. Thus the difficulty with the kind of bargain likely to be reached between an individual employee and an individual employer in the absence of protective labor legislation has often been thought of in terms of this distinction. But "human cost" is a vague, subjective concept. The costs an employer or other individual avoids ought to be more definitely identified. They commonly can be more definitely identified by a form of social accounting proposed by J. M. Clark. As he puts it—and his point is quite independent of any monopsony element in the bargaining—the cost of living is a kind of overhead cost to the labororer, but the labor contract makes wages a direct cost to the employer.[4] Indeed the labor contract is a species of short-term lease. It is similar to a real-property lease in that it gives the lessee (the employer in this case) an inadequate incentive to take reasonable care of the worker's health and safety. Further, under such a short-term lease the employer can be expected to avoid the kind of responsibility for a worker's maintenance when business is slack and when the worker is superannuated or technologically unemployed that he would have to assume if the worker were his property.

4. *De facto* torts. The distinction between money cost and human cost has been applied also to various other circumstances under which an individual could do something without paying the full cost. And in general, social cost accounting makes possible a more definite identification of the costs he has avoided. An important class of such avoided costs involve what I have elsewhere analyzed as *de facto* torts, situations in which an individual inflicts damage on another person or another person's property or exposes another person's life or limbs to danger, without that other person's having a reasonably adequate remedy at law.[5] There have been many cases of this sort, among them cases involving unsafe buildings and building installations, unsanitary

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[3] A. C. Pigou, *Economics of Welfare* (London, 1932), Pt. II, Ch. 9.

[4] J. M. Clark, *The Economics of Overhead Costs* (Chicago, 1923), esp. Ch. 2 and 19. See also "Soundings in Non-Euclidean Economics," *Am. Econ. Rev.*, Suppl., March 1921, Proposition 5.

[5] M. A. Copeland, "Communities of Economic Interest and the Price System," in R. G. Tugwell, ed., *Trend of Economics* (New York, 1924), pp. 105–50.

arrangements and practices, air pollution, stream pollution, soil erosion, spring floods and fall droughts, and uses of real estate in ways that adversely affect the value of nearby properties.

5. Eminent domain. As Richard T. Ely emphasized, there are under existing property rights some types of project that, though definitely in the public interest, it has not been profitable for a private enterprise to undertake without the aid of a delegated right of eminent domain.[6] Projects such as constructing a railroad or a canal or damming up a stream are cases in point.

6. Corporation management. The financiers and officers of a corporation have often found ways to make money for themselves that have had the effect of taking money out of the pockets of the corporation's stockholders. High finance has been a source of many large fortunes. Thorstein Veblen was by no means the first economist to give attention to this peculiarly lucrative form of pecuniary employment, but the penetrating analysis he gave it surely deserves mention here as we commemorate the hundreth anniversary of his birth.[7]

I have cited these six cases of significant divergences between what is good public policy and what is profitable to suggest that there have been a great number and a great variety of such divergences quite apart from any that result either from mere monopoly restrictions of output and monopsony restrictions of input or from inadequate aggregate demand. It seems to me economists should be actively concerned both to discover such divergences and to devise remedies for them.

But I do not mean to imply that this task of correcting defects in our scheme of pecuniary incentives is one that can ever be completed. There will continue to be many significant divergences between what is profitable and what is in the public interest. For one thing people's ideas about what is in the public interest change with the passage of time. For another both economic conditions and the law are continually changing, and with changes in economic conditions and changes in the law new divergences are bound to develop. Indeed, when one reflects on all the aspects of the law that condition the profit system, it is clear it could hardly be otherwise. Anything like perfect coincidence of the profitable and the common good would require something like perfection plus perfect up-to-dateness in the law of property; of conditional sale, lease and other forms of contract; of torts; of debt and collateral security; and of partnerships and corporations; not to mention the

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[6] R. T. Ely, *Property and Contract* (New York, 1922), esp. Pt. I, Ch. 20.

[7] T. Veblen, *The Theory of Business Enterprise* (New York, 1915), Ch. 5 and 6. Also, "On the Nature of Capital: II. Investment, Intangible Assets, and the Pecuniary Magnate," *Quart. Jour. Econ.*, Nov. 1908, XXIII. (Reprinted in *The Place of Science and Modern Civilization* [New York, 1919], pp. 352-86.)

special kind of perfection and up-to-dateness that would be needed in our tax laws.

One reason why such perfection and up-to-dateness are unlikely ever to be realized deserves special mention here. This reason involves an aspect of the profit system that by its very nature lies outside the field of phenomena the ordinary type of economic-model analysis can handle. In general any model for the profit system assumes the profit of a firm to be defined as the algebraic sum of a list of revenue and cost items; and throughout the analysis of such a model the list of these revenue and cost items must necessarily be held constant. Thus implicitly the analysis assumes the firm operates under some set of legal rules and interpretations of those rules, and takes the rules and interpretations as fixed. But certainly businessmen often give telling support to proposed legislation they deem good for their business, and they are continually seeking new favorable legal interpretations—or what comes to the same thing, new loopholes in the law—not to mention the fact that getting some special discriminatory favorable consideration from a public official has all too frequently been regarded as a profitable expedient. One thing the pursuit of profit means is seeking favorable changes in the way profit is defined and determined.

II

So much for the policy implications of the role of the profit system in organizing economic activity. In considering policy implications of the role of competition we need to note another central-management function, that of regulating the prices and other terms on which trading takes place. This function is of course auxiliary to the four we have so far been largely concerned with, for they will only be performed as they should be if the economy's scheme of pecuniary rewards and incentives is appropriate to induce such performance, and we will only have a scheme of pecuniary rewards and incentives that is appropriate in all its details if the regulatory function is properly performed, *i.e.*, if the prices and the other trading terms are just right. So far as we rely on the impersonal operation of institutions for performing this auxiliary regulatory function we rely primarily on the institution of competition.

Under the hypothetical conditions of a perfect competitive market the operation of the regulatory function is quite simple. Buyer competition pushes prices up, seller competition pushes them down to a uniform level that leaves no potential buyer or seller who is willing to trade at that level unsatisfied. However an actual market adjustment that approximates this kind of perfection will not necessarily be one that conforms to sound public policy. The price may represent a cut-throat

competitive level; indeed in its pertinent empirical sense marginal cost pricing would mean cut-throat pricing for most businesses most of the time. Moreover, the market that approximates the perfect model might be—say—the market for a dangerously infected food or the market for the services of prostitutes. Still the clearest case for relying primarily on competition to produce an adjustment that conforms to sound public policy is in a market where there are many traders on both sides and where all terms except the prices are uniform and fixed so that perfect market conditions are approximated.

Alternatively when price competition and competition on other terms of trade are combined, the regulatory function may be impaired. Indeed, as Henry Carter Adams long ago pointed out, in such circumstances the tendency may be to lower the plane of competition and "force the moral sentiment pervading any trade down to the level of . . . the worst man . . . in it." This quotation comes from Adams' essay entitled "The State in Relation to Industrial Action."[8]

An extreme difference between the number of buyers and the number of sellers in a market is a further circumstance that may impair the regulatory function of competition, for it is likely to mean that the less numerous traders on the one side of the market are specialists at the trade, while the very much larger number on the other side are amateurs.

Nonetheless, where the more numerous traders are buyers, even when sellers are competing in quality of product and in sales effort as well as in price, provided certain conditions are satisfied, it is possible that the regulatory function of competition may be reasonably well performed. Some years ago J. M. Clark proposed the term "workable competition" for these conditions.[9] The two main ones he specified are: (1) that the demand curve confronting the individual seller shall be neither too elastic nor too inelastic, and (2) that there shall be just enough of a tendency for sellers to cut prices in dull times and raise them when business is brisk—that is, just enough demand elasticity and just enough price flexibility to assure prices that shall be neither unreasonably high nor unreasonably low. But presumably Clark had in mind at least one further condition. I assume he meant a market in which a limited number of sellers announce their prices and other terms of sale in advance and in which buyers are moderately well—though not too promptly—informed regarding seller deviations from list prices.

[8] *The State in Relation to Industrial Action,* Publications of the American Economic Association, Vol. I, No. 6, 1887. This essay and Adams' presidential address are reprinted in J. Dorfman, ed., *The State in Relation to Industrial Action and Economics and Jurisprudence* (New York, 1954).

[9] J. M. Clark, "Toward a Concept of Workable Competition," *Am. Econ.* Rev., June 1940, XXX, 241-56.

The concept of workable competition is undoubtedly a highly signifi-
cant forward step in our understanding of the regulatory function; and
I hope and expect Clark will give us a further significant forward push
tomorrow afternoon. However, let us not forget that the conditions
needed to make a market "workably competitive" are quite special con-
ditions; also that there are serious difficulties in controlling these condi-
tions and in knowing when they are realized; further that there are
various markets to which they do not apply; and finally that these
conditions seem pertinent primarily to the price levels charged by the
firms in an industry, not to the price differentials between the different
specifications of a product or between different delivery points. We are
still a long way from having a realistic, generally applicable concept of
competition that can be depended on for a reasonably adequate per-
formance of the regulatory function.

Nonetheless there continue to be two main ways of viewing this
regulatory function, especially as it applies in the strictly private enter-
prise area, *i.e.*, the area including all private, nonfinancial enterprises
except those classed as public utilities. It will be convenient to call
these two ways the institutional-supplements view and the perfect-
market view.

Anyone who delves into the history of our Association will be im-
pressed by the extent to which it was the institutional-supplements view
that provided the impetus to its establishment. The most active leaders
were young men in their thirties, like Adams, Ely, and J. B. Clark.
Fresh from their studies in Germany, these men believed various gov-
ernment actions were needed to supplement the working of competition
in the performance of the regulatory function. Let me cite the first sen-
tence of the statement of principles they wrote into the Association's
original constitution: "We regard the state as an agency whose posi-
tive assistance is one of the indispensable conditions of economic prog-
ress."[10]

The other view of the regulatory function of competition I call the
perfect-market view because it tends to regard the perfect-market model
as the norm which most markets should be made to approximate. On
this view if monopolies are effectively prevented or effectively broken
up, and if undue integration and unfair competitive practices are effec-
tively prohibited, the regulatory function of competition will be ade-
quately performed, so that, except for such efforts to maintain fair

[10] *Report of the Organization of the American Economic Association,* Publications of the
American Economic Association, Vol. I, No. 1, p. 35. Two of the older founders who had
expressed their convictions that the state should have a positive role in the economy were
the Association's first president, Francis A. Walker, and Andrew D. White. White was the
first president of Cornell University. Cornell has just celebrated the 125 anniversary of
his birth.

competition, a policy of *laissez faire* can and should in general be followed.

If in 1885 this perfect-market view was somewhat overshadowed by the view that in various circumstances positive government action is needed to supplement the regulatory influence of competition, it seems fair to say that since that date there has been a recurring tendency for economists to revert to a *laissez-faire* position. Indeed at the Association's fiftieth birthday party this tendency received some attention. Frank A. Fetter, the toastmaster on that occasion, spoke of *laissez faire* as "that most persistent concept . . . with more than the proverbial nine lives of the cat."[11]

That was 1935. But the tendency is still with us. Consider, for example, the school of thought that Kenneth Boulding, naming it after Pareto, calls "Paretian welfare economics." In his survey article on this school Boulding makes clear the thoroughgoingly *laissez-faire* nature of its usual economic-policy conclusions. The main ones are "a strong predisposition in favor of 'perfect markets' "; an insistence on "marginal cost pricing"; and "a general predisposition against" taxes that are deemed to "distort the . . . structure of economic activity"[12] (in other words against taxes that serve any regulatory purpose).

Since "Paretian welfare economics" is pure theory, its policy conclusions can be pure *laissez faire*—so pure that they seem pertinent to a hypothetical model rather than to the real world. But not all Paretians concur. And on the more specific level the negative conception of public economic policy is less extreme, though it is still negative. Those who take the perfect-market view of the regulatory function of competition tend to think of various government actions in regard to private, nonpublic-utility (nonfinancial) businesses as interferences with our free enterprise system, or—to use Boulding's phrase—actions that "distort the . . . structure of economic activity."

On the other hand those who take the more painstaking, institutionalist view of competition and the profit system tend to think of most government actions in this broad area as supplements to competition designed to make it and the profit system work better. The issue, then, is between regarding government actions in this broad enterprise area as interferences and regarding them as supplementary to the regulatory function of competition.

But there is more to this issue. The perfect-market view tends to treat differences in competition, provided it is reasonably fair, merely as a question of more or less. The institutional-supplements view re-

[11] *Am. Econ. Rev.*, Suppl., March 1936, XXVI, 320.

[12] K. Boulding, "Welfare Economics," in B. F. Haley, ed., *A Survey of Contemporary Economics*, Vol. II (New York, 1952), pp. 23-26.

gards the detailed nature of the function of competition and of the supplements to it that may be needed as varying from industry to industry and market to market. Simply listing the forms that government action has so far taken and that now command quite general approval lends strong support to this latter, more painstaking institutionalist view. Let me mention a few: forecasting crop production; prescribing minimum quality standards for most meats and for fluid milk; establishing grades and grade labeling for various commodities; a costly wire service providing day-to-day market information; requiring elaborate registration statements from the obligors of most large security issues; granting patent rights; insuring home mortgages; flexible regulation of margin requirements for security trading; requiring actuarial reserves for life insurance companies; requiring open competitive bidding in floating railroad securities. All these various forms of government action are aimed at supplementing the regulatory effects of competition so as to improve the operation of our economy.

In general such supplements mean that the government introduces modifications in the conditions under which business managerial decisions are made. The important point is that these modifications, if they are properly devised, do not involve the substitution of government decisions for the day-to-day decisions by the managements of individual businesses. We already have a great number and variety of supplements in this sense and as time goes on both the number and the variety seem likely to increase. Indeed economists should make it their business to discover and to propose new possible supplements.

Government can, by such modifications in the conditions under which business decisions are made in a particular industry, exert a powerful influence toward decisions that more closely conform to the public interest. Theoretically at least, there is another possible kind of government action toward an industry through which such an influence could be exerted, another kind of action that would not involve substituting government decisions for the detailed day-in-day-out managerial decisions of business. This other possible form of action is planning the constitution or structure of an industry. So far we have had no real experience in this country with such planning. The closest approaches are plans for the railroad industry that did not materialize. But perhaps one should also mention federal policy in disposing of surplus war plants after the second world war, private plans that have received judicial sanction in trust dissolutions and private plans to merge that have been disapproved. However, we have not really tried industry structure planning. But surely there should be instances where it would be possible to improve the functioning of competition through planned changes in an industry's structure. I think we are likely one of these

days to experiment with this kind of government action.

To my mind the fact that so little attention has been paid to this possibility is convincing evidence that the model-analysis oversimplifications of the profit system and competition and the persistent propensity for economic theorizing to push in a *laissez-faire* direction have tended to act as serious restraints on the freedom of economic inquiry and to discourage explorations by economists of ways—structural planning, for example—to improve the functioning of our free enterprise system. At all events current explorations of this sort are distinctly timid. I venture to hope that as we become more precise in our statement of the way competition and other institutions perform the central-management functions, our explorations of how to improve our private enterprise system will grow bolder.

Such greater precision should mean regarding many of the things government does in the economic sphere as supplements to the working of our free enterprise system, not as interferences with it. But there are clearly ways in which government action can interfere. Let me mention one. Freedom of enterprise means the management of a business has a wide discretion regarding the transactions it enters into so that in general for any individual transaction no other party's consent is needed except the consent of the other party to the transaction. But any law that leaves an unduly wide discretion to judicial interpretation, however laudable it may be substantively, interferes with freedom of enterprise during the period when it is being interpreted. Having to get a determination of the meaning of such a statute by a law suit can be quite as serious a restriction on enterprise freedom as it would be to have to get approval for a budget item from the economy's central manager if we had one. Every new law requires some interpretations; the vaguer or newer its wording the more it requires. The recently enacted internal revenue act is an example. We have put a needlessly large number of such free-enterprise-restricting provisions on the statute books during the past 25 years.

The two views of competition I have distinguished often differ on the adequacy of competition as a regulator that will induce the firms in a given industry to produce products of a sufficiently high quality and prevent them from charging an unduly high level of prices. Hence they often suggest different economic policies. But let me conclude my discussion of competition by commenting briefly on a matter of policy on which these two views largely agree. They agree in condemning discriminations in prices and in other terms of trade that serve no public purpose. And many of those who take the perfect-market view would agree with those who take the institutional-supplements view that competition is inadequate as a regulator when it comes to price differentials

and other discriminatory practices; for competition frequently means discrimination. In fact a competitor that gets ahead in an industry may do so in substantial part by developing business connections, *i.e.*, arrangements that give him preferential treatment in terms of financing, in terms of purchase, in access to market information, in the award of private contracts, even preferential treatment in the administration of a public office. The fact that various businessmen who have become public officials seem to have found it difficult to understand our conflict-of-interest laws bears witness to the prevalence of such discriminatory arrangements in the business world. They are not only prevalent; many businessmen seem to think freedom to make such arrangements is an essential feature of our free-enterprise system.

If there are significant restrictions on unjustifiable discrimination in prices and other terms of trade today they must be attributed largely to various supplements to the regulatory influence of competition. Both public opinion and business ethics impose such restrictions. And there are some legal restraints too, *e.g.*, in the Robinson-Patman Act. But there should be quite general agreement among economists that the right to develop business connections that give a firm preferential treatment is not a necessary part of our free enterprise system and that there is need to eliminate the many unjustifiable discriminatory practices still extant in the business world. The problem here is partly one of what means can be used to eliminate such practices but importantly also it is one of how to distinguish these practices from differentials in prices and other terms of trade that are in accord with the public interest. I hope the problem of how to make this distinction will be given far more intensive study during the next several years than it has yet received.

III

Although our free enterprise system operates without any central management, production is in large part carried on in organizations each of which specializes in particular phases of the productive process and each of which is under the direction of a management. Hence it has sometimes been said that while our economy has no general manager it has a large number of functional foremen. Because there are many organizations engaged in the productive process—many business enterprises each of which is a social organization, not merely a single individual—our economy involves the central-management function of providing for the functional foremen—determining who shall act as foremen, clothing them with authority to act as foremen, and imposing on others who participate in the work of any business enterprise the duty to obey its management.

Basically provision for the economy's functional foremen is made by two institutions, property and the wage system. The form of property involved is the business proprietorship or net worth. From the point of view of the accountant net worth is merely a residual claim to the assets of a business, what is left of the assets after the claims of creditors are satisfied. But the title to a business is much more than this. In the case of a sole proprietorship it conveys to the proprietor the right to manage the business and the right to the profits derived from it. In the case of an incorporated enterprise these rights are conveyed to the corporation as a legal person, which means in effect that the management is vested in the corporation's directors and officers.

The title to a business clothes the economy's functional foremen with managerial authority. At the same time the wage system imposes on its employees the obligation of subservience. As John R. Commons insisted, the wage bargain is more than a mere bargaining transaction between equals before the law, it is also what he called a "managerial transaction." It makes "One person . . . a legal superior who has the legal right to issue commands. The other . . . a legal inferior who, while the relation lasts, is bound by the legal duty of obedience."[13]

Thus the institution of the business proprietorship and the wage system together provide for the establishment and operation of business enterprises that are not mere one-man affairs but social organizations, and also provide for the expansion of such organizations. And in this kind of a social organization the management not only issues orders to the employees but also in any large enterprise it prescribes and administers rules governing their conduct as employees and inflicts penalties for disobedience, among others the penalty of dismissal. It need scarcely be said that making and administering shop rules and inflicting penalties involve quasigovernmental powers; the setting up a business organization means in fact establishing a kind of industrial government.

When we in the West speak of our free way of life we commonly have in mind both our free enterprise system and our bill of personal rights and liberties. Sometimes I fear it has been assumed that the two types of freedom involved are one and the same. They do have a good deal in common, it is true, but they are not the same, and particularly when we are considering the way business proprietorship and the wage system provide for our economy's functional foremen, it is urgent to distinguish them.

Let us briefly recall the main items that have so far attained a recognized place in our bill of personal rights and liberties. These include: the right to vote, eligibility to hold public office; the right to a fair

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[13] J. R. Commons, *Institutional Economics* (New York, 1934), p. 64.

trial; and (though we are less proud of this one) the right to discriminate arbitrarily in choosing one's friends. They include also: freedom of one's person and freedom to move about at will; freedom of one's property from search, seizure and the infliction of damages and (in the case of real estate) from unauthorized entry; freedom of assembly and association; freedom of calling and, for each household, freedom in the management of its budget; freedom of thought and its expression (including religious freedom); freedom from retroactive legislation. Of course these freedoms are not absolute; they are subject to various limitations. Thus freedom of one's person does not exclude imprisonment as a penalty for a crime.

Some of these items in our bill of rights are necessary to our private enterprise system. Freedom of consumer's choice and freedom of calling are essential features of the institutional framework we depend on for organizing economic activity. Further, workers must have freedom to move about, and in some degree freedom of their persons and freedom of thought to make our economic system operate as it does today.

The overlap of the legal foundations of our economic organization and those of personal freedom consists partly of these common bill-of-rights items. But there is another part of the overlap too. This consists of a number of rights and freedoms for the business enterprise itself that are included in the legal foundations of our economic system and that are more or less precise counterparts of the items in the bill of personal rights. Thus if our economy is to operate as it does today, a business corporation, likewise an unincorporated enterprise, must have the right to a fair trial and must in some sense have freedom of its property from seizure, search and the infliction of damages and (in the case of real estate) freedom from unauthorized entry. Also a business enterprise must have substantial freedom from retroactive legislation and something like a counterpart of freedom of calling and household budgetary freedom—a substantial degree of freedom of contract and freedom in making up its budget. But freedom of contract should not mean the right to discriminate arbitrarily. Further no business enterprise *qua* business enterprise needs the right to vote or to hold office, or freedom of its person, or freedom of its thought, so long as owner-operators and employees in working for the enterprise have somewhat of both. And, except for a small special class of enterprises that need freedom to print, broadcast or exhibit, no business enterprise as such needs freedom of expression.

There is a substantial overlap between our bill of personal rights and liberties and our free enterprise system. Nonetheless the two are separate and distinct. The reason for distinguishing them is that there have been serious problems in reconciling the two. Historically such prob-

lems arose in the process of developing legal rights that would put a creditor in as strong a position as possible to enforce a debt contract without jeopardizing the personal freedom of the debtor. The importance of reasonable protection for the creditor in our present economy is obvious. But obviously too it has been necessary to rule out the possibility of enslavement or imprisonment for debt as a means of debt collection.

Serious problems of reconciling the institutional arrangements for economic organization with those that give personal freedom have arisen too in another connection. When employees bargain with employers as individuals the law has the effect of clothing the employers with autocratic quasigovernmental powers—the powers of prescribing and administering shop rules and imposing penalties. Partly in order to get rid of the restrictions on personal freedom that such autocratic powers entail and to give employees a voice in shop rules and provide something like due process in their administration there has been a decided shift of public policy toward favoring collective bargaining. But no doubt this shift is also to an important extent, like most other phases of labor legislation, a recognition that competition cannot be relied upon to perform the regulatory function in the labor market.

I do not wish to minimize the problems connected with employer-employee bargaining. But for brevity I will pass them over with a single comment. So far as the interests of employees are concerned the case for substituting collective for individual bargaining is a strong one; but the substitution may leave the interests of an important group of third parties, viz. the consumers, quite inadequately protected.

For brevity too I will pass over the question, How fully have we eliminated the restrictions on personal freedom that were introduced into our social order in the first place through the development of the employer-employee relationship? My present concern is not with what has been accomplished here. Rather it is with the general approach we have taken to deal with the conflict between our bill of rights and the rights of business management which are part of the legal foundation of our free-enterprise system. The approach has been to resolve that conflict by a significant curtailment of the rights of management, a significant restriction of the right to hire and fire. And the objective has been to promote personal freedom. Even if the accomplishment falls somewhat short of the intent, I think we should regard what has been done as illustrating a broad basic general policy proposition, viz.: When a conflict arises between our free enterprise system and the objective of developing and strengthening our bill of personal rights and liberties, it is the free enterprise system that should yield.

The reason for this is clear. The rights and freedoms of business enterprises that are part of the legal foundation of our economic organization and that we have called counterparts because of their resemblance to items in our bill of personal rights, differ sharply in one respect from the bill-of-rights items. And that difference is a fundamental one. We prize the items in our bill of rights for their own sakes. Our personal freedom is an end in itself. But we prize the business-counterpart rights and freedoms—what we call freedom of enterprise—because they are part of the legal foundations of our economic organization. They are means to an end, because the organization itself is a means to an end—the end of getting the things done that we as individual human beings want done.

We rightly prize our economic organization because it has proven to be extraordinarily productive and progressive and has enabled us to achieve an extraordinarily high level of living. But we do not prize it as an end in itself. We should therefore be open-minded about possible further restrictions of business freedoms and other changes in it—both changes that will supplement and improve the way economic institutions perform central-management functions and enhance our economy's productiveness and progressiveness and changes that will enlarge and strengthen our personal freedoms. Twenty-five and more years ago economists were devoting a good deal of attention to the kind of welfare economics that is concerned with such changes in our free enterprise system. It is high time to resume the active investigation of this kind of welfare economics.

CAUSE AND CONSEQUENCE OF CHANGES IN RETAILERS' BUYING

By RUTH P. MACK AND VICTOR ZARNOWITZ*

Business fluctuation is a contagious process that spreads from one corner and aspect of the economy to another. One of the many directions in which expansion or contraction travels is a vertical one: buyers transmit fluctuation to sellers. It is important to understand not only what factors contribute to this vertical transmission but how rapidly it takes place. Must it wait on the sequence of actual production processes, or does it travel by swifter channels?

It is the contention of this paper that orders provide such a faster mechanism, and that transmission can start with the very first business agent serving consumers' needs—the retailer. Orders placed by retailers tend to recede from peak levels and rise from trough levels before consumer buying does, and to fluctuate more widely. The immediate transmission of retailers' buying to earlier stages of processing can explain, though it is by no means the only way to explain, why consumer buying, income, and production move up and down, as they often do, virtually at the same time. The analysis on which this assertion is based incorporates precedents in acceleration theory. Its more particular form has evolved primarily from an effort to explain, in the light of business problems and practices, the observed relationships between sales to consumers, production and prices in the shoe, leather, hide sequence.[1]

The argument has two parts as it proceeds from retailers' to manufacturers' buying. Basic operating problems and business arrangements explain why retailers' orders placed with their suppliers often reach peaks or troughs before, and rise higher and fall lower than, sales to consumers. The first section sets forth the observations and logic on which the assertion is based. The second examines evidence, derived chiefly from merchandising data for department stores, which bears upon the realism of the conception.

[1] The analysis of retailers' buying was developed and explained in R. P. Mack, *Consumption and Business Fluctuations: A Case Study of the Shoe, Leather, Hide Sequence,* National Bureau of Economic Research (New York, 1956). This volume is subsequently referred to as Mack, *op. cit.*

The third section considers whether the lead and greater amplitude in retailers' orders, relative to sales, is rapidly communicated to earlier stages of distribution and production. We contend that transmission can be virtually immediate and doubtless often is. Here again the logic of the position and the all too scant evidence are examined. In the concluding section, some of the implications of the analysis are discussed.

I. *A Theoretical Sketch of Retailers' Buying*

The notion that retailers' orders often reach turns ahead of sales to consumers and have a wider amplitude of fluctuation rests on five characteristics of retail store operation, of which four will be discussed presently and the last one a little later. We speak of stores other than food stores or those selling services or goods made to customers' order. First, since consumers are not ordinarily willing to order and wait, retailers must have their wares in the store before they can sell them; consequently substantial quantities of the goods which retailers sell must have been ordered from suppliers several months earlier. Second, because retailers sell a wide variety of individual items, stocks must be carefully planned and every effort made to make them conform to the desired size and composition. Third, goods must be purchased at advantageous prices and without undue risk that they may arrive too late to be of maximum use; since delivery periods and expected prices change, this involves alternately extending and contracting ownership positions. Fourth, neither extension nor contraction of the ownership position—goods on hand and on order—can be permitted to go too far; there are limiting zones beyond which it is thought dangerous to anticipate needs or to wait upon deliveries.

The first two requirements make it necessary to guess the successive seasons' needs, to purchase at least part of them well ahead of time, and to correct promptly for unanticipated developments. Requirements three and four imply that retailers must give careful consideration to advantageous timing of buying, taking into account the prices and delivery conditions which they foresee. These two sets of problems will be considered separately, the first as *buying under stable market prospects* and the second as buying linked to *changing market prospects*.

Buying under Stable Market Prospects[2]

Disregard for awhile the influence upon retailers' purchasing of

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[2] Many of the essential aspects of this problem have been explored by J. M. Clark ("Business Acceleration and the Law of Demand," *Jour. Pol. Econ.*, March 1917, XXV, 217-35), Lloyd Metzler ("Nature and Stability of Inventory Cycles," *Rev. Econ. Stat.*, Aug. 1941, XXIII, 113-29), and others. Conclusions of the shoe and leather study are given, Mack, *op. cit.*, Ch. 7, 8, 9, and summarized in Ch. 10; the relationship between other acceleration models and the one presented here is discussed on pp. 133-34.

observed or anticipated changes in conditions in the markets in which they buy. What factors then determine their buying?

In retail stores, the highly particularized character of the wares makes it essential that stocks be very closely controlled. Since a substantial part of buying must always be done before customers' orders are known—for most articles, perhaps three months before—purchases that anticipate requirements for the coming season must be based on a guess. Assume that the central tendency of the guesses of many retailers is that the level of past sales (implicitly corrected for seasonal characteristics) will continue, or occasionally perhaps follow a slight upward or downward trend. Recognition of the trend often takes the form of assuming that the percentage amount by which sales in recent months exceeded or fell short of those in the same month of the previous year will continue to apply. But how much further than this retailers typically go is questionable. In any event, past sales doubtless form the core of the forecast, and buying predicated on expected changes, particularly upward change, probably has a conservative bias.

If past sales form the core of the forecast, the error in forward procurement has the pattern, with signs reversed, of the rate of change in sales (the amount by which sales have changed between the date when the forecast is made and the date to which it looks).[3] The error is reflected in stocks which are larger than planned in proportion to the amount that sales have fallen, and smaller than planned in proportion to the amount that sales have risen. Just what the proportion will be depends on the particular character of the stock objective.[4] Of particular importance in this context is that sales-stock ratio which must be validated within the season by adjustments of fill-in or make-up orders. It seems unlikely, if market prospects are deemed invariant, that the implemented ratio will be as large as the average sales-stock ratio. It is more likely that this validated incremental ratio will be greater than one but smaller than the average.

But if stocks must always be held sharply to plans, the error in procurement must be corrected. This can be accomplished by enticing consumer buying by a decrease in selling prices or by repelling it with an increase. It may also be accomplished by changing the amount of

[3] At times when trends are so well established as to be incorporated in forecasts and therefore in advance buying, they cause differences, in effect, to be calculated from a slanted line rather than a horizontal one. When, as is usual, comparison is made with the same month of the previous year, the special characteristics of last year's sales also contribute to the error. See Mack, *op. cit.*, p. 106.

[4] If stocks are meant to be held to a uniform figure, the incremental ratio is 0; if increments or decrements in stocks are meant to be once or twice those of sales, the incremental ratio is 1 or 2. If stocks are meant to be held at some constant ratio to total sales, the incremental ratio equals the average ratio.

new buying currently undertaken. We assume that a substantial portion of the corrections will be effected in the second of the two ways— by the corrective order which may be positive or negative; this is a fifth characteristic of retail store operation on which the argument rests. Under these assumptions, corrective buying approximately reproduces the pattern of the rate of change in sales of the recent past.

Significantly, the rate of change in sales (monthly first differences) has shown a strong tendency to lead sales proper. Statistics for department stores, 1919 to the present time, provide evidence. Leads of the rate of change over sales proper occurred at 30 turns, lags at 2 and coincidences at 6. The average timing for all comparisons was a lead of 3.4 months.[5] Under the conditions outlined, this leading pattern will be reflected in corrective buying.

If the argument and assumptions about retailers' practice are sound, total stable-market buying, the sum of advanced buying (assumed to reflect current sales proper) and corrective buying (assumed to reflect recent first differences in sales) will have a tendency to lead sales proper. The turns in total stable-market buying will come between those of sales proper and of the rate of change in sales. Consequently, if sales do not retard before they turn, stable-market orders cannot lead sales. But even if retardation does anticipate the reversal in sales, orders may not lead sales. In order to judge whether the lead will be present, the shape of the sales curve and the weight of corrective buying in the total must be considered.[6]

In addition to the tendency toward a lead, the analysis implies an amplitude of fluctuation in buying which is somewhat larger than that of retail sales. Sales rise at an increasing rate during a considerable portion of their upward swing, and fall at an increasing rate during

[5] The Federal Reserve Board sample of all department stores was used 1919-1941, and the merchandising sample thereafter (see note 9 below). A 5-month moving average of monthly first differences was compared with sales proper. There are technical problems in measuring leads and lags on the basis of smoothed data, but they are not controlling. (See Mack, *op. cit.*, pp. 54-59.)

The time when first differences in sales reached a subcyclical peak or trough was compared with the corresponding turn in sales proper. The average lead at all matched turns for the first period was 3.7 months and 2.6 for the second period, with average deviations of 2.8 and 2.5 months respectively. The timing at turns in sales that took place in conjunction with cyclical turns in business at large, of which there were 16 for the total period covered, had an average lead of 5.7 months, whereas at other turns the lead averaged 1.7 months (the average deviations were 3.3 and 1.4 months respectively).

[6] When the rise or fall in sales is large and retardation slight, the relative weight of corrective buying necessary to cause total stable-market orders to reverse ahead of sales is greater than when the rise or fall in sales is small and retardation strong. The weight of corrective buying in total buying is, according to our formulations, a function of the amount of buying done for advance delivery on specified terms and the size of the desired incremental sales-stock ratio. See note 14.

much of their decline. Consequently, the extra emphasis that corrective buying places on rates of change will tend to add slightly to the amplitude of buying. In so far as the stock objective is to increase stocks when sales increase, this tendency is heightened.

Market-Prospect-tied Buying

If conditions are expected to change in the markets in which retailers purchase, it is often desirable to buy more or to buy less than would be justified on the basis of the considerations previously sketched. At some times retailers certainly do anticipate changes in the wholesale markets. Delivery periods may be expected to lengthen, prices may be expected to rise, and selections to become impoverished when cyclical or seasonal expansion is under way. At other times the anticipated changes are of the opposite sort. Under the first set of circumstances buyers purchase an increasing proportion of the expected seasons' requirements at the time when advance orders are placed, thus extending the average term of the order. The reaction to the second set of conditions is the opposite—they decrease the amount of goods bought well in advance of the required delivery date and increase the proportion bought for immediate or short-term delivery. Also, orders may be greater or smaller than those justified by stable-market prospects, not strictly because of expected change but because actual changes in delivery periods or in selections had recently taken place and were expected to be maintained, and these had not yet been translated into appropriate action.

The component of buying, then, that we call market-prospect-tied buying focuses on the proper short-term timing of buying; it is a response to recently observed or to expected change in the markets in which retailers buy. It can be positive, as it is when retailers provide for sales over an increasing span of months; such provision involves some extension of stock on hand and on order, the ownership position. It can be negative and cause the ownership position to contract.

It seems likely that for goods whose supply is not subject to abrupt and extensive change, market prospects that favor an extension of the ownership position occur only when sales to consumers are rising; conversely, contractions occur only when they fall. This presumption is supported by the fact that movements of sensitive prices, or in the rate of change in prices of semimanufactured goods, tend broadly to parallel sales and changes in sales. If parallel changes in sales are a necessary (though not a sufficient) condition of shifts in market prospects, the component of buying tied to market prospects must add to the amplitude of fluctuation in retailers' buying compared to that of their selling.

Regard for market prospects may also often tend to set turns ahead.

There are many reasons why buying associated with bullish expecta-
tions is likely to decline before sales start to fall, perhaps more nearly
when increases in sales and prices start to diminish. A similar state-
ment applies to bearish expectations. The problem is a complicated one
and cannot be discussed here.[7] Two points, however, should be borne in
mind. First, it seems likely that market tension resulting from a heavy
flow of orders for immediate delivery, such as positive corrective orders
imply, would be readily interpreted as auguring a rise in prices or some
difficulty in getting the more popular merchandise at short notice.
Negative corrective buying, identified by the absence of usual fill-in
orders, may foster the opposite interpretation. If so, buying motivated
by changing market prospects would have some tendency to reproduce
the pattern of corrective orders, including these early turns. The second
point involves the amount of buying, positive or negative, associated
with changing market prospects. This amount is reflected in the rate at
which ownership position is being extended or contracted; it declines
when the rate slows, not when extension or contraction ceases. Yet not
only can a given rate not be increased indefinitely, but extension or
contraction itself can proceed only within limits. For each commodity,
business experience has shown that there is a stipulated number of
weeks' supply on hand and on order beyond which it is dangerous to
extend procurement; movement toward the "hand-to-mouth" position
likewise has its recognized limits. Thus the ownership position moves
up and down within limiting bands beyond which risk increases rapidly.
These reflections suggest that buying associated with changing owner-
ship position would often reach peaks and troughs before sales to con-
sumers; it would have a wider amplitude of fluctuation.

II. *Evidence Bearing on the Realism of the Theory*

Abstracting from seasonal variations, buying is a function, and not
necessarily a simple one, of current selling and of the rate of change in
selling. It is a function also of the current market position relative to
the accepted limits thought necessary and proper, of delivery condi-
tions, and of expectations about them and about prices.[8] All these in-
fluences would presumably cause buying broadly to parallel sales but
also to reach peaks and troughs before sales to consumers and to have
a larger range of short-term fluctuation.

The basic picture that has been outlined was developed in the course

[7] Paragraphs numbered 4 to 8, pp. 243-46, Mack, *op. cit.* summarize what the study
of the shoe, leather hide sequence seemed to suggest concerning why a lead is likely to
be present.

[8] Since actual change in delivery conditions and prices depends in part on the amount
of market extension or contraction that is taking place, and expectations probably depend
in part on current rates of change, there doubtless is circularity in the causal spiral.

of a study of the shoe and leather sequence. It was based on an effort
to explain the relationships among branches of the industry in the light
of the business objectives and methods revealed by conversations with
retailers, wholesalers and manufacturers. In this sense, the theory it-
self, like most theories that aim to explain the facts of existence, is
generated partly by empirical observation. Here we aim to confront
it with the facts of a wider world than the one in which it was cradled.

A stepwise procedure is chosen. First, statistics on orders and sales
are examined to see whether some of the more obvious characteristics
of the model are manifested. Other merchandising statistics are re-
viewed for the same reason. Second, the evidence is examined to see
whether the more distinctive features of the explanation are supported
—the accelerator that relies mainly on corrections, and a component of
buying which is linked to changing market prospects.

Timing and Amplitude of Sales and Orders

Information on new orders placed by retail stores is provided by de-
partment stores that have reported to the Federal Reserve Board since
1940.[9] The sales of these stores are compared with their orders in
Chart 1. To facilitate study of the time series, their usual seasonal pat-
terns have been removed. Movements, selected independently in each
series, have any of the following characteristics: a sequence of rising
followed by falling months, a flat area preceded and followed by sharp
rises or falls, distinct differences in level between one group of months
and a previous and subsequent group of months. Peaks and troughs in
each of these "subcycles" are shown by circles on the chart (by the
vertical grid for orders). For expansions, they aim to indicate the high
month in the high area, though the highest month may be passed by if
its significance seems countermanded by a neighboring extreme low
month. For contractions the analogous statement applies.[10]

Orders, the chart shows, have two subcycles that were not marked
in sales; their contraction phases are in 1943 and in 1947-48. All other
movements are common to both series, so that taking each month in
sequence, both orders and sales are in a like subcycle phase (rather
than the one in an expanding and the other a contracting phase) for all
but 21 per cent of the months from January 1941 to December 1954.

[9] The figures are supplied by about 325 independent department stores holding about
half of all department store stocks. The stores report monthly on their sales, stocks, and
outstanding orders; their receipts are compiled by the Board by adding sales and change
in stocks; orders are computed by adding receipts and change in outstanding orders. The
Univac method of moving seasonal correction is applied.

[10] For further discussion of the concept and procedure for identifying subcycles see
R. P. Mack, "Notes on Subcycles in Theory and Practice," *Am. Econ. Rev.*, May 1957,
XLVII, 162-63 and Mack, *op. cit.*, p. 34.

CHART 1

SALES AND NEW ORDERS OF DEPARTMENT STORES, 1940-1955 .
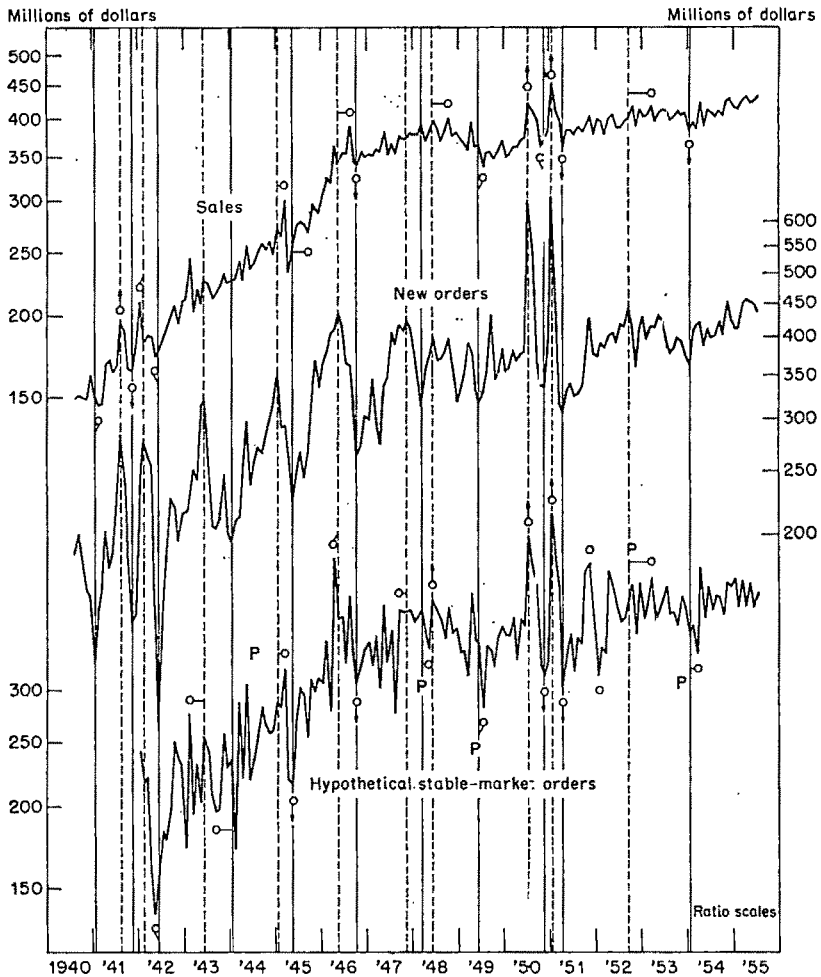
Millions of dollars   Millions of dollars

Broken and solid vertical lines represent, respectively, peaks and troughs of subcycles ir new orders.

For the other series, subcycle turns are marked by o. When a turn is matched with a turn in new orders, a horizontal line or a vertical arrow indicates the association.

The letter P shows selected turns in the monthly change in the index of spot prices of industrial commodities (see text).

This statistic is shown in the first line of Table I, column 9. As data of this sort go, the finding that only one fifth of the months are in unlike phase bespeaks a high degree of parallelism.

As to the timing of the turns in the two series, the chart suggests that the majority of reversals in subcyclical fluctuations occur in the same months or within one month of one another. On five occasions, a longer

Line	Series	Number of Subcyclical Turns					Timing (months)		Percentage of Months in Unlike Subcyclical Phase			
		All Turns	Matched Turns				Mean Lead (−) or Lag (+)	Average Deviation	Jan. 1941–Dec. 1954 (167 mos.)		July 1947–Dec. 1954 (89 mos.)	
			Total	Leads	Lags	Coincidences			Timing Adjustment[a]	Per Cent	Timing Adjustment[a]	Per Cent
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
	1. Reference Frame: Department Store Sales (17 Turns)											
1	New Orders	21	17	7	3	7	−1.1	1.6	0	21	0	18
2	Receipts	19	17	3	9	5	+0.9	2.1	+1	26	+1	22
3	Stocks	16	6	0	6	0	+2.3	1.3	+3	26	+3	17
4	Outstanding Orders	19	17	6	9	2	+0.4	2.4	+1	29	+1	24
5	Ownership Position	18	16	2	12	2	+1.0	2.1	+1	29	+1	26
	Month-to-Month Change in											
6	Sales, 5-month average	18	14	9	1	4	−2.6	2.5	−2	28	−2	34
7	Stocks	19	11	3	7	1	+0.5	3.3	+1	40	+1	38
8	Outstanding Orders	24	16	9	2	5	−2.2	2.8	−1	36	−1	39
9	Ownership Position	22	16	7	5	4	−1.2	2.5	0	32	0	35
	2. Reference Frame: New Orders Placed by Department Stores (21 Turns)											
10	Receipts	19	19	0	16	3	+2.3	1.8	+1[b]	20	+1[b]	19
	3. Reference Frame: 5-Month Centered Average of Month-to-Month Change in Department Store Sales (18 Turns)											
11	Stocks	19	15	0	14	1	+3.7	2.1	+2	32	+2	37
12	Outstanding Orders	24	20	13	1	6	−1.6	1.7	−1	22	−1	30
13	Ownership Position	22	20	5	3	12	−0.2	1.5	0	19	0	33
14	New Orders	21	16	3	8	5	+1.6	2.1	0	25	0	20
	Month-to-Month Change in											
15	Stocks	19	11	3	6	2	+1.1	3.7	+3	40	+3	30
16	Outstanding Orders	24	14	9	3	2	−1.1	1.6	−1	32	−1	33
17	Ownership Position	22	14	5	5	4	+0.1	1.8	0	28	0	28

[a] Number of months by which the reference frame is shifted forward (+) or backward (−) in order to minimize the percentage of months in unlike phase. When equal results are obtained for different timing adjustments, the adjustment shown is that which seems to characterize best the given relation for the total period covered.

[b] Allowing for a two-month lag, the figures in cols. 9 and 11 become 21 and 22, respectively.

difference is apparent. In each case, it was orders which led sales. It is interesting also that these longer anticipations occurred chiefly at times when downturns were somewhat more generally diffused in the economy at large than at the other times when specific subcycle peaks occurred in department store sales and orders.

Unfortunately, there are no data on orders of department stores for earlier years. The only comparisons that can be made for periods before the second world war are for furniture, textile and apparel sales and orders; the figures cover sales of retail stores or departments and new orders received by manufacturers. They refer to all or part of the period 1927-1941.[11] Of the 66 subcyclical timing comparisons, 46 are leads (34 of over one month), 12 are synchronous, and 8 are lags (3 over one month). Here too the leads averaged longer at times of more general business fluctuations than at the minor subcyclical turns. These fragmentary figures, then, confirm the tendency for orders placed by retailers to be closely associated with their sales and usually to turn shortly before consumer buying.

Another glance at the chart reveals a third characteristic of the relation between orders and sales: orders rise more and fall more in each small wave than do sales. A summary measure can be obtained for total subcyclical amplitude by adding the fall from peak to trough (measured by the difference in a three-month average, centered at the peak and trough months) to the rise from trough to peak, cumulatively phase by phase; the sum is then divided by the number of months covered by the data. For sales, average amplitude (expressed as a per cent of the average level of sales during the cycle) is 1.4 per cent per month. The corresponding figure for orders is 5.0 per cent.

Change in Ownership Position and Stocks

The three characteristics of retailers' orders relative to their sales—general parallelism, frequent leads, greater amplitude—imply a pattern of change in ownership position of retail stores. When orders are larger than sales in a given month, stock on hand and on order must have increased by the amount of the difference, and decreased correspond-

[11] The figures refer to orders received by manufacturers. But since only a small part could have been placed by wholesalers, the statistics may be read as applying to retailers' ordering. The orders series are: furniture, Grand Rapids district, 1927-40 (Seidman & Seidman); furniture and floor coverings, 1929-40, and textiles and apparel, 1929-41 (both compiled by the Econometric Institute, Inc.); we are indebted to the Institute for graciously supplying us with these data. The sales series are: furniture departments of department stores, 6 districts, 1926-40 (NBER); furniture and house furnishings stores, 1935-41 (Commerce); apparel stores, 1935-40 (Commerce); and total department store sales, 1919-41 (FRB) within which the textile and apparel transactions are the largest component.

ingly when orders are smaller than sales.[12] Monthly changes in owner-
ship position, that is monthly investment in stock on hand and on
order, is the third line of Chart 2. Short waves that conform with
those of orders, for which peaks and troughs are indicated by the verti-
cal grid, appear clear and strong. Even during peacetime, monthly
change in the neighborhood of peaks or troughs often amounted to plus
or minus $30 million—about 7 per cent of average sales—and of course
the world war and Korean war occasioned huge changes amounting to
a third or more of current sales.

The waves in ownership position reach peaks or troughs at about the
same time as new orders; 12 of the 20 matched turns were actually
synchronous and the average deviation and per cent in unlike phase
low (see Chart 2 and Table I, line 13). In a sense this is simply a piece
of arithmetic—change in ownership position is the difference between
a series which fluctuates more, orders, and one that fluctuates less,
sales. But in another sense the economics of the association is interest-
ing. Each of the two segments of change in ownership position—change
in stocks on hand and change in outstanding orders—underwent quite
clear subcyclical fluctuation of a sort tending to augment the effect of
the fluctuation in consumer buying as this effect was transmitted toward
the manufacturer of finished goods. The two series may be seen in the
second and fourth lines of Chart 2. Outstanding orders typically
reached their maximum or minimum rate of increase or decrease several
months earlier than stocks. Indeed, change in outstanding orders led
new orders (Table I, line 12), whereas change in stocks lagged not only
orders but also often sales (lines 11 and 7). The very loose relation
between change in sales and change in stocks (line 15) indicates that
sales-linked stock objectives are not achieved in the first instance.

The lag of change in stocks relative to sales implies that receipts
of merchandise by department stores (sales plus change in stocks)
might have a similar tendency to lag behind sales and this appears to
be the case (line 2). A lag relative to orders (line 10) of one or two
months is very common, and this makes sense, too, in terms of the
need to order ahead on the basis of a guess about requirements which
we assumed would apply to a substantial segment of the goods pur-
chased.

The very early turns in the rate of change in outstanding orders
suggests early turns in market-prospect-tied buying, since it seems
likely that market prospects would be one of the more important in-

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[12] An example may clarify the statement. Say, in a given month, orders are 100, receipts
of goods 80, and sales 90. Then stock on order has increased 20 (= 100 — 80); stock on
hand has decreased 10 (= 80 — 90); and stock on hand and on order, the ownership posi-
tion, has increased 10 (= 100 — 80 + 80 — 90).

CHART 2

MONTHLY CHANGE IN STOCKS, IN OUTSTANDING ORDERS,
AND IN SENSITIVE PRICES, 1940-1955
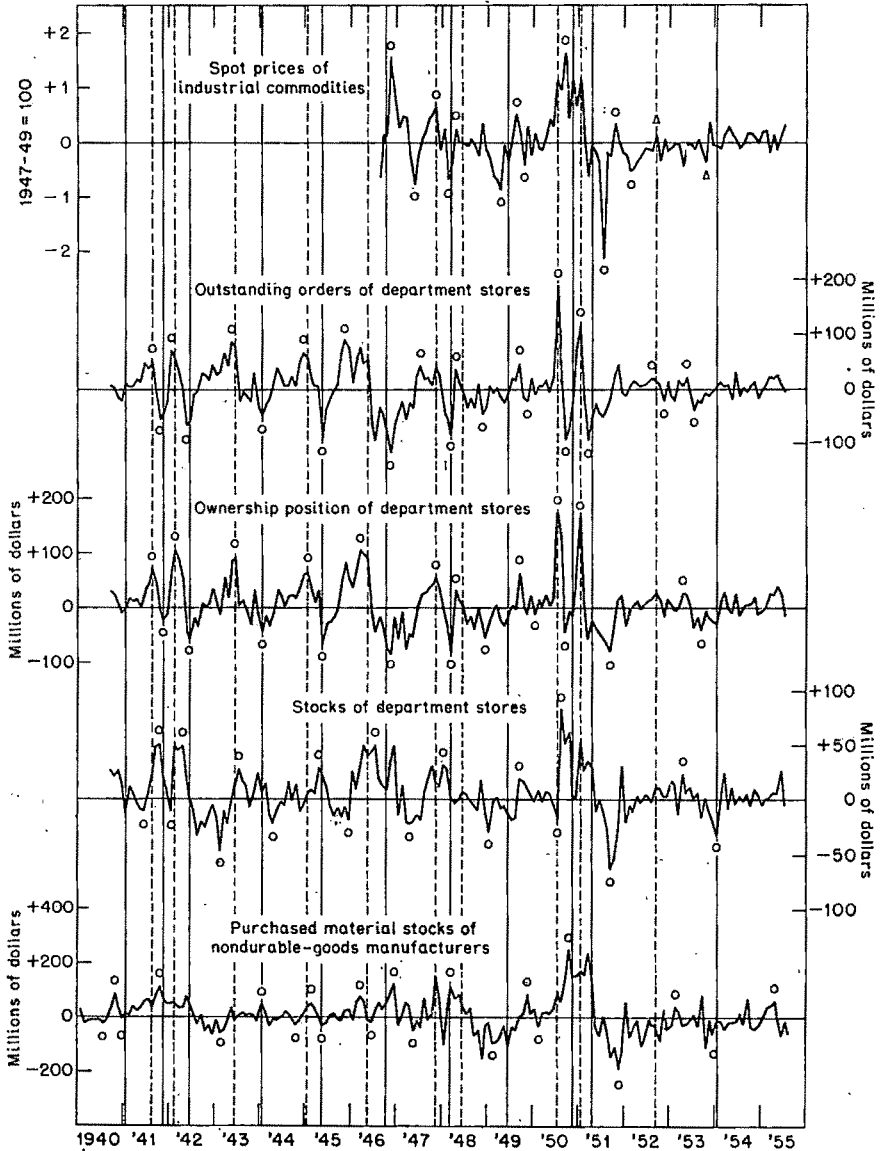
Broken and solid-vertical lines represent, respectively, peaks and troughs of subcycles in new orders.
Subcycle turns in all series are marked by o, retardations by Δ.

fluences causing changes in the volume of goods on order. When wholesale markets are expected to tighten, a larger proportion of the expected requirements for the season are likely to be purchased at the time of the preseason showing, and this would tend to increase the volume of outstanding orders. A tendency for more firms to extend buying of more articles would augment the rate of increase. A parallel statement applies to declines. An index of spot prices of industrial commodities may perhaps reflect, however imperfectly, expectations about market conditions; and comparisons in Chart 2 of changes in this index with changes in outstanding orders or ownership position reveal elements of similarity.[13]

The explanation that has been offered for the relationship between sales and orders also suggests that ownership position could be brought more closely in accord with intentions than would be the case for stocks (which are subject to the inverse impact of unexpected change in retailers' sales and the eccentricities of suppliers' production and delivery schedules) or would be the case for unfilled orders (which are prey to the latter vagary). Retailers can take all current information into consideration at the time they place their new orders: the volume of current sales, unexpected recent changes in sales, the size of stocks on hand, the volume of goods now on order, and the relationship of these to accepted standards given the state of the buying markets. Of course the inevitable departures from the desired relationships in the past will have some continuing impact on the difference between current buying and current selling—change in ownership position. Nevertheless much of the undesired fall (rise) in stocks will be reflected in a rise (fall) in outstanding orders and thus be zero in the sum of the two. It therefore conforms with expectations to find average deviations and the per cent of months in unlike phase in Table I consistently less for change in ownership position than for the two components, whether retail sales, new orders, or change in retail sales provide the frame of reference. Another fact, presently to be discussed, that falls in with these ideas is that the relationship of the

[13] The price index used is the Bureau of Labor Statistics daily index of spot prices of 13 raw industrial commodities. These are prices judged to be "particularly sensitive to . . . trade's estimates of current and future economic forces and conditions." (*Techniques of Preparing Major BLS Statistical Series,* Bull. 1161, Bureau of Labor Statistics, p. 93). First differences in prices and in ownership position are compared. Though parallelism in the immediate post-Korean episode is disturbed by price ceilings, 28 per cent of the months July 1947 through December 1954 are in unlike phase. Prices lead at four turns, lag at one and synchronize at two; the average lead is 0.3 months with an average deviation of only 1.4 months. Such association as there is must reflect causality that runs from buying to prices as well as from prices (via expectations) to buying. But in so far as it runs from buying to prices, it must mean that buying of many more enterprises than department stores alone move in the same rhythms at about the same time—a fragment of evidence bearing on a point made below, p. 41.

ownership position proper to sales behaves more sensibly than that of stocks proper to sales.

Hypothetical Stable-Market Orders

The pattern of orders and the associated change in retailers' stocks and commitments seem to exhibit the expected behavior. It could have been caused in many ways. Of the possible explanations we have chosen one that ascribes the pattern of orders to two types of procurement problems, those associated with the physical requirements of servicing sales and acquiring goods, and those focused on conditions in the buying markets. If the explanation is correct, it has specific implications about how changes in retailers' buying move backward from later to earlier production processes. Consequently the finding of the previous section that statistics of retailers' buying *could* have been explained in the way we suggest is not enough since the facts could also have been explained in other ways which would have different implications concerning the backward transmission of demand. It is essential to seek evidence that supports or contradicts the particular explanation that has been offered.

The assumptions concerning the first set of considerations can be made sufficiently explicit to construct a time series in which sales and changes in sales are combined in the manner appropriate to represent, hypothetically, retailers' buying under stable market conditions.[14] We

[14] The steps are taken in a manner suggested by an analysis of shoe buying by retailers and by discussions with executives of retail stores. For reasons given in Mack, *op. cit.,* Ch. 8, the following assumptions are made: Total stable-market orders are orders for advance delivery plus corrective orders. Orders for advance delivery are forecast sales for three months hence; the forecast is made by, in effect, assuming that the current level or trends in sales will continue. Corrective orders are the difference between the forecast of three months ago and current sales as they actually materialize. Corrective orders, which may have either a plus or minus sign, are added to advance orders twice— once to correct stock to a given constant size and once to allow for a stock objective that aims to have stocks change by the same amount that sales change. Thus, corrective orders aim to enforce an incremental stock-sales ratio of 1 (judged to be more realistic than ratios of 0 or 2, used in the calculation for shoes).

These procedures mean that hypothetical stable-market orders are a function solely of sales of stipulated months of the past and of their rate of change. The formulas for advance orders used here embody a slight improvement over the ones used *loc. cit.* For corrective orders they are identical as given in Mack, *op. cit.,* pp. 106, 108. To illustrate, the third of total stable market orders based on the same month of the previous year (corrective element Ib, *op. cit.,* p. 106) consists of the following (S denoting department store sales in the subindexed month):

(1) Advance orders (placed in month 0 for months $+1$, $+2$, $+3$) equal to:

$$\frac{1}{3} \frac{S_0}{S_{-12}} (S_{-9} + S_{-10} + S_{-11})$$

(2) Corrective orders (placed in month 0 to correct for errors in the advance orders placed in months -1, -2, -3) equal to:

assume that the level of current sales or their ratio to the same month of the previous year is expected to continue, and that if stocks do not increase or decrease by the same absolute amount as sales the failure to do so is promptly corrected.

The construct is shown, along with sales and actual orders, at the bottom of Chart 1. It reproduces the essential characteristics of orders in contrast to those of sales: the amplitude of subcyclical movements is substantially greater than that of sales in both hypothetical and actual orders; indeed, after 1947, when the disruptions of the war had quieted, the amplitude of the two order series seems quite similar. Several movements not found in sales appear in both the construct and actual orders, and there is substantial similarity in the time when they occur; all of the subcyclical movements that have been marked in actual orders have corresponding movements in the hypothetical series. The hypothetical series is more jagged than the actual one and this is a reflection of the part that rates of change in sales play in its calculation; perhaps it would be more realistic to assume further implicit smoothing by virtue of the further averaging or discounting by retailers of past experience. Stable-market orders explain the timing of total orders, in that they turn in the same month or earlier at 12 of the 17 turns in orders. The Korean buying movement is strongly visible in the hypothetical series; apparently consumers contributed strongly to war-scare buying of retailers. The dip in buying prior to the relaxation of wartime controls in the fall of 1946 occurs in the hypothetical as well as in the actual series; as will be seen presently, this dip did not occur in orders received by manufacturers at earlier stages of production. In general, the evidence of Chart 1—the substantial similarities between actual orders and hypothetical stable-market orders and the systematic differences between the latter and sales—can be regarded as consistent with the model developed in the first section of this paper.

Evidence of Market-Prospect-linked Buying

There are 5 turns in actual orders that are unexplained by hypothetical stable-market orders in the sense that at these times the construct follows, rather than coincides with or leads, actual orders. In each case, the index of spot market prices of industrial commodities reached inflection points shortly before or at the time of the turns in orders. The letter P is marked on the chart close to each of these

$$2\left[S_0 - \frac{1}{3} S_{-12}\left(\frac{S_{-1}}{S_{-13}} + \frac{S_{-2}}{S_{-14}} + \frac{S_{-3}}{S_{-15}}\right)\right]$$

A trailing 3-months average of these formulas constitutes a second third of the total (corrective element IIb, *ibid.*, p. 108). The last third is based on the past three months (corrective element IIa, *ibid.*, p. 108).

turns with the stem of the P designating the month when the rate of change in sensitive prices turned.[15] This price series may give as good a picture as is easily available of changing expectations about market conditions, partly because expectations may be strongly influenced by these sensitive and well-advertised prices, and partly for a variety of other reasons whose discussion would lead us too far afield. There is a suggestion, therefore, that market prospects may have influenced the turn in buying on these five occasions at least.

But these are isolated events, and if prospect-linked buying were a common component of retailers' buying it should occur not merely at a few but at many times. A few pieces of evidence bearing on this point warrant review. The most interesting set of facts concern not buying but a result of buying—total ownership position. Ownership position rather than stock or outstanding orders is selected because, as indicated earlier, it is more subject to retailers' control. Its size is ordinarily judged in terms of the number of months' or weeks' supply that it constitutes, and therefore ownership position is divided by sales and shown as a ratio in Chart 3. The first line on the chart gives this ratio on the assumption that the only buying that took place was based on stable market prospects.[16] The downward slope of the ratio during the war results from the strong rise in sales in view of the assumption, underlying the estimation of stable-market orders, that the stock objective was a constant incremental ratio which was considerably smaller than the average ratio. When postwar levels are once attained, the ratio flattens and merely undergoes minor fluctuations that represent incomplete responses to short-term irregularities in sales.

Contrast this series with the actual ratio of ownership position to sales (second line on Chart 3). Apparently the high general level of ownership position was maintained throughout the war and the immediate postwar period; it reflects a three-fold increase in outstanding orders between the middle of 1942 and 1943, a trend which was not reversed until after price controls were eliminated. Fluctuations in the ratio in 1947 and the next two years are shared by sensitive prices as well as many other economic events. The ratio also shows the strong forward buying of the Korean war scare and the flat wave that followed it and terminated in the mild 1954 slump. These several movements

[15] The month-to-month change in the Bureau of Labor Statistics sensitive price index of 13 raw industrial commodities (the time series is shown on Chart 2, first line) was used, except that prior to 1946 a related BLS series of spot market prices of 28 commodities was used.

[16] The ratio was computed by replacing actual buying—orders as recorded—by hypothetical "stable-market buying." Change in the associated market position is this stable-market buying minus actual sales for the month. Change in ownership position thus computed is linked to actual total ownership position at the end of 1952. The ratio is this figure for the end of the month divided by sales during the month.

CHART 3

RATIO OF OWNERSHIP POSITION TO SALES AND OF STOCKS TO SALES
FOR DEPARTMENT STORES, 1940-1955
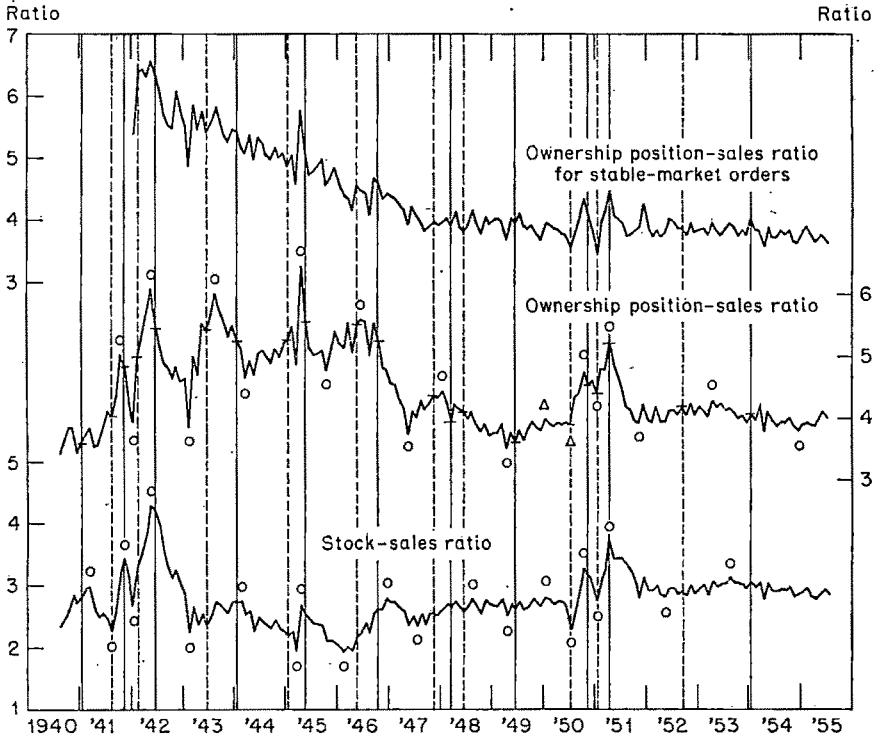
Broken and solid vertical lines represent, respectively, peaks and troughs of subcycles in new orders.
Subcycle turns in all series are marked by o, retardations by △.
The level of the ownership position-sales ratio at subcycle peaks and troughs in new orders are
marked by —.

all seem explicable in terms of an effort to time buying in line with expectations about market prospects.

The third line on the chart—the ratio of stocks on hand to sales— does not share the major changes in level during and after the war. Instead, the heavy drop in 1942 and the uneven rising trend after 1946 are hard to interpret in terms of retailers' intentions. There is hardly any response to the 1948-49 recession nor is the recovery in 1954 reflected in the ratio. In June 1950 the war scare depleted stocks but retailers made up for it promptly by building up outstanding orders, thereby keeping the ownership position fairly steady. In general the difference between the two ratios agrees with the notion that it is the total ownership position rather than either of its components, stock on hand and outstanding orders, that is the focus of management attention.

The shifts in the ownership-position ratio, occurring as they did at times when explanation in terms of changing market prospects appears reasonable, seem to constitute evidence that market prospects do influence retailers' buying. This supposition receives a little further support from another characteristic of the ratio. Except for shifts of the sort discussed, its level at the times when orders reach peaks or troughs is confined within narrow limits. The times when orders turn are noted by small horizontal lines on the ratio; whether they are peaks or troughs can be ascertained by reference to the vertical grid, in which peaks in new orders are solid and troughs dashed lines. During the war years, after the high level of ownership position had been achieved (June 1942 through December 1946), though the ratio fluctuated considerably, ordering started to pick up whenever the ratio fell below a level of between about 5.2 and 5.5 months supply, and ordering started to fall off whenever the ratio rose above the same narrow band. Thus we seem to see shifts in ordering with a view to maintaining an ownership position judged appropriate. During postwar years, turns in orders may also be interpreted as responses to a preconceived level for the ratio. From the middle of 1947 to the end of the period under review—the middle of 1955—orders reached subcyclical troughs or peaks when the ratio was at a level of between 3.9 and 4.2 months supply, except during the depression of 1949 and during the disruptions of the Korean buying scare when it seemed important to grab the available supply.

The previous paragraphs suggest that changes in the ownership-sales ratio can be reasonably interpreted as associated with shifts in market prospects. This line of thought can be pushed a little further by computing hypothetical market-prospect-linked buying simply by subtracting stable-market orders from total orders.[17] The new series consists of small differences between two large aggregates and therefore exaggerates all of the uncertainties of the estimates; nevertheless, it should be inspected. Smoothed by a five-month centered average it appears as the fourth line in Chart 4. This rather fancy construct seems to bear a genuine family resemblance to rates of change in the ownership position-sales ratio (line 1). Changes in unfilled orders is one of the more volatile determinants of the ratio; also it has a direct logical association with market-prospect-linked buying since an increase or decrease in the amount of merchandise bought for advance delivery should be the first manifestation of changing ideas about delivery periods or prices. Comparison between the two series (lines 2 and 4)

[17] Stable-market orders were computed as before (see note 14) except that corrective orders were multiplied by four instead of by two. Subtracted from actual orders, this gives the component hypothetically linked to changing market prospects.

CHART 4

HYPOTHETICAL MARKET PROSPECT-LINKED ORDERS COMPARED WITH SELECTED DATA,
FIVE-MONTH MOVING AVERAGES, 1942-1955
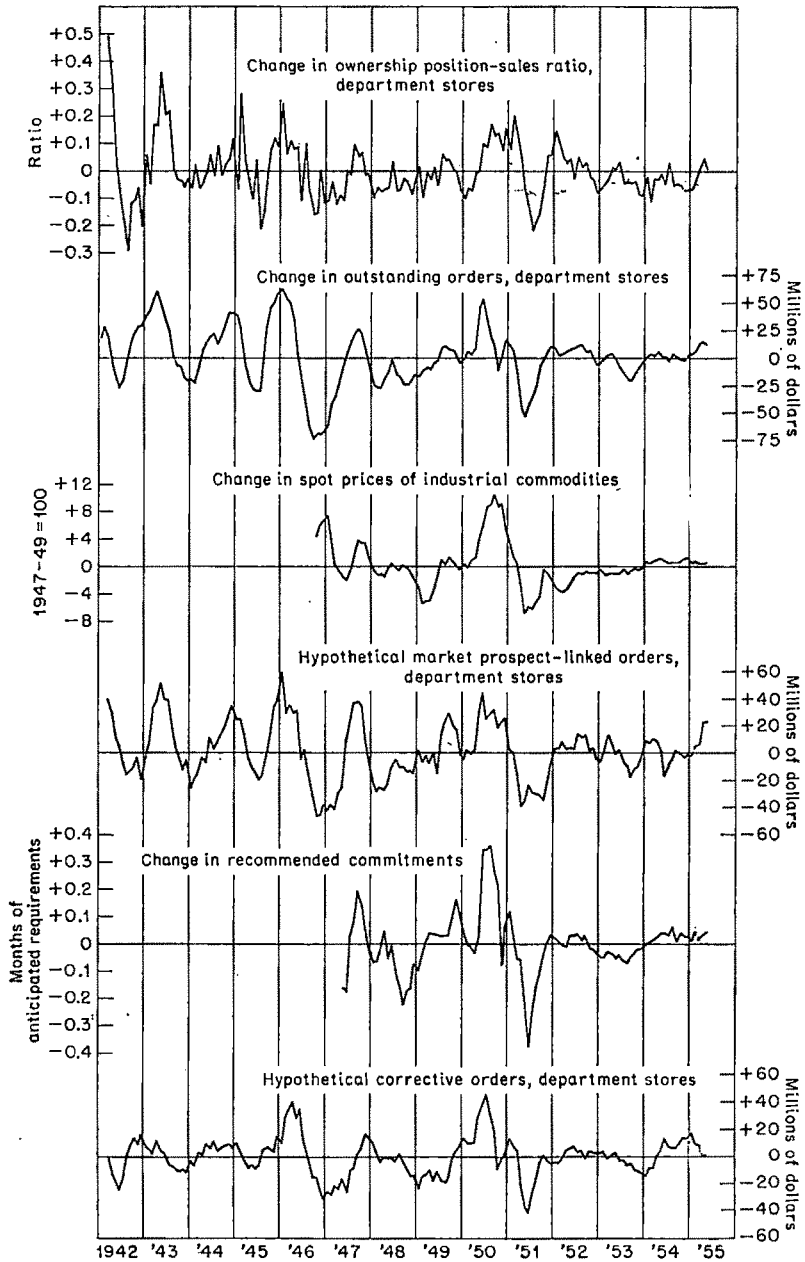
show substantial similarity. These similarities bolster but certainly do not establish the point at issue, namely that the pattern of retailers' buying cannot be explained without acknowledging shifts in the short-term timing of buying predicated on opinions about changing delivery periods, selections and suppliers' prices.

More conclusive empirical test is made difficult by the fact that the chief competitor in explaining buying—corrective orders which represent our version of the "acceleration principle"—ought to and does have marked similarity to market-prospect-linked buying, since a flock of make-up orders or the absence of the usual fill-in orders must influence judgments about market prospects (compare lines 4 and 6). A critical assumption incorporated in our hypothetical series is an implemented incremental monthly sales-stock ratio of one. The average ratio was a little under 3 for the last few years covered (it had been lower previously; see Chart 3). We have recalculated market-prospect-linked buying under the assumption that retailers try to enforce a constant average ratio (an incremental ratio of 3) month by month. This is doubtless an extreme assumption. Investigations made in the shoe and leather study suggest that an implemented ratio of 3 is unrealistically high; though an average ratio may be roughly sustained in fact, failure to maintain it month by month will not set off corrective buying of the sort here described. The new series still shows prospect-linked buying to be important, and at very much the same times as in the series in which computations were based on an enforced ratio of one. Apparently empirical manifestations of the market-prospect-linked component of buying can not be blown away by even an extreme assumption about the possible role of the corrective element in total buying.

Nevertheless, it is highly desirable to have evidence bearing directly on changing market judgments and their impact on buying. Certainly market-prospect-linked buying must affect and be affected by the sensitive prices that are watched by department store executives. Yet the association of our hypothetical series with the most appropriate price series readily available, spot prices of 13 sensitive industrial commodities (3rd line), is not impressive.

Another piece of direct evidence is a time series that we compiled from monthly recommendations made by the International Statistical Bureau to their department store clients. Recommendations concerned the number of weeks' requirements that should be held on hand and on order for each of a large number of departments.[18] Averaged for

[18] We are greatly indebted to A. W. Zelomek and Robert C. Shook for making these figures available. Requirements are defined as sales plus intended change in stocks. We corrected the series for seasonal variation.

all departments, the recommended position varied from about three-quarters of a month's requirements early in 1949 to almost three and a half months' requirements during the Korean war; they led at all major business turns. Month-to-month changes in the recommended holdings are shown, smoothed by a five-month centered average, in the fifth line of Chart 4. It seems unlikely that department store executives would pay for this service if they were not interested in the bearing of market prospects on buying; thus the very fact that the service is provided supports the notions here set forth. Moreover, if the series is reasonably successful in reflecting the changing market conditions that influence department store ordering, the chart indicates that market-prospect-linked buying might well look not seriously unlike what is suggested by our hypothetical computations.

In general, such evidence as can be mustered about department store merchandising falls in line with the theoretical sketch of retailers' buying based on a previous study of a single commodity group. Further test of the basic two-part explanation is required. Unfortunately, the information presently available needs to be supplemented before substantial progress is likely. Surer knowledge based on the study of retailers' objectives and practices is required. It is also essential to steady statistical test by cross reference among computations repeated for a number of commodities for which, unlike department store sales as a whole, adequate statistical representation of relevant prices or other indications of market expectations could be developed. One important advance would be made if the merchandising statistics now available for the whole store were also provided for several important depart-ments of department stores.

In the meantime, there is strong suggestion in the evidence that corrective elements which imitate first differences in sales are present in retailers' buying and that buying is also influenced by judgments about the rapidity with which the desired merchandise can be obtained and at what price. Whether the latter factor is or is not present has a very important bearing on how demand can transmit to earlier stages.

III. *Manufacturers' Buying*

If retailers' orders often reach peaks or troughs ahead of sales to consumers, the sales of manufacturers of finished consumer goods (orders placed by retailers) must also lead sales to the final users. But is there reason to believe that the factors responsible for the lead and greater amplitude of retailers buying relative to their selling also apply to manufacturers of finished goods? Do they apply to still earlier sequences in the chain? The answer must, it seems, be partly in the negative for a wide area of manufacturing.

Consider the five basic requirements mentioned in Section I. Unlike

retailers, manufacturers have advance orders that make it possible often to buy materials on the basis of known, or at least fairly accurately forecast, sales. Though they sometimes have to order materials months in advance, at least a good part of the less particularized goods produced in local factories can often be obtained very quickly. Stocks of these less particularized goods are not subject to extreme obsolescence, as are retailers' stocks, and therefore often do not need to be held so rigidly to precise plans. Moreover, for manufacturers, errors in procurement can more readily than for retailers be corrected in other ways than by adjustment of buying. For one thing, since manufacturers' customers are highly sensitive to and able to recognize a slight price advantage or disadvantage, relative change in the explicit or implicit price of finished goods has a more direct effect on sales; also, for many manufacturers changes in promotional effort, broadly defined, provide a varied and, in the hands of salesmen, a flexible tool for influencing the volume of sales. Only the factors associated with market prospects seem to apply with at least equal strength to manufacturers and retailers alike.

These differences imply that the theory of stable-market buying, as developed for retailers in Section I, cannot be applied to most manufacturers by means of any simple transformation. By and large, manufacturers' stocks increase with manufacturers' output; accordingly the rate of change in their sales may affect the size of their inventories over several semesters, but not with sufficient precision to prescribe an exact pattern of acquisition of stocks in response to variations in sales, and certainly not one characterized by leads of a few months duration. If the buying of each manufacturer in a sequence does not necessarily lead his sales, there is no reason to assume that progressive acceleration will occur as additional stages of processing are performed by additional firms, a somewhat embarrassing implication of the usual formulation of the acceleration principle.

Consider an example. Suppose a department store places a large order for bath towels with a textile mill. The cost of production on the basis of which the mill determines its selling price is necessarily greatly affected by the cost of the cotton yarn. If the bath towels are to be delivered in two months and actual production takes ten days, the yarn need not be bought for several weeks after the order is received; or, on the other hand, it could be bought and stored before the order is booked, or any time up to the ten-day limit plus the time required to deliver the yarn. There is, in other words, a "period of option" during which orders for materials may be placed.[19] Many things

will determine when, within this range, the actual purchase is made. When prices are expected to fall and the market supply is ample, buying tends to be later than when prices are expected to rise, selections are poor, and deliveries slow. If prospects are uncertain, a conservative policy dictates buying the yarn at the time that the order is booked, so that the cost of the yarn implicit in setting the price of the towel will be the actual cost to the manufacturer. If this is done, the order for yarn can follow the receipt of the order for towels by no more than a few minutes. Time for a telephone call is all that is needed. The spinner of yarn can with equal speed buy his cotton from a shipper by picking up the phone. Thus the sequence of purchases that convey the signal of work-to-be-done from the last to the earliest stage of the processing and marketing of a finished good, need take virtually no time. The sequence may be somewhat different when orders do not anticipate delivery dates by an interval long enough to allow for buying and delivery as well as for processing of materials. Also, the feasibility of hedging operations and the cost of carrying stocks affect the character of the relation.

If these descriptions are realistic, the probable pattern of stable-market buying for manufacturers is unspecified. They may buy at any time within the "period of option." The link of buying to sales can be precise, but does not need to be. The inventory objective need not be sharp. If it is, it can often be validated in the first instance, rather than by subsequent corrective buying. If correction should prove necessary, it can be effected through direct or indirect adjustment of prices and promotion, as well as through buying.

But the same logic that dictates these conclusions, suggests that market prospects may be more important in manufacturers' than in retailers' buying. However, when market prospects are uncertain, a usual policy would be the conservative one of buying materials at the time that goods are sold; at such times, the earlier or later buying of some firms would tend to cancel out for the group as a whole, so that the central tendency would be buying that synchronized with selling. At times when trends in market prospects seem well established, manufacturers may extend or contract their market position in much the same fashion as retailers. Indeed, by so doing, manufacturers may have more to gain, since prices fluctuate more widely, and less to lose, since materials are less particularized.

But in connection with the acceleration imparted by this market-prospect-linked buying, though amplitude will increase there is no reason to envision progressive leads as earlier stages of processing are reached. Processors or distributors in a given production sequence are likely to base their judgments about coming events on the same set of

prices and other indicators. For, though increasing tension is often more readily observable in the relatively public raw material markets than at later stages where trading is private, substantial tightening in the raw materials markets usually results in tightening all along the line as sellers grow coy and buyers eager. The statement with appropriate changes applies to decreasing tension. Likewise, a tendency for retailers to have misjudged demand, with the consequent flurry or cessation of "at once" orders, communicates without delay to earlier stages. As a result, businessmen at all stages of a vertical sequence tend to extend or shorten their market position at about the same time. Who constitute the vanguard and who the laggards is probably more usually a reflection of particular business situations and personalities than of the position in the vertical chain.

The pattern of orders, then, at each stage in the vertical sequence can be notably similar. But it does not have to be. It would be useful to turn to the figures to learn what actually has occurred. But unfortunately, the required figures have not been collected. Ideally we require a sequence of matched data—orders placed by department stores, by manufacturers of materials used in the production of finished goods sold to department stores, by manufacturers at the next earlier stage, and so on to the purchase of truly raw materials. What we have is very different—information for orders received by some 2,000 manufacturing corporations at any or all stages in all industries.[20] Some separation of major industry groups is feasible, but vertical sequences can not be isolated, nor do we have data on orders received and placed by the same companies.

The best that can be done is to compare two series—orders placed by department stores and orders received by nondurable-goods manufacturers that are engaged, whether at the finished or at an earlier stage, in the production of much of the merchandise that department stores sell. The two series have in common the orders placed by department stores and received by the finished goods manufacturers. Interest focuses on differences between the two series. Difference may arise from the character of the orders received by the manufacturers

[20] The Office of Business Economics of the Department of Commerce has collected monthly information since 1939 for a group of less than 2,000 manufacturing corporations in many industries. They submit data on their stocks, shipments ("sales") and monthly change in orders outstanding; the latter two combined yield data on net new orders. The sample is not large enough to make it possible to isolate firms performing a vertical sequence of operations. But even if it were, the composite character of the business done by many of the corporations would make the necessary subdivision impossible without a change in the system of reporting. For a discussion of the nature of the changes required see *Statistics of Business Inventories,* Report of Consultant Committee on Inventory Statistics Organized by the Board of Governors of the Federal Reserve System, Nov. 1955, pp. 65-70.

of semifinished goods. Divergence will occur if manufacturers of finished goods do not buy their materials (from manufacturers of semifinished goods) at the same time as they sell finished goods to department stores. Whether they do or not is what we want to know. Unfortunately, a difference between the two series may also arise because of the impossibility of compiling data on manufacturers' orders for all industries and for only those making the major sorts of goods department stores sell. Thus no durable goods industries (since they are predominantly capital-goods manufacturers) can be included; also, the apparel industries are represented by shipments rather than by orders (a difficulty meliorated but not cured by the availability of orders for the textile industry).[21]

The two sets of figures—orders placed by department stores and orders received by nondurable-goods manufacturers—are shown in Chart 5. Except for the years 1946 and 1947 they seem to share most of the major and minor movements.[22] Comparison of the dates at all matched subcycle turns shows a scatter of leads and lags, with average synchronous association. Table II (line 1) gives the figures. Although the percentage of months in unlike subcyclical phase indicates a relatively low degree of confluence, study of the chart makes this conclusion questionable, particularly since it rests on two highly marginal selections of months of turn.[23] But there is one period when there are clear differences between the two series—most of the months from V.J. Day to the final demise of OPA in mid-1947 were in unlike phase in the two series. Trade journals mention reasons: the very early buying of department stores in anticipation of the lapse of price controls, pressure put on suppliers by retailers not to raise prices, spurts in buying of manufacturers intended to beat the expected response of prices to control or decontrol measures. We conclude that at most times the

[21] For the OBE sample, sales of manufacturers of apparel are taken by the compilers as equivalent to orders. All that we could do was to follow the same procedure and add "sales" of the apparel industries to new orders for the four major nondurable-goods industries that reported new orders; the industries are textiles, leather, paper, and printing and publishing. In addition to the difficulties mentioned, there are many other presumably lesser ones such as the fact that some department store orders are placed with wholesalers, not manufacturers, and that some of the manufacturers' orders are for finished goods not sold to department stores, indeed, not sold to consumers.

[22] The department store figures have just one movement that does not appear in the manufacturing series—the minor expansion early in 1948. The manufacturing series has one movement that does not appear in department store series—the contraction toward the end of 1949. Otherwise the minor as well as the major fluctuations are shared.

[23] There are two occasions when the choice of the month of turn was a marginal decision—the trough in manufacturers' orders in December 1948 rather than June 1949 and the peak of department store orders in September 1952 instead of April 1953. Had the choices been reversed only 25 per cent of months would have been in unlike phase for the whole period and 18 per cent for the period after July 1947.

CHART 5

NEW ORDERS AND MONTHLY CHANGE IN UNFILLED ORDERS, DEPARTMENT STORES
AND NONDURABLE-GOODS MANUFACTURERS, 1940-1955
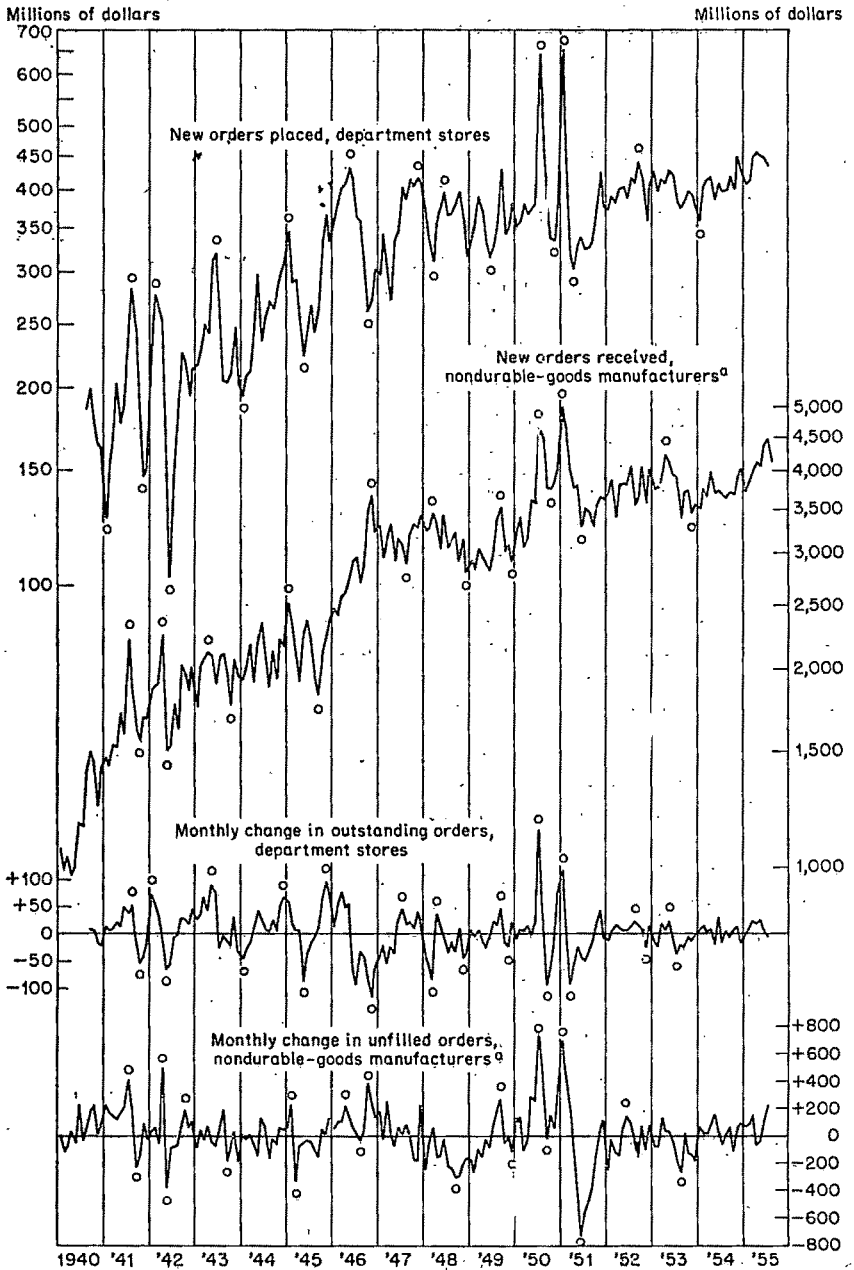
ᵃ Includes industries reporting unfilled orders and the apparel industry.
Subcycle turns are marked by o   First two series plotted on ratio scales

timing of the two series is sufficiently similar to suggest that the statistics that they do not share—among other things, orders for other than finished goods—may not have been characterized by sharply different behavior than the statistics that they do share—orders for some of the finished goods sold by department stores. However, at the one time when there is a strong reason to believe that department store buying and materials buying of manufacturers may well have differed—the year or so following the war—clear differences in the two series are evidenced.

., The system of communication between customer and supplier is not limited to new orders. A supplier may make independent guesses about what his customers are going to require and place his orders on the basis of these guesses. In placing his orders he is likely to consider the size of his backlog of unfilled orders which are, of course, also the outstanding orders of his customers. Large backlogs may justify long commitments for materials and consequently large backlogs for suppliers. If this were not the case, the shared component of the last two series in Chart 5—change in outstanding orders of department stores and in unfilled orders of their suppliers—might behave differently from the unshared component, thus causing an over-all difference between them. Both the measures in Table II (line 2) and the time series suggest close similarity once the postwar disruptions have subsided, for which again a different impact on earlier than on later stages of manufacture seems reasonable. Only 18 per cent of months are in unlike phase after 1947.

In spite of the viscosity with which stocks respond to orders, it is worth examining the appropriate statistics on stocks for retailers and for manufacturers. At least, unlike orders, the latter refer entirely to an earlier stage. Purchased-materials stocks of manufacturers reflect the firm's buying of materials more sensitively than do total stocks. Accordingly, month-to-month changes in stocks of purchased materials of the same group of manufacturers previously studied are compared with changes in retailers' stocks in Table II (line 3) and Chart 2. Retailers' stocks combine elements of purchased-materials and finished-goods stocks and therefore might be expected to respond more sluggishly to changes in buying than would be the case of purchased materials alone. Nevertheless, the figures show no evidence of a lag relative to purchased materials of manufacturers; if anything, there are more leads than lags. The average timing is virtually synchronous. A considerable similarity in timing of movements is present after the war and wartime decontrols; only 19 per cent of months are in unlike phase after 1947. An alternative comparison is provided by change in the ownership position of retailers; here, response, which is dominated

TABLE II.—COMPARISONS OF SUBCYCLES IN SELECTED INDICATORS OF RETAILERS' AND MANUFACTURERS' BUYING

Line	Series	Number of Subcyclical Turns[a]					Timing (months)[a]		Percentage of Months in Unlike Subcyclical Phase			
		All Turns	Matched Turns				Mean Lead (—) or Lag (+)	Average Deviation	Jan. 1941–Dec. 1954 (167 mos.)		July 1947–Dec. 1954 (89 mos.)	
			Total	Leads	Lags	Coincidences			Timing Adjustment[b]	Per Cent	Timing Adjustment[b]	Per Cent
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
					1. Reference Frame: New Orders of Department Stores (21 Turns, 1941–54)							
1	New Orders Received by Manufacturers Nondurable Goods[c]	20	16	9	4	3	−0.2	2.1	0	34[d]	0	33
						2. Reference Frame: Month-to-Month Change in Outstanding Orders of Department Stores (24 Turns, 1941–54)							
2	Month-to-Month Change in Manufacturers' Unfilled Orders: Nondurable Goods[c]	20	18	6	7	5	+0.2	2.1	0	32	0	18
						3. Reference Frame: Month-to-Month Change in Department Store Stocks (19 Turns, 1941–54)							
3	Month-to-Month Change in Manufacturers' Stocks of Purchased Materials	19	17	6	9	2	+0.2	2.9	0	33	0	19
						4. Reference Frame: Month-to-Month Change in Department Store Ownership Position (22 Turns, 1941–54)							
4	Month-to-Month Change in Manufacturers' Stocks of Purchased Materials	19	13	3	8	2	+1.2	1.9	+2	29	+2	15
						5. Reference Frame: Shoe and Leather Orders[e] (24 Turns, 1927–40)			Jan. 1927–Dec. 1940 (167 mos.)			
5	Leather-Goods Mfrs. Receipts of Leather[f]	23	18	6	5	7	+0.1	1.2	0	20		
6	Hypothetical Hide Orders[f]	22	16	4	4	8	−0.1	1.1	0	30		
						6. Reference Frame: Wholesale Shoe Sales, Deflated[h] (28 Turns, 1927–40)			Jan. 1927–Dec. 1940 (167 mos.)			
7	Leather-Goods Mfrs. Receipts of Leather[f]	23	21	4	6	11	+0.2	1.5	0	18		
8	Hypothetical Hide Orders[g]	22	18	9	5	4	−0.1	1.7	0	30		

[a] Figures in cols. 1–7 refer to periods identified in headings for cols. 8–9.
[b] Number of months by which the reference frame is shifted forward (+) or backward (—) in order to minimize the percentage of months in unlike phase. When equal results are obtained for different timing adjustments, the adjustment shown is that which seems to characterize best the given relation for the total period covered.
[c] Nondurable goods industries reporting unfilled orders plus apparel, see text p. 42.
[d] This figure is 32 when a lead of one month is allowed for. But the timing did not seem to characterize the whole period since 5 leads were in the war years. After 1943, synchro-

nous timing maximizes conformity.
[e] Source: Associated Industries of Massachusetts, Boston. For description see Mack, op. cit., App. B, ser. no. 38.
[f] Source: Tanners' Council of America and NBER. For detail on construction and coverage see ibid., ser. no. 89.
[g] Compiled by NBER from Tanners' Council and Census data and ibid., ser. no. 104.
[h] Source: NBER, using data collected by Federal Reserve district banks and by the Department of Commerce. See ibid., ser. no. 35.

by change in unfilled orders, is likely to be too sensitive to changes in buying to afford correct comparison with manufacturers' purchased materials. The calculations in Table II (line 4) attest to the lead. Allowing for an average lead of two months in retailers' ownership position, only 14 per cent of the months after the middle of 1947 are in unlike subcyclical phase with change in purchased-material stocks of the manufacturers who, as far as can at the moment be contrived, supply them.

One further bit of information is available. For 1927-1940 comparisons can be made between two series that purport to reflect the shoe buying of retailers and two series representing buying at earlier stages.[24] Table II, lines 5 and 7, reveal close synchronous association between our surrogates for shoe orders and for leather orders. It is less close, though still synchronous with low average deviation for a substantial number of matched turns, when comparisons are made for shoe orders with hide orders (lines 6 and 8).

In general, then, under usual conditions the data exhibit not inconsequential similarity of fluctuation for vertical sequences and no clear departure from average synchronous timing. Certainly they do not reveal the progressive advancing of turns as demand moves back to earlier stages which the acceleration principle calls for. The confusions of war and postwar readjustments seem to cause divergence in the patterns of buying and stock accumulation at earlier and later stages. Thus our admittedly inadequate information is consistent with the hypothesis that manufacturers often reproduce in the orders they place the time-pattern of orders they receive, a natural implication of the immediate backward transmission of demand or of market-prospect-linked buying. The available figures are incapable of throwing light on the relative amplitude of fluctuations. For this, as for adequate study of timing, well matched and isolated industry sequences are required; orders both received and placed by the same companies are needed.

IV. Conclusions

It seems likely that retailers' orders usually parallel their sales but with a wider amplitude of short-term fluctuation; they often reach peaks or troughs several months before those of sales. This pattern is generated by the combined impact of buying associated with two sorts

[24] The two representing retailers' buying are wholesale sales of shoes, which, the study suggests, probably bear a strong resemblance to buying, and orders received by a small sample of manufacturers of both shoes and leather. The two representing earlier stages are deliveries of leather, which are used to represent buying by shoe manufacturers, and hypothetical orders for hides by tanners (obtained from statistics on hide deliveries by assuming that domestic hides are delivered within the month ordered, and the foreign ones two months later). See also Table II, notes d to g.

of considerations: (1) close stock control necessary to retailing which gives rise to the corrective-order accelerator, and (2) the need to seek advantageous timing of buying in view of changing prospects about delivery periods, selections, and prices. The available evidence supports or fails to conflict with these notions.

Manufacturers of finished consumer goods have, by virtue of the lead in retailers' orders, advance notice of requirements. There is no reason to assume that their buying will necessarily decrease or increase this lead in orders, since the influence of rates of change in sales on corrective orders, which contributes to the earlier timing, is absent or weak. More likely, the orders will be passed on, possibly even in a matter of minutes, to the earliest stages in the production sequence, with probably, at least at some stages, increased amplitude of fluctuation. Changing market prospects contribute to the increased amplitude. Here too, the evidence does not demand explanation in other terms.

This line of thought, which highlights the importance of orders in the transmission of demand, is curiously contentious in the company of many widely accepted economic notions. An example is the "output lag." Our analysis makes doubtful the reality of a systematic and necessary lag of production as a whole relative to final sales of finished goods, for which there also seems to be no empirical evidence.[25] For each manufacturing enterprise, finished output tends to lag behind orders, or to be relatively independent of their short-term changes.[26] The length of this output lag for a particular company must depend on the length of the production period and the feasibility of its partial interruption; the possibility of variation in finished inventories is another

[25] Retail sales show some tendency to lag rather than lead manufacturing production as a whole, but this may be attributed to the marked lag in retail prices. It is difficult to make adequate allowances for price influences. Furthermore, nondurable-goods productions has a clear tendency to lead durables and the former are more intimately associated with consumption than the latter. In any event, there is certainly no empirical evidence of a lead of consumption.

Where evidence of the "output lag" has been found, its reliability is questionable. Lloyd Metzler, for example, in "Three Lags in the Circular Flow of Income" in *Income, Employment and Public Policy, Essays in Honor of Alvin Hansen* (New York, 1948) bases the conclusion that a substantial lag may be found in quarterly data on the idea that "a rise of business stocks after the close of a period of rising income is . . . *prima facie* evidence of a lag of output" (p. 25). But this is actually not the case. Output and consumption could have perfectly synchronous fluctuations, but if those of output had a wider *amplitude* of movement, stocks would continue to rise after consumption and output had started to fall, until output actually fell below consumption.

[26] The flow of new orders to manufacturers tends to recede from peak levels before manufacturers' output begins to recede; similarly, orders begin to rise from trough levels several months before output does. It seems likely that at least one of the important factors governing the interval by which output lags behind orders is the length of the time required to start and complete production. The few pieces of supporting evidence at hand are marshalled in Victor Zarnowitz, *Cyclical Behavior of Manufacturers' Orders*, forthcoming publication of the National Bureau of Economic Research.

variable. But though, for an individual company, output lags behind orders received, it only lags behind final sales to consumers if the company-output lag is greater than the lead, relative to consumer buying, of the company's orders received. There is no evidence that this is generally the case, although it might be so for complicated durable goods which must be manufactured to individual order.

A further implication of the function of orders as an agent transmitting demand is the susceptibility of the economy or portions of the economy to swift and therefore potentially brief reversals in its direction of change. Production sequences that take many months to perform may, if stocks are adequate as they typically are, be rapidly turned from rise to fall (or from slow rise to fast rise), or paced in any other way, by the order, which takes virtually no time to transmit. If for one reason or another swift changes in orders of the same sort fall on many industries at the same time, the change in pace or direction can become general; it can tangle almost immediately with additional and more complex reactions by virtue of related changes in income payments and consumer buying, businessmen's expectations, and prices.

But without surer knowledge about the actual character of the role of orders in propelling change, theoretical implications can not be comfortably explored. Available information about orders is inadequate to answer many of the questions that must be asked. This is, perhaps, the chief finding of this paper. More needs to be known about the relevant business objectives and ways of achieving them. But most urgently, monthly time series need to be collected. Again and again in the course of this exploration, we have been stopped by the inability to make the correct comparisons. To do so, information is needed as to sequences in which identifiable consumer demand can be matched with supply and demand (orders received and placed) of each member of the production sequence, with associated changes in stocks by stage of manufacture, with outstanding and unfilled orders, and with appropriate prices.[27]

The basic process of demand transmission, one of the central preoccupations of economic theory and elaborately formulated in supply and demand analysis, has remained cloistered from empirical study partly because of the clumsiness with which relevant aspects of the real world are depicted by production data. Yet far more appropriate statistics exist in every office and store.

Policy decisions also suffer for want of statistics on orders. For example it may well be that most businessmen, individually and collectively, are more harmed than benefited by wide fluctuations in their

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[27] The need for data of these kinds is discussed and collection recommended in *Statistics of Business Inventories, op. eit.,* pp. 85, 99, 100.

stocks or ownership positions. Individually, they may wish business conditions were such as to make these shifts unnecessary. Yet it is impossible to work out policies designed to diminish such fluctuations without knowing to what extent the latter reflect procurement and servicing requirements under stable market conditions and to what extent they reflect actual or expected change in market conditions.

Order data have a further use: they provide a periscope through which to look around the corner. The order records the signal to start or stop work and action follows command; consequently orders forecast output. Also, orders form the base upon which businessmen construct some of their more solid expectations about sales and inventories; and these expectations, in turn, provide the groundwork for further commands. The value of orders in forecasting has been the basis of many lucrative business advisory services. But the public and economics profession have had to subsist on a scanty fare of these data. The imagination and enterprise of the Federal Reserve Board in collecting orders of department stores for the total store, and of the Department of Commerce in the start made in the manufacturing field, whet the appetite. They provide us with most provocative questions. For solid answers we shall need to wait for more and better data.

THE SUPPLY OF STORAGE

By MICHAEL J. BRENNAN*

It is a familiar proposition that the amount of a commodity held in storage is determined by the equality of the marginal cost of storage and the temporal price spread. Why then do we observe stocks being carried from one period to the next when the price expected to prevail in the next period—reflected in the futures price quotation for delivery in that period—is below the current price.

In an attempt to explain "inverse carrying charges" in futures markets (futures prices below spot prices or prices of deferred futures below that of near futures) the concept of a convenience yield on stocks has been introduced.[1] Stocks of all goods provide a yield or compensation to the holder which must be deducted from storage costs proper in calculating *net* storage costs. In equilibrium the spread between a futures and a spot price is equal to the marginal expenditure on rent for storage space, interest, handling charges, etc., minus the marginal convenience yield of stocks. Since marginal convenience yield is a decreasing function of stocks held, the marginal convenience yield may exceed the marginal expenditure on physical storage when stocks are relatively small; hence the futures price will be below the spot price.

We shall attempt here to generalize this theory in terms of the demand for and the supply of stocks for storage. Our theory purports to provide an explanation of the holding of all stocks, including those for which there is not an active futures market. It will be shown that, on the supply side, in addition to the marginal expenditure on physical storage and the marginal convenience yield another variable, a risk premium, is required to explain the holding of stocks as a function of price spreads. In the empirical part of the study the theory will be applied to stocks of several agricultural commodities. The risk premium for each commodity will be measured residually under specified con-

[1] N. Kaldor, "Speculation and Economic Stability," *Rev. Econ. Stud.*, Oct. 1939, VII, 1-27. H. Working, "The Theory of the Inverse Carrying Charge in Futures Markets," *Jour. Farm Econ.*, Feb. 1948, XXX, 1-28.

ditions by deducting from the price spread between two periods the other two components of the marginal cost of storage.

I. *Theory of Storage*

During any period there will be firms carrying stocks of a commodity from that period into the next. Producers, wholesalers, etc., carry finished inventories from the periods of seasonally high production to the periods of low production. Processors carry stocks of raw materials. Speculators possess title to stocks held in warehouses. These firms may be considered as supplying inventory stocks or, briefly, supplying storage. The supply of storage refers not to the supply of storage *space* but to the supply of *commodities* as inventories. In general, a supplier of storage is anyone who holds title to stocks with a view to their future sale, either in their present or in a modified form.

On the other hand there will be groups who want to have stocks carried for them from one period (in which they do not intend to consume them) to another period (in which they do intend to consume them). These consumers may be regarded as demanding storage. Since production is not stable for all commodities, consumers demand that the storage function be so performed that the flow of commodities for sale will be made relatively stable. We assume that there is no significant time lag between sales out of stocks by suppliers of storage and utilization of the commodity by households, for otherwise we could not distinguish suppliers from demanders of storage. For example, in the case of butter, storage is normally performed by the manufacturer— stocks are carried from the months of seasonally high production to the months of seasonally low production. Sales out of stocks by manufacturers to wholesalers (or retailers) are assumed equivalent to sales to consumers, and the relevant price entering storage decisions is the wholesale price. This is a simple analogy to the derived demand for a commodity: with no time lag between wholesale purchases and household consumption the demand for consumption can be expressed as a function of the wholesale price.

A. *The Demand for Storage*

The demand for storage of a commodity can be derived from the demand for its consumption. We assume that consumption during any period depends only upon the price in that period; all other variables affecting consumption are exogenous. Let P_t be the price in period t and let C_t be consumption during t. The demand function in period t can then be written:

(1) $$P_t = f_t(C_t), \qquad \frac{\partial f_t}{\partial C_t} < 0.$$

The subscript indicates that demand may shift periodically.

Consumption in any period equals stocks carried into the period plus current production minus stocks carried out of the period. Consequently we may rewrite (1) as:

$$(2) \qquad\qquad P_t = f_t(S_{t-1} + X_t - S_t),$$

where S_{t-1} is stocks at the end of period $t - 1$, X_t is production during t and S_t is stocks at the end of t. For convenience it is assumed that current production and subsequent levels of production and stocks are known. To derive the demand for storage of the commodity from period t to period $t + 1$, consider the effect of an increase in carryout from period t, *i.e.*, an increase in end-of-period stocks. Under the specified assumptions, if the price in t rises, less will be consumed. With stocks carried into the period known and current production given, the rise in price in t results in less of the commodity offered for sale in t and more carried out of t. Since future levels of production and stocks are given, more of the commodity is consumed in period $t + 1$, *i.e.*, price in $t + 1$ will fall. Conversely a reduction in carryout from period t will, under these assumptions, be associated with an increase of P_{t+1} relative to P_t. In general, price in the next period minus price in the current period may be expressed as a decreasing function of stocks carried out of the current period.

Symbolically the demand for storage from period t to period $t + 1$ can be represented as follows:

$$(3) \quad \begin{aligned} P_{t+1} - P_t &= f_{t+1}(C_{t+1}) - f_t(C_t) \\ &= f_{t+1}(S_t + X_{t+1} - S_{t+1}) - f_t(S_{t-1} + X_t - S_t). \end{aligned}$$

If we differentiate this expression with respect to S_t, we see that the partial derivative is negative. With S_{t-1} known and X_t, X_{t+1} and S_{t+1} exogenously determined, the price spread is a decreasing function of S_t. The price spread may be positive or negative. Figure 1 shows the demand curve for storage.

The assumption that demand may shift periodically is a realistic and, for our purposes, useful one. In general, the demand curve for storage of a commodity from period t to period $t + 1$ will shift upward (*e.g.*, to $D'D'$ in Figure 1) as a result of (1) an increase in production in t, (2) a decrease in production in $t + 1$ or (3) an increase in stocks carried out of $t + 1$. Opposite movements of these exogenous variables will produce a shift downward.

B. *The Supply of Storage*

The supply of storage is forthcoming from those firms holding title to stocks carried from one period to another. In a competitive industry

in an uncertain world a firm seeking to maximize net revenue will hold an amount of stocks such that the net marginal cost of storage per unit of time equals the expected change in price per unit of time. We have seen that the net marginal cost of storage need not be positive. The net marginal cost of storage is defined as the marginal outlay on physical storage plus a marginal risk-aversion factor minus the marginal convenience yield on stocks.
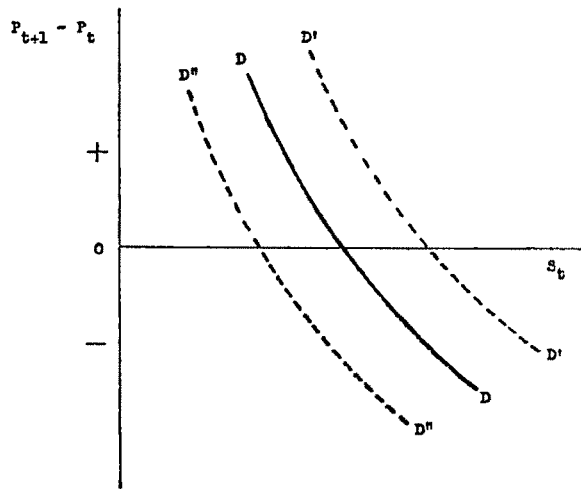
FIGURE 1

The total outlay on physical storage is the sum of rent for storage space, handling or in-and-out charges, interest, insurance, etc. As the quantity of stocks held by a firm increases, the total outlay increases. Although for any single firm this cost may increase at either a constant or an increasing rate, it seems reasonable to suppose that the marginal outlay is approximately constant until total warehouse capacity is almost fully utilized (each firm can store all it wishes without affecting the cost per unit of the commodity stored). Beyond this level marginal outlay will rise at an increasing rate.

Suppliers of storage are mostly engaged in production, processing or merchandising with storage as an adjunct. The costs of storage must be considered as charged against the business operation as a whole. Given day-to-day fluctuations in the market, a producing firm can meet a sudden and "unexpected" increase in demand by filling orders out of finished inventories or by adjusting its production schedule or by some combination of these. The convenience yield is attributed to the advantage (in terms of less delay and lower costs) of being able to keep regular customers satisfied or of being able to take

advantage of a rise in demand and price without resorting to a revision of the production schedule. Similarly, for a processing firm the availability of stocks as raw materials permits variations in production without incurring the trouble, cost and perhaps delays of frequent spot purchases and deliveries. A wholesaler can vary his sales in response to an increased flow of orders only if he has sufficient stocks on hand.

The smaller the level of stocks on hand the greater will be the convenience yield of an additional unit. It is assumed that there is some quantity of stocks so large that the marginal convenience yield is zero. Distinction is sometimes made between "surplus" stocks which will not be carried to a future period without expectation of a monetary return and "pipeline" or "working" stocks. There is an implication that pipeline stocks are relatively small and fixed in quantity. Such a distinction has little functional meaning. Actually working stocks may vary through a considerable range, their upper limit being defined as the level at which the marginal convenience yield is zero.

The third component of the net marginal cost of storage is the marginal risk-aversion factor. We should expect total risk aversion to be an increasing function of stocks. If a comparatively small quantity of stocks is held, the risk involved in undertaking the investment in stocks is also small. An unexpected fall in the price at which stocks must be sold will result in a relatively small loss to the firm holding stocks for later sale. For firms holding a small quantity of stocks as raw materials for use in production, an unexpected fall in the price will involve a relatively small loss. However, given the total capital resources of the firm, the greater the quantity of stocks held, the greater will be the loss to the firm from the same unexpected fall in the future price. There is probably some critical level of stocks at which the loss would seriously endanger the firm's credit position, and as stocks increase up to this point the risk incurred in holding them will steadily increase also—the risk of loss will constitute a part of the cost of storage. The marginal risk-aversion factor may be assumed to be either constant or, more likely, an increasing function of stocks held.

Again let S_t denote the stocks carried out of period t. Let $o_t(S_t)$ be the total outlay on physical storage, $r_t(S_t)$ the total risk-aversion factor and $c_t(S_t)$ the total convenience yield. Then the net total cost of storage $m_t(S_t)$, is defined as:

$$(4) \qquad m_t(S_t) = o_t(S_t) + r_t(S_t) - c_t(S_t).$$

o_t and r_t are increasing functions of S_t so that the marginal outlay and marginal risk aversion are either constant or are increasing functions of S_t, i.e., $o_t' > 0$ and $o_t'' \geq 0$; $r_t' > 0$ and $r_t'' \geq 0$. c_t is also an increasing function of S_t, but the marginal convenience yield declines and reaches zero

at some large level of stocks, *i.e.*, $c_t' \geq 0$ and $c_t'' \leq 0$. The net marginal cost of storage in period t may be written as:

$$(5) \qquad m_t'(S_t) = o_t'(S_t) + r_t'(S_t) - c_t'(S_t).$$

The net marginal cost of storage need not be positive. When stocks are relatively small, c_t' will be large. If c_t' is large enough relative to o_t' plus r_t', the net marginal cost of storage will be negative. Figure 2 de-
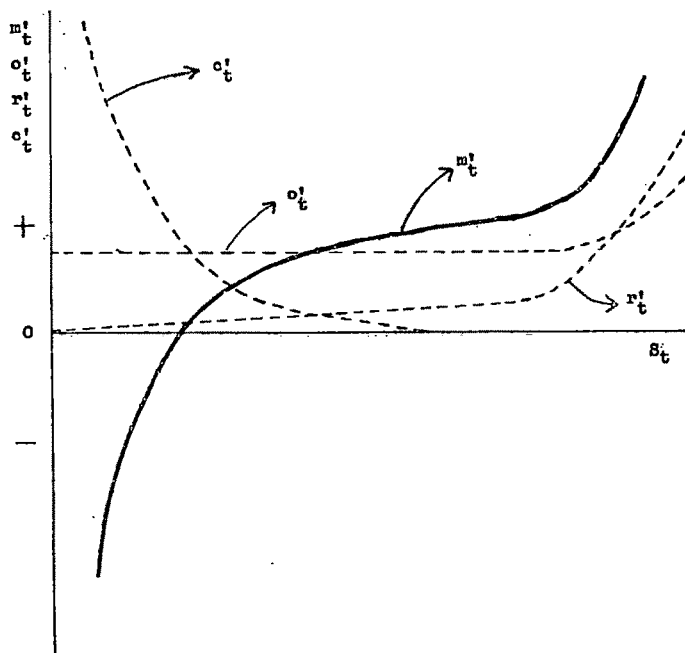
FIGURE 2

picts graphically the net marginal cost of storage and its three components for a typical firm.

Let the expected marginal revenue of stocks carried out of period t be represented by $u_t'(S_t)$. In a competitive industry u_t' equals the expected change in price from period t to a future period, say $t+1$.[2] Total expected net revenue is equal to:

$$(6) \qquad u_t(S_t) - m_t(S_t).$$

The quantity of stocks which maximizes expected net revenue is found by differentiating (6) with respect to S_t and setting the derivative equal

[2] For a firm operating under conditions of nonpure competition, u_t^1 is a decreasing function of stocks held.

to zero. This gives

(7) $$u_t'(S_t) = m_t'(S_t),$$

which expresses the familiar condition that expected net revenue is maximized when expected marginal revenue equals net marginal cost. The conditions on the second derivatives of o_t, r_t and c_t insure that the solution is a maximum.

Under the assumptions of pure competition and no external economies or diseconomies in the storage industry the supply curve of storage is the horizontal sum of all individual net marginal cost functions. Thus the sum of equations like (7), when solved for S_t as a function of u_t', is the supply curve of storage. We denote the supply of storage by

(8) $$u_t' = g_t(S_t).$$

C. *Equilibrium*

We can now use the demand for and supply of storage to determine the equilibrium quantity of stocks carried out of t as a function of the price spread. We suppose that prices expected to prevail in future periods are the same for each firm. The equilibrium quantity of stocks is determined by the equality of the demand for stocks and the supply of stocks:

(9) $$u_t' = EP_{t+1} - P_t,$$

where EP_{t+1} is the price expected in period $t + 1$ and P_t is assumed known. Using (3) and (8) this can be written

(10) $$g_t(S_t) = Ef_{t+1}(S_t + X_{t+1} - S_{t+1}) - f_t(S_{t-1} + X_t - S_t).$$

For a two-period model the equilibrium is illustrated in Figure 3. *DD*, *D'D'* and *D"D"* are demand-for-storage curves; *CC* is the supply curve of storage. If *DD* is the demand curve in t, the equilibrium price spread will be *OR* and the equilibrium quantity of stocks carried out of t will be *OL*. If production during t or the expected production and/or carryout in $t+1$ were to change, the demand curve would shift. Take, for example, the case in which production is reduced in period t, production in $t+1$ is expected to increase or stocks carried out of $t+1$ are expected to decrease. Then the demand curve will shift downward to *D"D"* so that the equilibrium expected price spread is negative, *OQ*, and the equilibrium quantity of stocks is *OK*.

II. *The Supply Curve of Storage of Selected Agricultural Commodities*

Suppose that the period under consideration is one month. Then as production and planned future inventories vary seasonally the demand curve will shift. Normally the cost per unit of storage space, handling charges per unit, the allowance for spoilage and negotiation

charges will not vary from month to month. Interest and insurance charges per unit vary seasonally with the price. Since insurance is a very small proportion of the cost per unit stored and since the variation in price would be responsible for only a very small percentage variation in interest charges per unit stored, we may take the marginal outlay function as relatively stable. Furthermore, it is unlikely that the marginal convenience yield function and the marginal risk-aversion
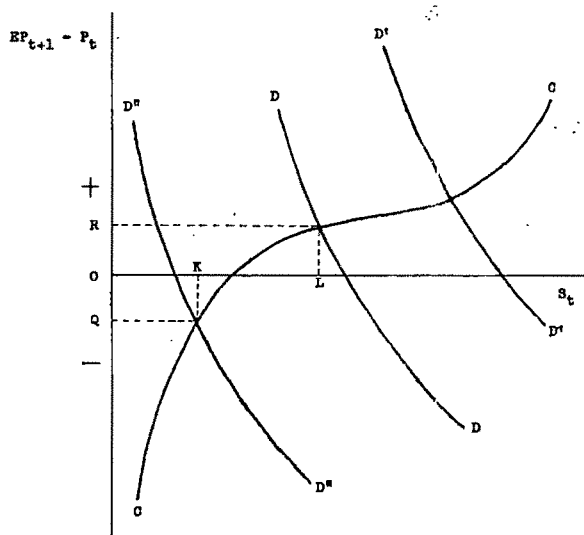
FIGURE 3

function will shift significantly from month to month. Consequently, we may reasonably assume that the supply curve of storage is relatively stable. If we allow demand to fluctuate over a stable supply function, a supply curve of storage can be generated empirically by measuring the relationship between $EP_{t+1} - P_t$ and S_t for $t = 1, 2, 3, \cdots, n$.

However, merely plotting appropriate price spreads against stocks would conceal the relative importance of the three components in the net marginal cost of storage. Indeed it would not tell us whether or not more than two components are required to explain end-of-period stocks. Consider the equilibrium equation $EP_{t+1} - P_t = o_t' + r_t' - c_t'$. The price in period t, P_t, and the marginal outlay, o_t', are in principle observable. Granted that a means can be obtained for estimating the expected price, EP_{t+1}, these three variables can be used to estimate the residual, $r_t' - c_t'$. If in conjunction with the marginal outlay the marginal convenience yield is sufficient to explain the level of stocks held, i.e., if

$r_t' = 0$, the residual should be a monotonically nondecreasing function with zero as a maximum. If, on the other hand, the residual rises above zero over a significant range of stocks, another variable is required to explain the holding of stocks as a function of price spreads. For several agricultural commodities the residual $EP_{t+1} - P_t - o_t'$ is labeled $r' - c'$ and is related to total stocks in Figures 4 to 8. Before proceeding to these empirical conclusions, however, let us examine the means used to derive estimates of the relevant variables.

A. *Expected Prices*

For stocks which are hedged on an active futures market the price spread relevant to a decision about storage levels is the difference between a futures and a spot price. Arbitrage between cash and futures markets will insure that the cash price expected to exist in a future period is accurately reflected in the current quotation of the futures price for delivery in that period. Some commodities have no futures market, yet they are stored in economically significant quantities. For others a futures market exists but the amount of hedging is negligible. Consequently we need a method for obtaining reasonable approximations to expectations of future prices.

In general it will be assumed that decision-makers include in their expectations of monthly prices notions of trend, seasonal normal, and transitory factors. In the absence of more specific information it seems reasonable to suppose that the sequence of prices expected to prevail within one planning interval, for example one production and storage year, depends in some way on past prices. Of course price expectations are shaped by a variety of influences so that the expression of expected price as a function of past prices is merely a convenient way to summarize these diverse influences.

To estimate the trend in monthly price expectations a linear multiple regression of annual average price on past annual average prices is used. This yields a prediction of the expected annual average price in the planning year. Upon this estimated annual average price a seasonal price index is superimposed as an estimate of the "normal" sequence of future monthly prices.[3] This method gives a *typical* structure to

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[3] Because it has yielded the smallest standard error of estimate for all commodities studied a linear multiple regression of annual average price on past annual average prices has been chosen as the means for estimating expected annual average price. For the period covered by the data the seasonal indexes were applied as follows: a new 5-year index was computed after each 3 years in order to take into account gradual changes in the seasonal price pattern. The 5 years' used to compute the index were taken as the 5 years preceding the first year of the 3-year period in which it was applied so as to take into account the fact that future prices are unknown and to (partially) eliminate any transitory elements in the monthly price pattern of the immediately preceding year.

the sequence of expected monthly prices. Since there are a number of diverse transitory influences which could possibly be operative in any given planning interval (leading to a complex expectation function) and since those which are operative will result in shifts in the demand and/or supply schedules, we assume for simplicity that the transitory factors can be summarized in the price variable. Recent prices are a partial result of forces expected to continue to operate in the near future. It is highly probable that the more recent the past price the more that price expresses the operation of forces relevant to expectations. The transitory influences may not have been operative three or four months previously; if transitory factors were operative then, they will likely be reflected to more or less degree in the prices of more recent months.

Assuming a planning interval of n months, before adjustment for transitory factors we have a vector of n expected monthly prices which reflect trend and seasonal normal. Let the vector of $n-t$ adjusted expected prices formulated in month t be represented by

$$(11) \qquad P^*(t)_{t+1}^{n} = (EP_{t+1}, EP_{t+2}, \cdots, EP_n).$$

There will also be a vector of $n-(t-1)$ adjusted expected prices which was formulated in month $t-1$. Let the vector of $n-t$ of these expected prices formulated in $t-1$ be represented by $P^*(t-1)_{t+1}^{n}$. That is, of the $n-(t-1)$ expected prices formulated in month $t-1$, $P^*(t-1)_{t+1}^{n}$ represents $n-t$ of them extending from period $t+1$ to period n. In this way both $P^*(t)_{t+1}^{n}$ and $P^*(t-1)_{t+1}^{n}$ contain the same number of expected prices covering the same future months. The adjustment for transitory factors in period t is:

$$
\begin{aligned}
P^*(t)_{t+1}^{n} - P^*(t-1)_{t+1}^{n} &= \alpha(P_t - EP_t) + \alpha(1-\alpha)(P_{t-1} - EP_{t-1}) \\
(12) &\qquad + \alpha(1-\alpha)^2(P_{t-2} - EP_{t-2}) + \cdots
\end{aligned}
$$

where α is a constant less than or equal to 1, EP_t is obtained from $P^*(t-1)_t^{n}$, EP_{t-1} is obtained from $P^*(t-2)_{t-1}^{n}$, etc.

Expression (12) states that the sequence of expected future prices is adjusted for transitory factors by a weighted moving average of the errors made in past predictions in which the weights decline toward zero as one goes back in time. Decision-makers in revising their expectations upward or downward take into account not only the error made in the preceding month but also those of several past months. In each month, as each new price becomes known, a new adjustment of all remaining future prices in the planning interval is made. The number of past errors to be included will depend upon the value of α. The assurance that the weights decline means that practically we can ignore

errors in the very distant past. The closer is α to 1, the less importance distant past errors assume.[4] If we try a number of different values for α, we can find a value which will yield the least deviation between expected and actual prices (and for the methods used to estimate trend and seasonal normal the greatest correlation between expected and actual prices).[5] This iterative procedure has been applied to the 5 commodities studied and the smallest errors of estimate are consistently obtained by using 3 past errors for some commodities and 4 for others with rounded values of α varying between .8 and .7.

What might be said in favor of this "naive" expectation model? First, it is likely that decision-makers with first-hand information available to them in each period will form price expectations with deviations from actual prices at least as small as those obtained by this method. Expected price spreads in relation to stocks derived from the use of this model are not significantly different in general form from actual price spreads, $i.e.$, where $EP_t = P_t$. If the price expectations of decision-makers should lie between actual prices and our estimates of their expectations, then the substantive conclusions about the supply curve of storage would not be significantly different from the case in which we knew exactly and completely how each price expectation was formed. Secondly, attempts to construct expectation functions of a more sophisticated nature have not yielded results as accurate in predictability as those derived from this model. Finally, in the case of those commodities for which there are active futures markets the futures price has also been used as the expected price. When the fitted curve relating stocks to expected price spreads derived by the above method is superimposed on observations involving futures price spreads, the scatter around the curve is close enough to warrant confidence in this model of expected prices.

B. *Marginal Outlay*

There are no published data on storage charges per unit per month made by warehouses to owners of stocks; it was necessary to collect this information directly from a sample of cold-storage warehouses,

---

[4] The sum of the weights for m past errors is $1 - (1 - \alpha)^{m+1}$. The number of past errors to be included can be found from the formula

$$\left| 1 - (1 - \alpha)^{m+1} \right| \leq 1$$

in which the sum of the weights is made as close to 1 as is practicable. In this way we allow the data to choose the number of past errors to be included subject to the constraint that the sum of the weights is less than or equal to unity.

[5] Under the assumptions made above it can be shown by maximization of the likelihood function for each chosen α that this iterative procedure yields maximum likelihood estimates.

grain elevators and other sources.[6] The total cost per unit is composed of rent for storage space, handling charges, interest, insurance and an allowance for negotiation charges and spoilage. The charge for storage space is relatively stable from 1924 to 1938 for eggs, butter and cheese and from 1924 to 1932 for wheat and oats. To determine the handling charge per month the charge, quoted in cents per unit, was divided by the average number of months that stocks were held in storage during the year minus one month. One month was deducted in order to give a maximal estimate of marginal outlay, and thereby the hypothesis being tested was placed at its greatest disadvantage.

From 1924 to 1930 interest was figured at 6 per cent per annum, the rate normally charged by warehouses on funds advanced to owners of stocks during this period.[7] The customers' rate charged by banks on commercial loans in 19 cities averaged a little less than 5 per cent for this period. Between 1930 and 1939 this bank rate fluctuated between 5 and $2\frac{1}{2}$ per cent, averaging about $3\frac{1}{2}$ per cent. Since warehouses report that they normally charge a rate slightly higher than the bank rate, interest has been imputed at 1 per cent above the bank rate. The monthly interest cost per unit was computed as a percentage of the weighted average monthly price during the months in which stocks were being accumulated seasonally. Each monthly price was weighted by the increase in stocks during that month.

The median insurance charge appears to be about .08 per cent of the value per month and was estimated at .10 per cent of the weighted average monthly price. An allowance of .05 per cent of the weighted average monthly price was attributed to butter, eggs and cheese for spoilage and negotiation charges. An allowance of .03 per cent was attributed to wheat and oats. In the light of comments made by warehouse operators these allowances are probably slight overestimates. The estimate of storage costs is the average of the charges made in the important storage areas. Geographical differences in storage rates and value of stocks are not sufficient to alter the empirical conclusions. The variance of o_t' relative to the residual $r'-c'$ is small enough to dismiss the possibility that the size of $r'-c'$ is significantly dependent upon the size of the variance of o_t'.

The charge per unit made by warehouses to owners of stocks does

[6] Sources in the survey from which replies were received for butter, cheese and eggs are 18 cold storage warehouses in 8 geographically dispersed cities; 2 meat-packers with national distribution; 2 middle-western cooperatives; the Chicago Mercantile Exchange and the U. S. Department of Agriculture. Sources for wheat and oats are 14 grain elevators in 7 dispersed cities; the Chicago and Kansas City Boards of Trade; the Grain Exchange Institute, Inc., Chicago and the U. S. Department of Agriculture.

[7] E. A. Duddy and D. A. Revzan, "Profits and Losses in the Storage of Butter," *Jour. Bus. Univ. Chicago,* Oct. 1933, VI, 293-317.

not increase as stocks increase. This, together with the fact that inter-
est, insurance and handling charges per unit are not rising functions
of stocks, suggests that the marginal outlay is constant and equal to
the average outlay. It is still possible, however, that marginal outlay
rises as stocks increase over the relevant range because at or near the
peak storage months warehouses in the important storage areas are
filled to capacity and additional stocks must be diverted to warehouses
located at a greater distance from the point of production or sale. Then
the marginal cost would rise with the distance from the normal storage
area and with the additional amount of handling required. The facts
do not support this supposition. In the years 1924 to 1938, for the
economy as a whole the peak occupancy period for public and private
cold storage warehouses was around October, at which time about
70 to 80 per cent of capacity was utilized. The peak seasonal storage
period for butter, cheese and eggs occurred in the summer months when
60 to 70 per cent of capacity was utilized. In the survey of warehouses,
firms in the most important storage areas replied unanimously that
capacity was never utilized during this period to the extent that stocks
had to be diverted to other areas. The same reply was received from
grain elevators with respect to wheat and oats. Except for a few geo-
graphically and temporally isolated instances, such as 1929 in Chicago,
there was never exceptional congestion of elevators during the period
1924 to 1932. As a consequence the average outlay has been used as
an approximation to the marginal outlay assumed constant over the
range of observed levels of stocks.

C. *Risk and Convenience Yield*

For several agricultural commodities end-of-month stocks are plotted
against the residuals $EP_{t+1} - P_t - o_t'$, where t is equal to one month, P_t
is the average monthly price and EP_{t+1} is calculated by the method
outlined previously. The relations of the residuals, $r' - c'$, with stocks
are shown in Figures 4 to 8.[8] Three semiperishable and two durable
farm commodities were selected for study. The commodities are further
differentiated by the importance of futures markets. For shell eggs,
cheese and butter the data cover the period 1924 to 1938; for wheat and
oats, 1924 to 1932. Later dates were omitted because of the influence
of government price-support and storage programs. Prices were deflated
by the wholesale price index of all commodities (1926 = 100). Stocks

[8] Data for these figures were derived from the following sources: (1) cash prices:
Yearbook of Agriculture, Agricultural Statistics; (2) futures prices: *Grain Futures Statistics,*
Stat. Bull. 131, USDA, July 1953; (3) stocks: *Agricultural Statistics; Cold Storage
Report,* USDA monthly; *Dairy Statistics and Related Series,* Stat. Bull. 134, USDA, Oct.
1953; *Grain and Feed Statistics,* Stat. Bull. 159, USDA, Mar. 1955 and information made
available by the U. S. Department of Agriculture.
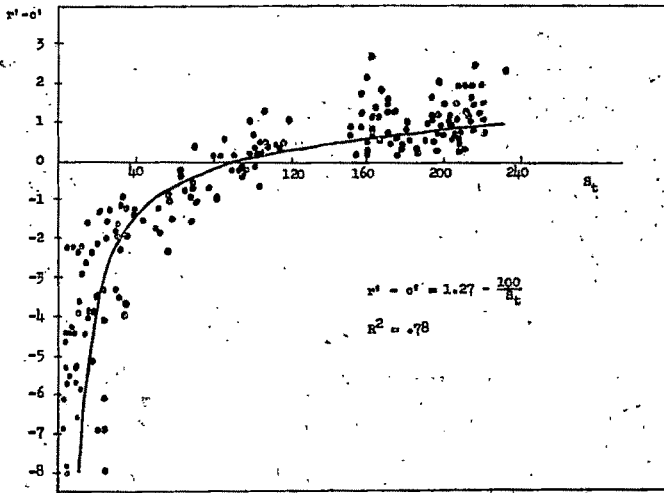
FIGURE 4.—Shell Eggs: Average wholesale price spreads per dozen, Chicago, by months, 1924-1938 minus marginal outlay per month related to end-of-month total cold-storage holdings.

are expressed as percentages of a 13-month moving average in order to facilitate monthly comparisons allowing for long-term growth in production and stocks. A number of curves that conform generally to the shape of the scatter diagram were fitted by least squares, and the curve giving the best fit was used.

Although most of the diagrams are self-explanatory, Figures 5, 7 and 8b call for some comment. The special circumstances surrounding the aging of cheese in storage account in large part for the relatively small percentage seasonal fluctuations in stocks. Because certain cheeses are undergoing a relatively long aging process the amount of
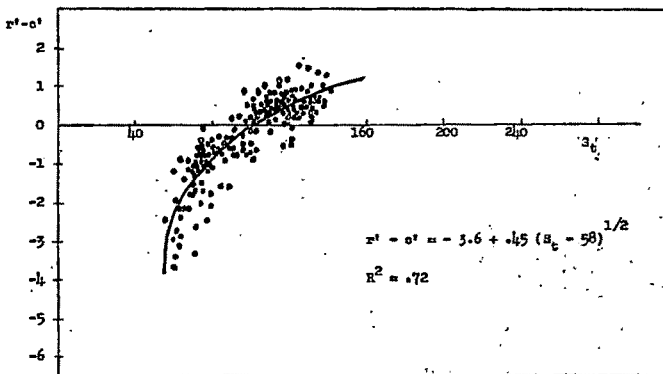
FIGURE 5.—Cheese: Fresh single daisies, average wholesale price spreads per pound, Chicago, by months, 1924-1938 minus marginal outlay per month related to end-of-month total cold storage holdings.
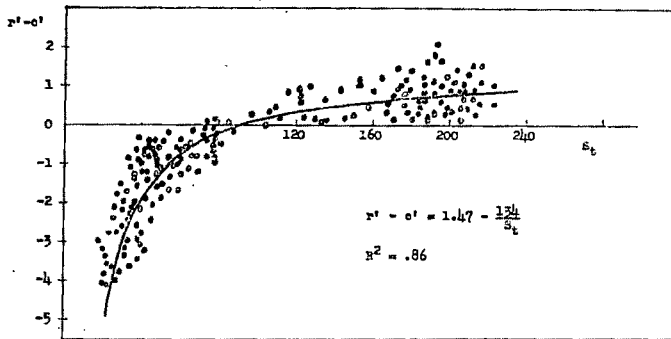
FIGURE 6.—Creamery Butter: Ninety-two score, average wholesale price spreads per pound, Chicago, by months, 1924-1938 minus marginal outlay per month related to end-of-month total cold-storage holdings.

cheese held in storage does not respond readily to changes in monthly price spreads. The effect of this is to raise the absolute level of the seasonal trough for total cheese holdings. The absolute quantity of stocks held during the peak months may be larger than stocks of commodities for which processing and storing are not a single operation;
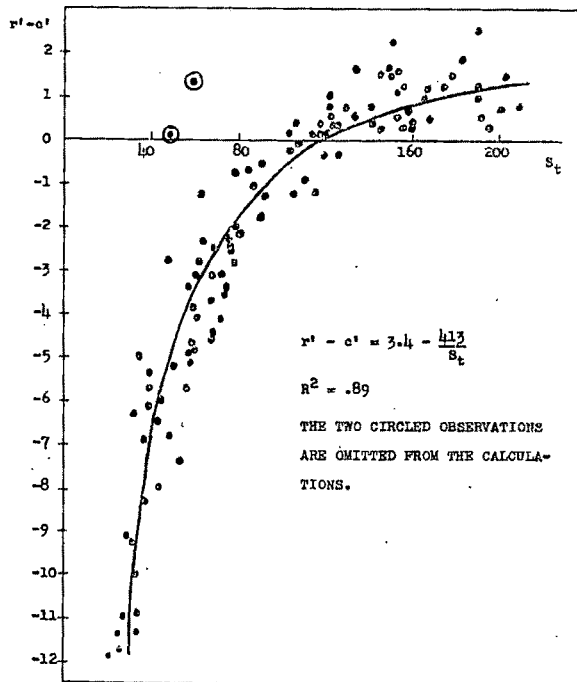
FIGURE 7a.—Wheat: No. 2 hard winter, average price spreads per bushel, Kansas City, by months, 1924-1932 minus marginal outlay per month related to total end-of-month wheat stocks.
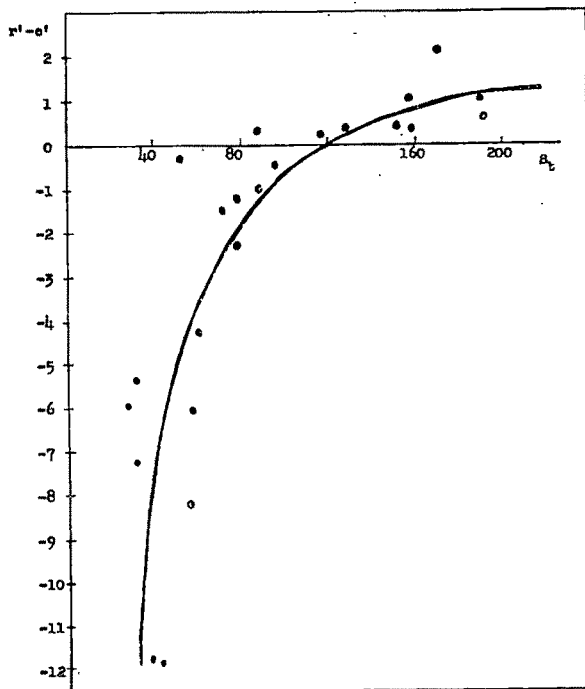
FIGURE 7b.—Wheat: Futures price spreads,* end-of-month closing prices of principal futures, Chicago Board of Trade, 1924-1932 minus marginal outlay related to total end-of-month wheat stocks.

* See footnote 9.

nevertheless, the higher annual average level of stocks per month will result in a relatively smaller seasonal peak when expressed as a percentage of trend. To enable comparison of expected-price calculations Figures 7b and 8b show the curves derived in Figures 7a and 8a superimposed on the scatter of points yielded by futures price spreads minus marginal outlay.[9] Though futures price quotations provide fewer ob-

[9] Rather than compare a current cash price with the current quotation of a distant futures price, the current quotation of the futures price for delivery in a distant month was subtracted from the current quotation of the futures price for delivery in the current month or a near month. This was done to insure that the prices being used referred to identical commodities. The cash wheat price in July may differ from the July futures wheat price quoted in July by the inclusion of wheat "on track" in the cash price. The futures price spreads used in Figures 7b and 8b are the following: (1) September futures minus July futures at the end of July 1924 to 1932 exclusive of 1928 and 1931; (2) September futures minus July futures at the end of June 1924 to 1932 exclusive of 1928 and 1931; (3) May futures minus March futures at the end of March 1929 to 1932 exclusive of 1931; (4) December futures minus September futures at the end of September 1925 to 1932 exclusive of 1928 and 1931. Years in which corners, squeezes or exceptional circumstances affected the price spreads are omitted in order to depict the normal relationship more clearly. March quotations are not available prior to 1929. The exceptional price spreads in 1929 are due in part to unusual congestion of grain elevators in Chicago.
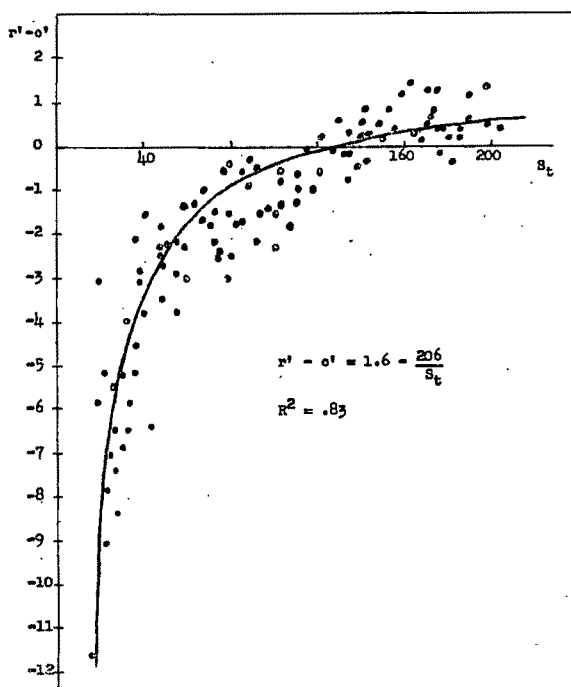
FIGURE 8a.—Oats: No. 3 white, average price spreads per bushel, Chicago, by months, 1924-1932 minus marginal outlay per month related to total end-of-month oats stocks.

servations, there is no significant difference between this curve and one fitted to the 23 observations by least squares. Similar comparisons were not made for the semiperishables because futures trading is often too light to afford good quotations.

If one is willing to allow a few "heroic" assumptions, it is possible to obtain a plausible quantitative estimate of the risk premium involved in storage. First, the marginal convenience yield is approximately zero in the peak storage months. Second, the marginal risk aversion is a linear increasing function of stocks; for the most plausible supposition about the marginal risk aversion is that it does increase with the level of stocks. If the marginal risk aversion were constant, this would imply that, given their capital resources, firms' decisions about additions to stocks would be independent of the amount already invested in stocks. This seems improbable. With c' equal to zero at the maximum observed level of monthly stocks, the residual is r', and the linear function passes through this point and the origin.

Since marginal outlay is equal to average outlay, the average monetary return or "profit" from storage can be obtained from knowledge
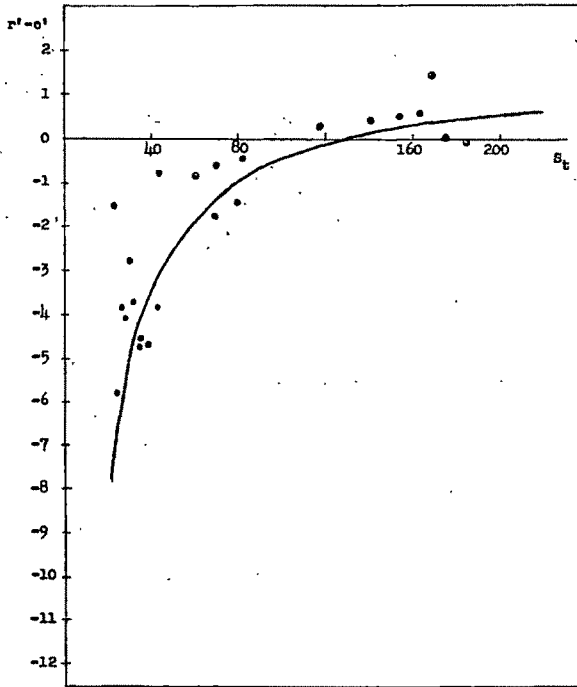
FIGURE 8b.—Oats: Futures price spreads,* end-of-month closing prices of principal futures, Chicago Board of Trade, 1924-1932 minus marginal outlay related to total end-of-month oats stocks.

  * See footnote 9.

of this linear function. When stocks are relatively small, for each unit of the commodity put into storage a small risk premium will be required. The larger are stocks already held, the larger will be the risk premium of each unit put into storage in that month. If the average of these monthly returns is taken, an approximation to the return per month is obtained. Suppliers of storage move temporally along the risk function from the month of seasonal minimum stocks to the month of seasonal maximum and back again to the seasonal minimum. The average of these monthly values expressed as a percentage of price gives the average percentage return per month.

   This has been done for each of the commodities studied and the estimated returns *per annum* are as follows: wheat 6.6 per cent, oats 6.8 per cent, butter 7.9 per cent, eggs 8.5 per cent and cheese 9.5 per cent. The relative difference in annual returns can be explained primarily by perishability. In addition to deterioration there is a quality factor which affects storage of and speculation in the semiperishables

which does not exist to nearly the same degree in the case of more durable commodities. A speculator in grain can be comparatively certain that the commodity coming out of storage is the same as the commodity that went into storage. Because of a possible taste differential between fresh and stored semiperishables this is not true to the same extent for these commodities. Consequently this lack of knowledge on the part of speculators would require that they obtain a greater return for undertaking the risk of investment in semiperishable stocks. Shell eggs appear to be the commodity most affected by this uncertainty with respect to quality. There is another reason for the relatively higher return on cheese. The absolutely larger seasonal minimum level of stocks, shown in Figure 5 and explained earlier, means that the annual average investment in stocks is larger. As a result we should expect a risk premium in proportion to the larger annual average quantity of stocks held.

One interesting feature of the functions in Figures 4 to 8 is that they may be used to explain why such small proportions of semiperishable stocks have been hedged on the futures markets. By far the most important holders of semiperishable stocks are producers, wholesalers and dealers. These stocks are carried from the months of seasonally high production to the months of low production. A maximal estimate is that about one-third of total annual butter stocks were hedged prior to 1940. Of those owners of butter stocks who did engage in hedging —those who held the largest quantities of stocks—most hedged only a part of their total holdings.[10] A similar situation exists for eggs and cheese. Opinions expressed by officials of the various mercantile exchanges indicate that they have been unsuccessful in attempting to establish active futures trading on a large scale for these semiperishables.

When stocks are relatively small, the marginal convenience yield is relatively large; stocks will be held unhedged if the owner can expect to receive a marginal risk premium reflected in the expected seasonal price structure. There is always the possibility that the demand for consumption will increase *within* any one month, and the marginal convenience yield expresses the allowance made for this by owners of stocks. If this increase in demand is considered to be permanent, expected prices will rise in each future month within the planning interval. The immediate effect of the rise in demand is to raise the current cash price. As we have seen in the theory of the demand for storage, if future levels of production and stocks were not to adjust to the

[10] H. S. Irwin, *Impressions of Trading in Butter and Egg Futures*, Comm. Exch. Admin., USDA (Washington, D.C., Feb. 1940).

permanently higher expected prices, this would mean a decrease in demand for stocks to be carried out of the current month and a decrease in the price spread. But when sufficient time is allowed for the production schedule to be adjusted, then production in the current month and perhaps production and storage in future months will increase.

The increase in demand for consumption will take the form of an expanded flow of orders, and the availability of stocks to meet the increased flow provides the convenience yield. Suppose that the cash price on August 10 is 50 cents and the December futures price is 60 cents (the net marginal cost of storage is 10 cents). If the cash price on August 11 were to rise to 55 cents, arbitrage between the cash and futures markets would insure a higher futures price also, given that the change in demand is considered permanent. Let us suppose that it rises to 65 cents, reflecting a continued higher expected price level. Production and stocks can be adjusted to reach the levels consistent with the ten-cent price spread. Consider a firm which has on hand unhedged stocks. It can meet the increase in demand for consumption and cash price by filling orders out of stocks. This temporarily reduces stocks below the level which maximizes net revenue, but these stocks can be replaced from later production in such a way that the new total level of stocks held maximizes the expected net revenue reflected in the new ten-cent price spread. Now consider the owner of stocks who has hedged. Though the stocks have been committed to future sale at a futures price quoted at the time of the hedge, e.g., 60 cents, the stocks could still be used to take advantage of the increase in the cash price; however, the futures price quotation will have risen also. Then the purchase of a futures contract to cover the short sale would (partially) eliminate the gain from sales out of stocks.

It might be argued that it would be unnecessary to purchase a futures contract. One could still meet the short sale out of later production. But this later production would have to be sold at the futures price originally contracted when the hedge was effected, and this would now be lower than the current futures price quotation. There is a return foregone. As long as there is a relatively high marginal convenience yield and the firm expects a risk premium for carrying stocks into a future period, it will be to the firm's advantage to hold some stocks unhedged in order to be prepared for a possible unforeseen contingency.

As stocks accumulate the marginal convenience yield will decline. Since the owner of stocks has on hand unhedged stocks out of which he can fill increased orders, he can now hedge any additional stocks and shift the risk to speculators. The reason why he may decide to shift the risk is that *total* risk aversion is approaching the critical level —critical in terms of the firm's capital resources and credit position as

explained earlier. Since there are a potentially infinite number of speculators, the risk will be spread over a greater number of individuals in society and borne at a smaller cost than that required in the case of any limited number of firms. When the marginal risk premium required by any individual firm exceeds that required by speculators, stocks will be hedged.

Under the specified assumptions it has been shown, on the basis of the evidence presented in Figures 4 to 8, that speculators in grains. require a smaller marginal risk premium than speculators in semi-perishables. Consequently we should expect that, given the capital resources and convenience yields of storage firms, a greater proportion of grain stocks would be hedged relative to semiperishable stocks. If the increasing marginal risk functions of a holder of butter and a holder of grains are approximately the same, the risk premium will exceed that of speculators at a smaller level of holdings for the storer of grains. Since the risk premium required by speculators in butter is larger than that required by speculators in grains, the butter storer must accumulate relatively greater stocks before his risk premium will exceed that of speculators. When stocks of any commodity are large enough to require hedging, the risk premium for the market as a whole is the risk premium of the speculators in that commodity. We may interpret the evidence on price spreads, risk premiums, convenience yields and hedgings for the economy as a whole as the combination of forces operating simultaneously to determine the level of stocks held as a function of price spreads and the proportion of these stocks hedged on the futures markets.

III. *Conclusion*

When referring to the motives for holding money, the convenience motive is ordinarily distinguished from the speculative motive. The exact counterpart of this convenience motive for holding stocks would apply strictly only to those held in order to avoid the nuisance and cost of (1) frequent deliveries for processing and (2) frequent revisions of the production schedule to effect increased sales. The definition presented in this paper is somewhat broader. To include in the definition of convenience yield the benefit of being in a position to take advantage of a possible price rise on short notice would seem to place these holdings in the category of speculative stocks.

In such questions of definition usefulness is generally accepted as the criterion for choice. The line of distinction between convenience and speculative stocks is a thin one. If a firm is presumed to hold stocks for the purpose of convenient handling of an expanded flow of orders, how can this increase in orders occur unless the total market

demand for consumption increases? If it is not a mere shift in orders from one firm to another while market demand remains unchanged, the current price can remain constant only if supply is perfectly elastic in the short run; there is nothing a priori to guarantee that this is the case. On the contrary, once stocks have been put into storage with the intention of carrying them to a future period, price in the current period would have to be bid up by those who intend to "consume" them in order that some of the stocks be diverted to current consumption.

Holbrook Working has pointed out two shortcomings of the price-of-storage theory which expresses net marginal cost of storage as marginal outlay minus marginal convenience yield (defined in the narrow sense).[11] Much storage is supplied by firms which do not hedge (completely), and many hedged stocks earn a gross monetary return per unit which is not exactly equal to the marginal outlay. The virtue of our broader definition is that, by means of this concept together with the marginal risk premium, it is possible to explain the degree of hedging as well as the difference between the gross return and the marginal outlay. It emphasizes the importance of risk for seasonal accumulation and provides an explanation of the "inverse carrying charge."

The notion of risk has entered previous discussion of storage. Several British writers have viewed the marginal risk premium as an increasing function of stocks; however, the risk premium was used primarily in an attempt to explain inverse carrying charge by the phenomenon of discounting the future.[12] For hedgers who are forward sellers (the majority of suppliers of storage) the futures price must fall short of the expected price by the amount of the marginal risk premium; hence, if the current price is expected to remain unchanged, the futures price must be below the spot price. For hedgers who are forward buyers the futures price is above the expected price by the amount of the marginal risk premium. Although this summary is somewhat skeletal, it illustrates the different role played by the risk premium. Holbrook Working has effectively demonstrated that the attempt to use risk in this way to explain inverse carrying charge hinges on a concept of substantial independence of cash and futures markets.[13] Continuous effectiveness of arbitrage between cash and futures prices makes it necessary to regard the two sets of prices as determined in a single market, i.e., expected price equals futures price. On the basis

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[11] H. Working, "The Theory of Price of Storage," *Am. Econ. Rev.*, Dec. 1949, XXXIX, 1254-62.

[12] J. M. Keynes, *Treatise on Money* (New York, 1930), II, p. 143.

[13] Working, "Theory of Inverse Carrying Charge," *loc. cit.*, p. 2.

of statistical evidence inverse carrying charge is better explained by a theory that does not involve explicit consideration of any supposed discounting of the future.

The theory presented in this paper provides a general hypothesis to explain the degree of hedging as well as intra- and interyear storage behavior. Two of the components which determine the supply curve of storage are not directly observable, yet the hypothesis can also be used to obtain a measure of the relative risk premiums involved in the storage of different commodities.

THE FOREIGN-TRADE AND BALANCED-BUDGET MULTIPLIERS

By F. D. Holzman and Arnold Zellner*

The primary purpose of this paper is to clear up some obscurities in the short-run[1] static theory of the foreign-trade multiplier. We shall also indicate the implications of our results for the theory of the balanced-trade, repercussion, and balanced-budget multipliers.

I. *The Foreign-Trade Multiplier*

In *The Means to Prosperity*, Keynes provides a verbal exposition of the foreign-trade multiplier concept which we formalize with the aid of the following model:

(1) $$Y = C + I + X - M_a$$

(2) $$C = C_a + cY.$$

Equation (1) is the national income identity with C representing total consumption including imports, I autonomous net investment, X exports, and M_a autonomous imports, assumed to be entirely consumer goods.[2] Equation (2) is the consumption function which we assume linear and in which C_a is the autonomous part of consumption and c the total marginal propensity to consume. In the absence of

[1] Our approach is short run in the sense that we make no attempt to handle problems arising from trade imbalances. In effect, we assume that an equilibrium level of income is reached when $M + S = X + I$ where M is the level of imports, S savings, X exports, and I investment. Over the longer run equilibrium would require that $M = X$ unless autonomous capital flows of proper amount and direction occur. Otherwise, the accumulation (decumulation) of foreign-exchange balances by the surplus (deficit) nation would force a change in the exchange rate or the imposition of direct controls (by the deficit nation) which would tend to equate M and X.

[2] By exports and imports we denote total receipts and expenditures, respectively, on current account, including unilateral transfers. Actually, imports may include not only consumer goods but investment goods, goods which are later re-exported and goods purchased for government use. The point of this article would not be invalidated by disaggregating imports by class of buyer; we are only concerned to indicate the impact of imports on domestic consumption and savings, respectively, and it does not matter that we concentrate imports in consumption.

The analysis could easily be generalized to include induced investment and induced government expenditures. In this event, we would substitute a marginal propensity to spend for our marginal propensity to consume.

induced imports, $c = c_d$, the marginal propensity to consume domestic goods. From (1) and (2), then, the change in national income produced by changes in autonomous factors is given by the following expression:

$$(3) \qquad \Delta Y = \frac{\Delta C_a + \Delta I + \Delta X - \Delta M_a}{1 - c}.$$

This formulation implies that fluctuations in trade affect national income via the current account surplus (or deficit) multiplied by the usual Keynesian multiplier.[3] If we abstract from changes in autonomous consumption and investment, that is $\Delta C_a = 0$ and $\Delta I = 0$, (3) becomes,

$$(4) \qquad \Delta Y = \frac{\Delta X - \Delta M_a}{1 - c}.$$

An alternative derivation of (4) may be obtained by summing the following two series:

$$(5) \qquad \Delta X + c\Delta X + c^2\Delta X + c^3\Delta X + \cdots = \frac{\Delta X}{1 - c}.$$

and

$$(6) \qquad -\Delta M_a - c\Delta M_a - c^2\Delta M_a - c^3\Delta M_a - \cdots = -\frac{\Delta M_a}{1 - c}.$$

These "traditional" derivations of the foreign-trade multiplier involve several implicit or hidden assumptions which seem to have gone unnoticed in much of the literature on the foreign-trade multiplier. With respect to exports, it is assumed that all exports are sales of currently produced goods and services and are therefore income-creating. This is not the case, however. First, exports usually include some sales on capital account. These would alter the exporter's portfolio of assets but not his income.

Second, the exports of most nations contain an element of imports. This takes two forms: raw materials or finished goods which are imported only to be transshipped in much the same form; and the case where raw materials are imported to be processed and exported as semifabricated or final products. The former is the case of the entrepôt nation. Great Britain and Japan are outstanding examples of the latter, as well as of the former. In fact, in the prewar period British re-exports of imported goods falling in the first category alone, amounted to some 10 to 15 per cent of exports and 5 to 7 per cent of total receipts on current account.

[3] This is a modification of J. J. Pclak's equation (1) in his "The Foreign Trade Multiplier," *Am. Econ. Rev.*, Dec. 1947, XXXVII, 892. This article, Gottfried Haberler's "Comment," and their joint "A Restatement" in the same issue of the *Review* constitute one of the most important sets of contributions in this field.

Third, receipts of immigrants' remittances, gifts, and reparations from abroad,[4] do not arise directly out of the production process of a nation and are not income-creating until they have been disposed of.[5] In the process of being spent, leakages occur in the form of savings.[6] In the present context imports are considered wholly autonomous; therefore an import leakage does not exist. In a model with induced imports and a government sector, there would be import and tax leakages in addition to the savings leakage.[7]

In the case of gifts such as unilateral government aid, the multiplier effect is likely to be very small or zero. The recipients of such aid typically maintain a level of imports which does not satisfy the average propensity to import. Hence, receipt of the gift is likely to involve an immediate and equal increase in the value of imports, with no stimulating effect on domestic national income. It is conceivable under these circumstances that a part of the increment in imports may substitute for domestically produced goods. Where this happens, the multiplier effect will be negative.

The transfer receipts discussed above are not unimportant in the balance of payments of many nations. Israel, for example, in 1951 showed immigrants' transfers and other remittances of 42.3 million

[4] Such unilateral transfers today are often not placed in the current account of the balance of payments but appear under special headings. The change in classification should not blind us to the income-generating or -destroying effects of such transfers.

[5] C. P. Kindleberger (*International Economics* [Homewood, Ill., 1953], pp. 341 ff.) argues that investment income from abroad does not generate income until it is spent and therefore suggests treatment similar to that which we suggest for immigrant remittances, etc. He says: ". . . exports generate more national income than investment income [does] in the first round. Exports require the expenditure of money for goods and services before goods are available for shipment overseas, while investment income requires no spending in advance of the foreign transactions." We believe that since interest and profits on capital invested abroad are considered to be part of national income (like interest and profits from domestic investment), the receipt from abroad of investment income must be considered in its entirety as income-generating. We admit that the justification for our position may be more conventional than "economic" and that the true impact of investment income from abroad may be more realistically estimated by classifying it with immigrants' remittances, etc. In this case, the export leakage parameter, $1 - t$ (below), would be increased.

[6] We have assumed that the remittance is made in the grantee's currency. If this were not the case, it would be necessary for the grantee to find a buyer for his foreign currency, a buyer (private or public) who is willing to hold idle balances of foreign currency since imports are autonomously given.

[7] Although we will not consider the capital account here, it should be noted that the usual assumption is that the purchase of a stock or bond in this country by a foreigner affects the portfolio and liquidity but not the income or consumption of the domestic seller. This is not, of course, always the case and domestic spending may be altered with an effect on income. Furthermore, there may be an effect on income through changes in the interest rate (see reference to S. C. Tsiang's article in fn. 12 below). Finally, if the foreign investor puts his money directly in new plant and equipment, this would be equivalent to a commodity export, though it would show up as part of domestic investment in the national income accounts.

Israeli pounds in comparison with exports of 16.7 million.[8] For Ireland, immigrants' remittances were 9.4 and 10.1 million Irish pounds in 1946 and 1951, respectively, in comparison with exports of 39.0 and 81.4 million pounds.[9] It is well known that unilateral government aid has loomed very large in the balance-of-payments receipts of many nations.

In our present model, no adjustment need be made for the second factor mentioned. We have avoided the problem of having to take account of the import content of exports by assuming that all imports are destined for domestic household consumption. Even without this assumption, no special problem arises in (3) and (4) above since goods which are re-exported exert a negative effect in the multiplicand as part of autonomous imports. The one instance when exports must be explicitly modified for import-content is in that formulation of the multiplier (below) in which imports are considered to be a function of income with no autonomous element. And here it would probably be more appropriate to introduce autonomous imports into the model than to modify exports. Receipts from unilateral transfers also need not be adjusted for import leakages. They must, however, be adjusted for the savings leakage.

We divide changes in exports into $e\Delta X$, $f\Delta X$, and $g\Delta X$ where $e\Delta X$ is immigrants' remittances, $f\Delta X$ is unilateral government transfers, and $g\Delta X$ the remaining receipts on current account; $e + f + g = 1$. Let u represent the fraction of immigrants' remittances which is spent on domestic goods in the first round. Let v represent the proportion of unilateral government aid which is spent on domestic goods and imports, some leakage occurring in the event that the entire amount of aid is not spent.[10] Let w represent the fraction of $g\Delta X$ which is income-creating, some leakage occurring due to sales on capital account.

Now if we define $t = eu + fv + gw$ which is normally less than 1 and which is taken to be the proportion of the change in exports which is income-creating, the effect of changes in exports on national income is then modified as follows:

$$(7) \qquad \Delta Y = t\Delta X + ct\Delta X + c^2 t\Delta X + \cdots = \frac{t\Delta X}{1 - c}.$$

With regard to imports, it is implicitly assumed in the "traditional" multiplier derivations that the elasticity of substitution between imports

[8] International Monetary Fund, *Balance of Payments Yearbook*, Vol. IV (1950-51), p. 172.

[9] *Ibid.*, p. 157.

[10] The imports are not viewed as a leakage from unilateral government aid since the negative income effect produced by them is accounted for in the multiplicand as an autonomous increase in imports.

and consumption of domestically produced goods and services is infinite, that is, any increase in autonomous imports is assumed to be accompanied by an equal downward shift in the propensity to consume domestically produced goods and services. It is only under this assumption that one is able to assume as M_a changes that $\Delta C_a = 0$ (an unchanged intercept of the consumption function) in the derivation of (4) above. The implicit assumption regarding substitution, an assumption which is highly restrictive and unrealistic, is also apparent in (6). It seems more appropriate to assume that an autonomous increase in imports may be accompanied by a decrease in expenditures on domestically produced goods *and* a diminution in savings. This possibility can be incorporated explicitly into our analysis by reformulating (7) in the following manner:

$$(8) \qquad -p\Delta M_a - pc\Delta M_a - pc^2\Delta M_a - \cdots = \frac{-p\Delta M_a}{1-c}$$

in which p is the proportion of imports which substitutes for domestic consumption. The "traditional" import effect on national income, shown in (6), is obtained from (8) under the very special condition that $p = 1$, that is when an increase (decrease) in autonomous imports is accompanied by an equal decrease (increase) in consumption of domestically produced goods. The value of p is likely to approach unity when the change in imports results from a change in the relative prices of identical commodities or close substitutes produced both at home and abroad.

At the other extreme, if imports are financed entirely at the expense of savings ($p = 0$), changes in imports have no effect on national income.[11] In this instance, the import leakage simply substitutes for the saving leakage.[12] A value of p which approaches zero might have

[11] Two writers who appear to have made this assumption are L. A. Metzler, "A Multiple-Region Theory of Income and Trade," *Econometrica*, Oct. 1950, 18, 329-54; and Hans Brems, "A Generalization of the Foreign Trade Multiplier," *Kyklos*, 1956, IX (4), 475-82. Brems' model is discussed below. Both of these writers assume that imports are a function of income.

[12] A possible difference between the two types of leakage is noted by S. C. Tsiang in a paper which includes, in the multiplier formulation, monetary factors such as changes in the quantity of money and in the interest rate ("Liquidity Preference and Loanable Funds Theories," *Am. Econ. Rev.*, Sept. 1956, XLVI, 539-64). He states (p. 562, fn. 37) that ". . . the so-called 'leakage' due to imports would radically differ from the 'leakage' due to savings; for additional imports under an inflexible exchange rate system would automatically lead to an equal reduction in reserve money whereas induced savings would not."

Tsiang's example as it is stated would seem to apply only to the extreme case. Reserve money would not flow out, for example, where importers own and draw down balances held in foreign banks; or where foreign exporters are content to accept payment in the form of building up bank balances in the importing country.

prevailed in many European nations after the war. Consider a nation with repressed inflation, demand backlogs, and import controls. Remove the import controls and the chances are that consumption will rise to almost the full extent of the increase in imports.

Finally, it might be assumed that the saving-spending relationship in the economy is unaffected by changes in imports and that any increase in imports comes out of spending and saving in the old proportions. Another way of stating this is that $p = c$.[13] Under this condition, the effect on income of a change in autonomous imports would be:

$$(9) \qquad \Delta Y = \frac{-c\Delta M_a}{1 - c}.$$

There seems to be no particular justification for assuming that (9) represents the general case.

In the light of the above considerations we introduce

$$(10) \qquad C_d = cY + C_{ad} - pM_a,$$

a "domestic" consumption functioning incorporating allowance for an import substitution effect and in which C_{ad} is the autonomous part of domestic consumption. Adding M_a to both sides of this domestic consumption function yields the following function to explain total consumption:

$$(11) \qquad C = C_a + cY$$

with:

$$(12) \qquad C_a = C_{ad} + (1 - p)M_a.$$

These relations plus the income identity,

$$(13) \qquad Y = C + I + tX - Ma,$$

represent a revision of the traditional model presented above.[14] Changes in income are related to changes in autonomous factors in the present model by:

$$(14) \qquad \Delta Y = \frac{\Delta C_{ad} + \Delta I + t\Delta X - p\Delta M_a}{1 - c}.$$

Equation (14) differs from (3) not only by the inclusion of the parameters t and p, but by the replacement in the multiplicand of ΔC_a by ΔC_{ad}. Thus, while deriving (4) from (3) involved an un-

[13] This is analogous to the balanced-budget treatment of taxes (see section IV below). It was this formulation which originally suggested to us the use of our parameter, p.

[14] The above model is used for comparability with our earlier one. A more straightforward approach would have been to combine equation (10) with $Y = C_d + I + tX$.

realistic hidden assumption $(p = 1)$, here no such assumption is involved and changes in income are related to changes in imports and exports, with $\Delta C_{ad} = \Delta I = 0$, by:

$$(15) \qquad\qquad \Delta Y = \frac{t\Delta X - p\Delta M_a}{1 - c}.$$

If imports are partially induced and partially autonomous rather than wholly autonomous,[15] that is:

$$(16) \qquad\qquad M = qY + M_a$$

equations (14) and (15) are changed slightly since now the total marginal propensity to consume is the sum of the marginal propensities to consume domestic and imported goods, i.e., $c = c_d + q$, whereas formerly, with no induced imports, c equalled c_d. With (16) incorporated into our model, the result paralleling (15) is:[16]

$$(17) \qquad\qquad \Delta Y = \frac{t\Delta X - p\Delta M_a}{1 - c + q} = \frac{t\Delta X - p\Delta M_a}{1 - c_d}.$$

A foreign-trade multiplier with imports entirely a function of income and only exports in the multiplicand has been popular with many economists in the past and is, in fact, still the typical textbook formulation.[17] The typical expression derived by these writers is:

$$(18) \qquad\qquad \Delta Y = \frac{1}{1 - c + q} \Delta X.$$

[15] It might appear at first glance that there is an asymmetry in having a substitution of imports for domestic consumption but no substitution of domestic consumption for imports. The latter might be introduced into (16) as follows: $M = qY + M_a - nC_{ad}$, where n is the proportion of domestic autonomous consumption which substitutes for imports. However, in the model presented above it does not matter whether changes in C_{ad} come out of imports or savings since both are leakages. On the other hand, it obviously does matter whether changes in imports come out of savings or consumption since only savings are a leakage. Further, we limit ourselves to a consideration of just the substitutions among *autonomous* imports, consumption of domestically produced goods, and savings. An analogous treatment of induced imports is possible but would involve a reinterpretation of traditional parameters. The value of such a reinterpretation has not as yet been ascertained by the authors.

[16] The conditions under which increases and decreases in Y result from changes in trade in this formulation are examined in section II below following our discussion of the balanced-trade multiplier.

[17] For example, Colin Clark, *The National Income of Australia* (Sydney, 1938), p. 102; P. T. Ellsworth, *The International Economy* (New York, 1952), pp. 342 ff.; D. A. Snider, *Introduction to International Economics* (Homewood, Ill., 1954), pp. 223 ff.; Fritz Machlup, *International Trade and the National Income Multiplier* (Philadelphia, 1950), *passim;* T. Scitovsky, "The Theory of the Balance of Payments and the Problem of a Common European Currency," *Kyklos,* 1957, X (1), 22.

In comparison, (17) with $\Delta M_a = 0$ is:

(19) $$\Delta Y = \frac{l}{1 - c + q} \Delta X.$$

Clearly the income-creating effects of an increase in exports is greater in the traditional formulation than in ours. Since t is normally less than one, the numerator of (19) is less than that of (18). Furthermore, since no autonomous imports appear in the multiplicand, t must be reduced in value to allow for the import-content of exports.
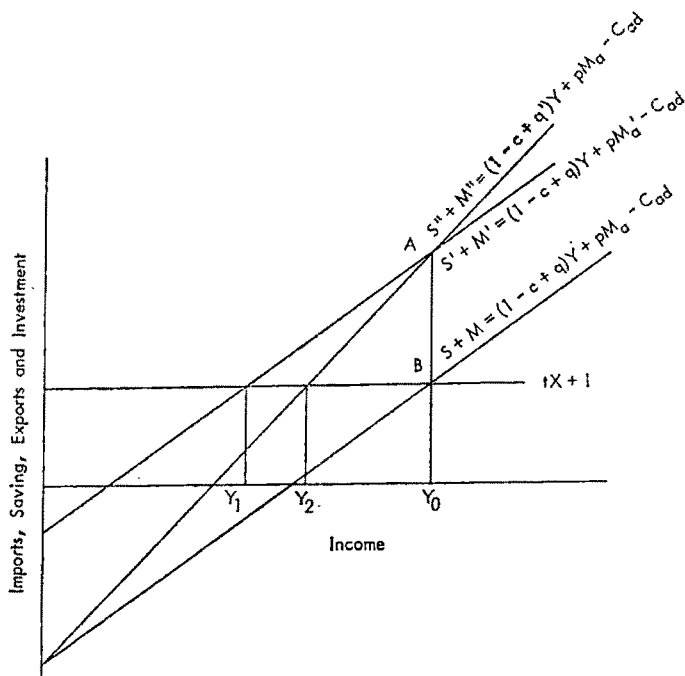
FIGURE 1

Equal increments of autonomous imports occurring in different ways may produce different changes in the equilibrium level of income. Consider, for example, an autonomous increase in imports as the result of an upward shift of the import function with slope unchanged, such as might be produced by a general lowering of tariffs, and an equal autonomous increase in imports resulting from an increase in the marginal propensity to import accompanied by an equal decrease in the marginal propensity to consume domestic goods, the intercept of the import function unchanged. The effects of these two changes on income are depicted in Figure 1 wherein AB represents the initial equal increases in autonomous imports, i.e., $AB = p(M_a' - M_a) = Y_0(q' - q)$. In the case of the parallel shift of the

import function, income decreases from Y_0 to Y_1 while in the case of the increase in the marginal propensity to import (with the total marginal propensity to consume assumed unchanged), the change is from Y_0 to Y_2. The smaller change in the second instance is explained by the fact that as q increases under the condition stated above the value of the multiplier decreases, whereas in the former instance the value of the multiplier does not change.

II. *The Balanced-Trade Multiplier*

Some of the issues discussed in Section I above have been considered in the literature in connection with the balanced-trade multiplier. We refer in particular to the brilliant article by Wolfgang Stolper[18] as well as those by Polak and Haberler mentioned earlier. Unfortunately, none of these writers successfully incorporates his insights into the traditional multiplier formulation which is essentially algebraic.[19] Since the literary tradition in this area is weak, the insights tend to be forgotten and old errors and misinterpretations tend to persist.

First, let it be clear that when we speak of a balanced-trade multiplier, we refer to the effect on national income of equal increments of X and M_a where M_a can equal either M or $M - qY$. Trade need not be balanced to begin with or after the multiplier has worked itself out, although what we have to say applies to balanced trade as a special case.[20] In fact, if any part of imports is induced, then deficit (surplus) on current account will increase (decrease) via the operation of the multiplier as income increases. Induced imports cannot be considered part of the multiplicand, of course, as a matter of definition.[21]

The traditional view of the effect on national income of a balanced,

[18] Wolfgang Stolper, "The Volume of Foreign Trade and the Level of Income," *Quart. Jour. Econ.*, Feb. 1947, LXI, 285-310.

[19] A partial exception to this statement is J. J. Polak's *An International Economic System* (Chicago, 1953), p. 37, whose multiplier formulation came to our attention after this paper had been drafted. Like many writers, Polak considers imports to be a function of relative prices as well as of income. Unlike the others, however, he has a factor, similar to our p, to take account of the degree of substitution between imports and consumption as a result of relative price changes of domestic goods and imports. Our parameter p is completely general, however, and based on the assumption that any change in imports (whether due to changes in relative prices, tastes, removal or introduction of import controls, exhaustion of resources, changes in technology, or what have you) will impinge on both savings and domestic consumption.

[20] In the balanced-budget case, this is equivalent to asking: what is the effect of increasing receipts and expenditures simultaneously by $10 billion starting from initial surplus or deficit? Those who speak of the balanced-budget theorem in terms of total budgetary receipts and expenditures always assume that there is no government sector when an equal increase in receipts and expenditures is introduced. Put this way, the total budget is identical with the increment. However, it is the equality of increments which is crucial to the analysis, not the balancing of totals.

[21] We admit the difficulty of separating statistically induced from autonomous changes in imports.

increase in trade is based upon applications of equation (4).[22] From this equation it would be deduced that a balanced increase or decrease in trade would leave income unaffected. Haberler, for example, argued 17 years ago that only a trade surplus is stimulating and that parallel increases in exports and imports are "neutral."[23] In 1947, in their "A Restatement," Polak and Haberler take an only slightly different stand substituting for the word "neutral," the phrase "nothing can be stated in general" and qualifying the phrase with Stolper's insights.[24]

Stolper's argument is summed up in his own words as follows:[25]

> . . . A simultaneous increase in imports and exports will have an expansionary effect if it is not offset by downward changes in the average propensity to consume domestic goods. . . . The best way to describe the effects of trade balances is by means of the marginal propensity to import. The best way of describing the effect of the volume of trade as distinct from the trade balance is by means of the average propensity to spend. For a discussion of the full effects of foreign trade on national income, both average and marginal propensities have to be considered, just as in the ordinary multiplier discussion, not only the slope of the consumption function is important but also its height and shifts . . .

Stolper believes that balanced increases in trade will cause the average propensity to spend to shift upward particularly in cases where it had previously been artificially depressed through exchange controls, quotas, and other such devices.[26]

The argument is illustrated by assuming that a tariff has been levied which leads to a balanced decrease of both imports and exports.[27] The balanced change can have the following effects: (1) Increases income if the nation levying the tariff substitutes domestic goods for foreign goods fully and substitutes consumption of domestic goods for savings. (2) Leaves income unchanged if domestic goods are substituted for foreign goods but there is no substitution between savings and foreign and domestic goods. (3) Decreases income if domestic goods are not substituted for foreign goods completely but instead savings are substituted in part for imports.

[22] Where there are induced imports, the multiplier would be:

$$\frac{1}{1 - c + q}.$$

[23] Gottfried Haberler, *Prosperity and Depression* (Geneva, 1941), p. 468. It should be pointed out that Haberler qualifies this statement in the text.

[24] Haberler and Polak, *op. cit.*, p. 907.

[25] Stolper, *op. cit.*, p. 309.

[26] *Ibid.*, p. 288.

[27] *Ibid.*, pp. 297 ff. Stolper also considers the effect of an export surplus resulting from the levy of a tariff. See below, p. 84.

These three cases can be conveniently analyzed in terms of a modified equation (14) eliminating from the numerator the parameter t which Stolper doesn't consider. Since both exports and imports decline, the numerator (multiplicand) changes sign becoming: $p\Delta M_a - \Delta X$. Let us look first at case (2) in which there is perfect substitutability between domestic and imported goods. In our terms this is equivalent to saying that $p = 1$, and the multiplicand is 0. Case (3) is the most likely one. Domestic consumption doesn't rise to the full extent of the decline in imports, hence savings rise.[28] Here $p < 1$, the multiplicand is negative and income falls. Case (1) assumes that $p > 1$ because not only does the increase in consumption of domestic goods substitute completely for the drop in imports, it rises at the expense of savings as well. Under these unrealistic conditions, income rises as a result of a decline in trade. Stolper considers this case improbable and in fact it is clearly against his own view (see above, p. 82) that the average propensity to consume is expected to rise, not decline, as a result of a balanced rise in trade.

Stolper sums up the results in these three cases (p. 298) as depending on the relative extent of changes in the average propensity to consume and level of exports. It is obvious that the shift in the average propensity to consume referred to is accounted for by the introduction of the parameter p. This may be easier to visualize from the consumption equations (10) and (12) above.

The impact of a balanced change in trade can be analyzed more generally as follows: since the increments of X and M are assumed equal, the change in income where imports are wholly autonomous is indicated by the equation

$$(20) \qquad \Delta Y = \Delta X \frac{(t - p)}{1 - c}$$

and where imports are partly autonomous and partly induced,

$$(21) \qquad \Delta Y = \Delta X \frac{(t - p)}{1 - c + q}.$$

In terms of (20),[29] a balanced-trade multiplier of unity corresponding to the traditional balanced-budget multiplier, occurs under the special conditions that $t - p = 1 - c$. If there were no export leakage $(t = 1)$, then the unit multiplier would result if $p = c$. When $t < 1$, c must be correspondingly greater than p for the multiplier to be 1.

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[28] Stolper makes the interesting point that the rise in savings due to the decline in imports may be offset in part by the decline in real savings due to the decline in income (cf. pp. 298-99).

[29] Modification of this paragraph for the formulation of (21) is left to the reader.

A multiplier of greater than unity results if $t - p > 1 - c$, and less than unity if $t - p < 1 - c$. If $p > t$, the multiplier is negative; if $p = t$, the multiplier is zero—this being the "traditional" assumption.

Haberler and Polak in their series of three articles, while holding to indeterminacy in the balanced-trade case, appear to maintain an unambiguous attitude toward unbalanced changes in trade. There is nothing in their presentation to indicate that they have any doubts but that if the increase in exports is greater than the increase in imports, income will rise, and that if the reverse is true, income will fall. If the "special circumstances" argument which renders the balanced-trade case indeterminate is well taken, and we believe it is, then these same circumstances must render the cases of unbalanced changes also indeterminate,[30] though of course the larger the export surplus (import surplus) the greater the possibility of an increase (decrease) in income as a result.

Stolper admits the indeterminacy of the unbalanced-trade multiplier and presents several illustrations. He says, for example (p. 296), that "If the export surplus comes about through a fall in imports, with exports unchanged, it will have a stimulating effect only if the average propensity to consume domestic goods is increased." In terms of equations (15) and (17) Stolper can be interpreted as saying that there has been a decline in ΔM_a, ΔX remaining unchanged (hence in the multiplicand we have: $+ p\Delta M_a$) and that this will have a stimulating effect on income if $p > 0$. In his second case (p. 297) he states that "If the export surplus is due to the fact that imports are falling faster than exports, it will have an expansionary effect only if the average propensity to consume domestic goods rises faster than exports fall. . . ." This can be interpreted to mean that an expansionary effect will occur if $p\Delta M_a > \Delta X$. This is obvious from (15) which, after changing signs for the decline in trade, has a multiplicand, $p\Delta M_a - \Delta X$. Finally Stolper argues that "Only if the export surplus arises because exports increase with imports increasing less or remaining constant is it unnecessary for the average propensity to consume domestic goods to increase in order to achieve an expansionary effect." In other words, if $\Delta X > \Delta M_a$, there will be an expansion even if $p = 1$. In all of these examples, Stolper's results would have to be modified for export leakages. Thus in the last case mentioned, for an expansionary effect to result, it would not be sufficient for $\Delta X > \Delta M_a$, rather it would be necessary for

$$\frac{t}{p} > \frac{\Delta M_a}{\Delta X}.$$

[30] This point was made to one of the authors in 1948 by Mathilda Holzman.

From equation (15) or (17), the effects on Y of unbalanced changes in trade can be generalized as in Table I.

TABLE I

Change in Trade	Direction of Change in Income		
	Increase	No Change	Decrease
Expansion	$\dfrac{t}{p} > \dfrac{\Delta M_a}{\Delta X}$	$\dfrac{t}{p} = \dfrac{\Delta M_a}{\Delta X}$	$\dfrac{t}{p} < \dfrac{\Delta M_a}{\Delta X}$
Contraction	$\dfrac{t}{p} < \dfrac{\Delta M_a}{\Delta X}$	$\dfrac{t}{p} = \dfrac{\Delta M_a}{\Delta X}$	$\dfrac{t}{p} > \dfrac{\Delta M_a}{\Delta X}$

The direction of effects of balanced changes in trade derive as a special case when the fraction on the right side of each of the above equations (and inequalities) reduces to unity ($\Delta M_a = \Delta X$).

While our multiplier formulation clarifies a number of previous studies in addition to Stolper's in the international trade field, we shall confine ourselves to one more—Svend Laursen and Lloyd Metzler's well-known "Flexible Exchange Rates and the Theory of Employment."[31] In the traditional theory it had been argued that flexible exchange rates insulated a nation from foreign cyclical fluctuations. The argument ran that since $Y = C + I + X - M$, and since flexible exchange rates keep $X = M$, then in effect $Y = C + I$, as X and M cancel out. Thus, under flexible exchange rates, domestic income is not affected by foreign fluctuations operating through the trade balance. Laursen and Metzler pointed out that this traditional argument assumes implicitly that changes in imports due to changes in the exchange rate are always exactly counterbalanced by changes in the reverse direction in domestic spending; and that this is not so because of the price and income effects associated with changes in the terms of trade.[32]

These effects are conveniently summarized by the introduction of our parameter p; recognition of export leakages represented by t would have further refined the analysis. Our formulation indicates that a balanced-trade policy implemented by flexible exchange rates or any other technique will insulate the domestic economy only if $t = p$. If $t > p$, balanced trade will be employment-creating, if $t < p$, deflationary.

[31] *Rev. Econ. Stat.*, Nov. 1950, XXXII, 281-99.

[32] As we have already seen, these points had already been made in another context by Stolper, and the authors cite him. Their need to repeat Stolper's arguments is due, in our opinion, to the fact that the multiplier formula had not been formally modified to incorporate them.

Under the traditional formulation (or whenever $t = p$), the same policy —a balanced-trade policy—will be effective both in maintaining international reserves at a stable level and in insulating the economy from income fluctuations abroad. Clearly, only the first of these objectives is achieved when $t \neq p$.[33]

III. *The Repercussion Multiplier*

In this section we shall reformulate the repercussion multiplier[34] to incorporate explicitly export leakages and substitution effects. We consider the following two-country model:

	Country 1		Country 2
(22)	$Y_1 = C_1 + I_1 + t_1 X_1 - M_1$	(22')	$Y_2 = C_2 + I_2 + t_2 X_2 - M_2$
(23)	$C_1 = A_1 + c_1 Y_1 + (1 - p_1) m_1$	(23')	$C_2 = A_2 + c_2 Y_2 + (1 - p_2) m_2$
(24)	$M_1 = q_1 Y_1 + m_1$	(24')	$M_2 = q_2 Y_2 + m_2$
(25)	$X_1 = M_2$	(25')	$X_2 = M_1$

All symbols are defined as above with the exception of A_1 and A_2 which represent autonomous elements in consumption of domestically produced goods, (heretofore C_{ad}) and m_1 and m_2 the autonomous parts of imports, (heretofore M_a) in countries 1 and 2 respectively. Investment in both countries is assumed wholly autonomous. Equations (25) and (25') state that the exports of one country are the other country's imports.

To consider the repercussions of a change in country 2's autonomous imports on country 1's income, it is necessary to differentiate the following expression for Y_1, obtained from equations (22) to (25) and (22') and (25'), with respect to m_2:

$$(26) \quad Y_1 = \frac{(A_1 + I_1 - p_1 m_1 + t_1 m_2)(1 - c_2 + q_2) + t_1 q_2 (A_2 + I_2 - p_2 m_2 + t_2 m_1)}{(1 - c_1 + q_1)(1 - c_2 + q_2) - t_1 t_2 q_1 q_2}.$$

Differentiation with respect to m_2 yields the repercussion multiplier:[35]

$$(27) \quad \frac{dY_1}{dm_2} = \frac{dY_1}{dX_1} = \frac{t_1 [1 - c_2 + q_2 (1 - p_2)]}{(1 - c_1 + q_1)(1 - c_2 + q_2) - t_1 t_2 q_1 q_2}.$$

It will be noted that repercussions of an autonomous change in country 2's imports (and thus country 1's exports) will be greater the larger

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[33] We abstract, as do Laursen and Metzler, from capital flows. We should also add that our summary of their position does not do justice to the complexity of their argument. See also the criticism of their paper by W. H. White and their "Reply" both in *Rev. Econ. Stat.*, May 1954, XXXVI, 225-29.

[34] For prior discussion of the repercussion multiplier, see C. P. Kindleberger, *op. cit.*, pp. 171-180, and the sources cited therein.

[35] Note that the denominators of (26) and (27) are positive since $1 - c_i$ is the marginal propensity to save which we assume larger than zero. Therefore

$$1 - c_i + q_i > q_i \geq t_i q_i, \text{ for } i = 1, 2.$$

are the values of the parameters t_1 and t_2 and the smaller the values of p_1 and p_2. That is, the less important are export leakages and the substitution of imports for domestically produced goods, the greater will be the repercussion multiplier.

Our formulation of the repercussion multiplier "reduces" to that derived by Kindleberger (*op. cit.*, p. 180) if it be assumed that there are no export leakages ($t_1 = t_2 = 1$) and that there is complete substitution of imports for domestically produced goods ($p_1 = p_2 = 1$), two assumptions which we regard as highly tenuous. On the other hand, if it be assumed that there are no export leakages and no substitution effects ($p_1 = p_2 = 0$), our model becomes identical to one considered by Brems[36] (except that Brems does not include autonomous elements in consumption and imports).

IV. *The Balanced-Budget Multiplier*

Baumol and Peston in their recent paper on the balanced-budget multiplier[37] give the impression that there is considerable difference between the balanced-budget and balanced-trade multipliers. They state:

> . . . It might be thought (as we did) that an equal change in imports and exports will also have a unit multiplier effect. But this is not so because, while a governmental balanced budget purchase will (at least initially) reduce the excess supply of goods available to the private sector, imports will increase the supplies by just the amount they are decreased by the change in exports.[38]

There are differences between the two multipliers as we shall indicate below, but these are not contained in the above statement which, in our opinion, is in error. The unit balanced-budget multiplier is derived as follows. Assume equal and simultaneous increases in government expenditure, ΔG, and taxes, ΔT. The multiplier effects of ΔG and ΔT, respectively, are indicated as the sums of the following series:

$$(28) \qquad \Delta G + c\Delta G + c^2\Delta G + c^3\Delta G + \cdots = \frac{\Delta G}{1 - c}$$

$$(29) \qquad -c\Delta T - c^2\Delta T - c^3\Delta T - \cdots = -\frac{\Delta T}{1 - c} + \Delta T.$$

Substituting ΔG for ΔT (since they are assumed equal) in (29) and subtracting gives:

$$(30) \qquad \Delta Y = \Delta G.$$

[36] Brems, *op. cit.*, recognizes that the omission of substitution effects is a deficiency of his model.

[37] W. J. Baumol and M. H. Peston, "More on the Multiplier Effects of a Balanced Budget," *Am. Econ. Rev.*, March 1955, XLV, 140–48.

[38] *Ibid.*, p. 141, fn. 4.

Now the unit multiplier results from this formulation because it is assumed that ΔG represents a 100 per cent increase in effective demand in the first round whereas taxes do not reduce effective demand in the first round to the extent that they come out of savings but only when they substitute for consumption. Thus, according to Baumol and Peston, the excess supply of goods available to the private sector is ("at least initially") reduced. A clearer way of stating this would be: there is a net increase in demand relative to supply due to the fact that ΔG exceeds $c\Delta T$ and this creates a temporary shortage of goods, and an increase in national income (equal to ΔG).

Turning to the balanced-trade multiplier, let us first reinterpret Baumol and Peston's statement that ". . . imports will increase supplies by just the amount they are decreased by the change in exports . . ." to read "imports will reduce demand by just the amount it is increased by the change in exports."[39] It now becomes clear that they are thinking of the foreign-trade multiplier in terms of the "traditional" formulation of equation (4) above. In Baumol and Peston's terms, ΔX involves an increase in demand[40] just like ΔG but whereas ΔT is not a net reduction from ΔG, ΔM is assumed to be a net reduction from ΔX. As we have shown, however, an increase in M will not usually be matched by a 100 per cent reduction of domestic consumption expenditures but will impinge on savings and other leakages (taxes) as well. In the special case where $p = c$, the balanced-trade multiplier will be unity just like the "traditional" balanced-budget multiplier. Thus there is nothing inherent in the nature of the balanced-trade multiplier problem which precludes a multiplier value of one; it does, however, represent the same sort of very special case as that deduced for the balanced-budget multiplier by Baumol and Peston.

It is apparent then that the balanced-budget and balanced-trade multipliers differ only in that they are applied to different kinds of equal changes and in that they involve different leakage parameters. For example, in comparing government expenditures and exports, it seems highly likely that a greater proportion of the former than of the latter

[39] A more accurate, but ponderous, way of restating this is: "an increase in imports will reduce the demand for domestic goods by an amount equal to the increase in exports (which increase is assumed equal to the increase in imports)."

[40] It should be noted that Baumol and Peston define E as the net income-creating government expenditures exclusive of transfers, purchases on capital account, etc. Our G can be transformed into their E by (in their terms) multiplying it by $(1 - k)$ where k is "the proportion of tax not being added to income-creating expenditures" (*op. cit.*, p. 145). It would be more accurate to view both ΔX and ΔG as net of leakages, *i.e.*, as $t\Delta X$ and $(1 - k) \cdot \Delta G$ in our terms; in their brief footnote, Baumol and Peston do not extend the concept of government-expenditure leakages to exports.

will involve a leakage in the form of sales on capital account, *e.g.*, land and buildings.[41]

Baumol and Peston define their G (or E) as "income-creating expenditures" and exclude from it all leakages including sales on capital account and government transfer payments. While the exclusion of sales on capital account would appear to be legitimate (in the usual multiplier formulation which ignores monetary effects),[42] government transfer payments should be included in a manner similar to our treatment of immigrants' remittances. That is to say, transfer payments, while not constituting 100 per cent effective demand as do government purchases of final output and services, do re-enter the income stream after the first round, modified for leakages in the form of savings, imports, and so forth. Had Baumol and Peston deducted payments from taxes as well as expenditures, their treatment on the expenditure side would be legitimate—but this they do not do. Even so, it would probably be better to include the amount of the transfer expenditure "gross" in both taxes and expenditures, since the propensity to spend of recipients of transfers probably differs from that of payers of transfers (the average taxpayer).

This brings us to Baumol and Peston's treatment of taxes. They correctly point out that the extent to which taxes will reduce consumption depends on the type of tax levied. For example, a tax on consumption ". . . may encourage saving at the expense of consumption . . ."[43] whereas taxes on savings or capital levies will encourage consumption at the expense of savings ". . . as people rush out to increase their consumption in an effort to avoid the taxes. . . ."[44] Other examples are given as well. Since the taxpaying class in the population differs from the class of consumers, the initial impact of the tax program on the spending-saving ratio will differ from subsequent effects. In a country like the United States where a highly progressive income tax predominates, the marginal propensity to consume of the average taxpayer is likely to be somewhat less than that of the average person (household) since only the latter category includes all those families with incomes too low to have to pay income taxes. It would seem more appropriate therefore, to treat taxes as we have treated imports, viz. assume that in the first round taxes reduce spending by some unspecified parameter, say p', rather than by c, and to replace (29) by:

$$(31) \qquad -p'\Delta T - p'c\Delta T - p'c^2\Delta T - p'c^3\Delta T \;\cdots\; = -\frac{p'\Delta T}{1-c}.$$

[41] *Ibid.*, pp. 143–144.

[42] Certainly Tsiang's analysis (see fn. 12) should be extended to cover the cases under discussion.

[43] *Op. cit.*, p. 143. [44] *Loc. cit.*

Letting k' be the expenditure leakage,[45] we have:

$$(1 - k')\Delta G + c(1 - k')\Delta G + c^2(1 - k')\Delta G + c^3(1 - k')\Delta G + \cdots$$

$$(32) \qquad\qquad\qquad\qquad\qquad\qquad = \frac{(1 - k')\Delta G}{1 - c}.$$

From these two equations we derive the change in income:[46]

$$(33) \qquad \Delta Y = \frac{(1 - k') - p'}{1 - c}\,\Delta G = \frac{1 - k' - p'}{s}\,\Delta G.$$

Our multiplier will tend to be greater than Baumol and Peston's for two reasons: on the expenditure side Baumol and Peston neglect the stimulating effect of transfer expenditures; on the tax side, they neglect the difference in marginal propensities to spend of taxpayers and the total population. Our multiplier,

$$\frac{1 - k' - p'}{s} = \frac{1 - (k' + p')}{1 - c}.$$

compares[47] with their $1 - k/s$.

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[45] Our k' would be less than Baumol and Peston's k because of their failure, as noted above, to take account of the stimulating effect of transfer payments in the second round. As in the case of t, we use k' to represent a combination of parameters.

[46] s represents all leakages other than those represented by k' and p'.

[47] It is interesting to compare the conditions under which the two multipliers give similar values:

Value of Multiplier	>1	$=1$	<1	$=0$	negative
Baumol and Peston	s negative	$k = 0$	$k < s$	$k = s$	$k > s$
	($i.e., c > 1$); $k \neq 0$				
Holzman and Zellner	$k' + p' < c$	$k' + p' = c$	$k' + p' > c$	$k' + p' = 1$	$k' + p' > 1$ and $0 < c < 1$; or $c > 1$ and $0 < k' + p' < 1$

The Baumol-Peston formulation tends to suggest multipliers of less than unity and perhaps even zero or negative. (Their statement, *op. cit.*, p. 146, that "multipliers considerably greater than unity . . . are by no means out of the question" would seem to be in error since a multiplier greater than one would involve a marginal propensity to spend on domestic goods of greater than unity.) This is due to their feeling that the marginal propensity to leak of the government (k) may be of the same order of magnitude or even greater than that of the population. A negative multiplier seems most unlikely in our formulation since neither condition is likely to be fulfilled: c is hardly likely to be greater than 1; further, p', the marginal propensity of taxpayers to reduce consumption, will be less than c, and k' will be less than Baumol and Peston's k because of their failure to take account of the stimulating effect of transfers in the second round. For similar reasons, multipliers of unity or greater than unity would not seem to be excludable a priori in our formulation; in theirs, a multiplier of 1 would seem to be the reasonable upper limit. It should be noted that in their reply to Hansen, the authors reformulate their multiplier in such a way that multipliers greater than 1 appear possible. ("Reply," *Am. Econ. Rev.*, March 1956, XLVI, 160-62.) But this is for a reason over and above those mentioned here.

A final point on taxes. It seems to us that taxes should not be considered to be only autonomously determined and that to the list of leakages (the propensities to "save, import, and purchase items not currently produced or replaced") there should be added the propensity.to pay taxes.[48]

In conclusion, we should like to point out that our criticisms of the Baumol and Peston article, while in our opinion worth taking into account in a theoretical formulation of the balanced-budget multiplier, do not in any way invalidate the central point of their article: that the actual multiplier effect of a balanced-budgetary change is not unity but depends on the relative receipts and expenditure leakages.

[48] *Cf.* A. H. Hansen, "Comment," *Am. Econ. Rev.*, March 1956, XLVI, 157-60.

THE CLAYTON ACT: SLEEPING GIANT OF ANTITRUST?

By ROBERT W. HARBESON*

> Thus, over 40 years after the enactment of the Clayton Act, it now be-
> comes apparent for the first time that Section 7 has been a sleeping
> giant all along. Every corporation which has acquired a stock interest
> in another corporation after the enactment of the Clayton Act in 1914,
> and which has had business dealings with that corporation is exposed,
> retroactively, to the bite of the newly discovered teeth of Section 7.[1]

The foregoing passage from the minority opinion of the United States
Supreme Court in *United States v. E. I. du Pont de Nemours and Com-
pany et al.*, handed down June 3, 1957, suggests the great legal and eco-
nomic significance which at least some members of the Court attach to
this decision. A similar view has been widely echoed in the business and
financial press. For example, *Fortune* comments editorially that the de-
cision is "potentially the most important antitrust development since
Justice Edward Douglass White enunciated the 'rule of reason' in the
Standard Oil Case in 1911."[2] The important word in this statement is
"potentially"; without minimizing the importance of the increased
scope which this decision gives to the antitrust laws it seems likely that
the unanswered questions which it raises and the uncertainties which
it creates may be of equal if not greater significance.

In the *du Pont* decision the Supreme Court held, 4 to 2, that the
purchase by du Pont in 1917-19 of a 23 per cent stock interest in Gen-
eral Motors violated Section 7 of the Clayton Act, in that through this
purchase "du Pont intended to obtain, and did obtain, an illegal prefer-
ence over its competitors in the sale to General Motors of its products,
and a further illegal preference in the development of chemical dis-
coveries made by General Motors."[3] The proceeding began in June
1949 as a civil action in the United States District Court for the North-
ern District of Illinois, the government charging that du Pont's stock
ownership in General Motors violated both Sections 1 and 2 of the
Sherman Act and Section 7 of the Clayton Act. The government's com-
plaint named, in addition to du Pont and General Motors, two holding
companies owning large blocks of du Pont and General Motors stock—

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[1] *U. S. v. E. I. du Pont de Nemours and Co. et al.*, 353 U.S. 586 (1957), at p. 611;
hereafter cited as *U.S. v. du Pont.*

[2] "Brennan on Bigness," *Fortune*, July 1957, LVI, 91.

[3] *U.S. v. du Pont*, p. 608.

Christiana Securities Company and Delaware Realty and Investment Corporation—and also the Wilmington Trust Company, United States Rubber Company, and members of the du Pont family.

Judge La Buy dismissed the government's complaint, holding that du Pont did not control General Motors, that there had been no limitation or restraint upon General Motors' freedom to deal with competitors of du Pont or to develop its chemical discoveries, and that there was no basis for a finding that there was or had been any reasonable probability of an illegal restraint within the meaning of the Clayton Act.[4] The government appealed despite the fact that, reportedly, government attorneys felt that the District Court's decision was "airtight."[5] However, in appealing, the government limited the complaint to du Pont, General Motors, and the two holding companies mentioned above. The appeal, like the original complaint, charged violations both of Sections 1 and 2 of the Sherman Act and Section 7 of the Clayton Act, but the latter apparently was included only as an afterthought or make-weight; according to Justice Burton the government referred to it only in the closing pages of its brief and for a few minutes in its oral argument. Nevertheless the majority of the Supreme Court rested its decision solely on the ground that a violation of Section 7 of the Clayton Act was involved.[6] The decision of the District Court was reversed and the case remanded to it for determination, after further hearing, of the equitable relief necessary and appropriate to eliminate the effects of the unlawful stock acquisition involved.

In reaching the foregoing conclusion the majority of the Supreme Court extended the scope of the Clayton Act in three ways. First, it was held that Section 7, even prior to the Celler amendments of 1950, covered vertical as well as horizontal acquisitions of stock; that is, acquisitions of stock of customer or supplier companies as well as stock of competitors. In the Court's words, "any acquisition by one corporation of all or any part of the stock of another corporation, competitor or not, is within the reach of the section whenever the reasonable likelihood appears that the acquisition will result in a restraint of commerce or in the creation of a monopoly of any line of commerce."[7] Prior to the present decision Section 7 in its original form had, except in one

[4] *U.S. v. E. I. du Pont de Nemours and Co. et al.*, 126 F. Supp. 235 (1954).

[5] "The Bite of the G. M. Decision," *Business Week*, June 8, 1957, p. 42.

[6] The relevant portion of Section 7 in its original form reads as follows: "That no corporation engaged in commerce shall acquire, directly or indirectly, the whole or any part of the stock or other share capital of another corporation engaged also in commerce where the effect of such acquisition may be to substantially lessen competition between the corporation whose stock is so acquired and the corporation making the acquisition or to restrain such commerce in any section or community or tend to create a monopoly of any line of commerce."

[7] *U.S. v. du Pont*, p. 592.

instance, been applied only to horizontal acquisitions;[8] the 1950 amend-
ments made it clear that all kinds of acquisitions and mergers, vertical
and conglomerate as well as horizontal, were covered; but the present
case was governed by the Clayton Act in its pre-1950 form.

Second, it was held that, although prior cases under Section 7 were
brought at or near the time of the stock acquisition involved, this fact
does not mean that the government is foreclosed from bringing an
action at any time when a threat of substantial lessening of competition
or restraint or monopoly of any line of commerce is evident. The Court
pointed out that the purpose of the Clayton Act was to arrest the fore-
going tendencies in their incipiency and that: " 'Incipiency' in this con-
text denotes not the time the stock was acquired, but any time when
the acquisition threatens to ripen into a prohibited effect. . . . To ac-
complish the congressional aim, the Government may proceed at any
time that an acquisition may be said with reasonable probability to
contain a threat that it may lead to a restraint of commerce or tend to
create a monopoly of a line of commerce."[9] In the present case the suit
was brought 30 years after the stock acquisition involved took place.

Third, the relevant market for purposes of determining whether there
had been a substantial lessening of competition as a result of the stock
acquisition in question was held to be the market for automobile finishes
and fabrics and not the entire industrial market for finishes and fabrics
of all sorts. The Court held that "automotive finishes and fabrics have
sufficient peculiar characteristics and uses to constitute them products
sufficiently distinct from all other finishes and fabrics to make them a
'line of commerce' within the meaning of the Clayton Act."[10] It was
noted that General Motors accounted for upwards of two-fifths of the
total sales of automotive vehicles in the United States and that in 1946
and 1947 du Pont supplied approximately two-thirds of General
Motors' requirements for finishes and 40 to 50 per cent of its require-
ments for fabrics. Hence the requirements for establishing a violation
of Section 7, namely, that the market involved be "substantial" and
that the firm involved supply a "substantial" part of that market, were
satisfied.

This holding stands in sharp contrast to that in the *Cellophane*[11] case
a year earlier, in which du Pont was held not to have violated the Sher-

[8] Prior to 1950 there was one lower-court decision arising out of a private treble-
damage suit which held that Section 7 covered vertical stock acquisitions. *Ronald Fabrics
Co. v. Verney Brunswick Mills, Inc.,* C C H Trade Cases 57514 (D.C. S.D. N.Y., 1946).

[9] *U.S. v. du Pont,* p. 597.

[10] *Ibid.,* pp. 593-94.

[11] *U.S. v. E. I. du Pont de Nemours and Co.,* 351 U.S. 377 (1956). See also G. W. Stock-
ing and W. F. Mueller, "The Cellophane Case and the New Competition," *Am. Econ.
Rev.,* March 1955, XLV, 29-63.

man Act despite the fact that it was one of only two producers of cellophane and accounted for 75 per cent of the output of that commodity. It was held that the relevant market was not that for cellophane but for all flexible wrapping materials and that cellophane's 17.9 per cent share of the latter market was too small to sustain a finding that du Pont was guilty of monopolizing within the meaning of Section 2 of the Sherman Act.

In addition to the foregoing specific findings the Court by implication made two further points. First, the percentage of stock ownership necessary to establish control may be substantially less than a majority. Under the circumstances of the present case du Pont's 23 per cent ownership in General Motors was found to be sufficient to establish control. Second, while it must be shown that a vertical stock acquisition results in excluding competitors from a "substantial" share of the relevant market in order to establish a violation of Section 7 it is not necessary that they be completely excluded. In the present case competitors of du Pont supplied a substantial share of General Motor's requirements for finishes and fabrics and the bulk of its requirements for a number of other products which could have been secured from du Pont. As with the definition of the relevant market, the significance of the foregoing features of the present decision will depend upon future interpretations.

The Court also found it necessary to demonstrate that du Pont's commanding position as supplier of finishes and fabrics to General Motors was not achieved on competitive merit alone but resulted from the elimination of competitive suppliers consequent upon du Pont's stock acquisition. In support of its conclusion the Court reviewed the circumstances antecedent to the acquisition and documents purporting to show du Pont's purpose in making the acquisition. Some years prior to the acquisition of the General Motors stock the du Pont Company, long dominant in the manufacture of military and commercial explosives, had decided to expand its business into other fields. The desirability of this step was emphasized by the decision in 1908 of the United States to construct and operate plants to supply explosives for the armed forces. A search for other uses of nitrocellulose, the principal raw material used in the manufacture of smokeless powder, revealed outlets in the manufacture of lacquers, celluloid, artificial leather, and artificial silk. Expansion into these fields by du Pont was begun by the purchase in 1910 of the Fabrikoid Company, then the largest manufacturer of artificial leather. The expansion program was barely started, however, when the first world war intervened and made it necessary for du Pont greatly to enlarge its facilities for producing explosives. The need to find post-war uses for these enlarged facilities stimulated du Pont to

continue its expansion program into other fields during the war years, $90 million being set aside for this purpose.

The trial court found evidence that at or near the time of du Pont's purchase of General Motors stock officials of du Pont were well aware that the latter was a large consumer of some of the commodities which their organization was producing in increasing quantities. John J. Raskob, then treasurer of du Pont, apparently was the principal promotor of the idea that his company should make an investment in General Motors in order to insure that it would supply a predominant share of the latter's requirements for finishes, fabrics, and other products. He was supported in this view by Pierre S. du Pont, then president of the du Pont Company, who had acquired personal holdings of General Motors stock in 1914, and by William C. Durant, founder and, for some time, president of General Motors. Two circumstances facilitated the eventual purchase of General Motors stock by du Pont. First, when Durant and the banking interests controlling General Motors deadlocked on the choice of a Board of Directors in 1915 they resolved the deadlock by agreeing to name Pierre S. du Pont chairman of the General Motors Board, and to make Pierre du Pont, Raskob, and two nominees of Pierre du Pont neutral directors. Second, $50 million of du Pont's $90 million expansion fund was still in hand. The first block of General Motors stock was purchased by du Pont in 1917. In recommending this purchase to the du Pont Finance Committee Raskob said that "Our interest in the General Motors Company will undoubtedly secure for us the entire Fabrikoid, Pyralin [celluloid], paint and varnish business of those companies, which is a substantial factor."[12] Du Pont's annual reports to its stockholders in 1917 and 1918 likewise emphasized that the purchase would result in the company obtaining a new and substantial market for its products. In view of the foregoing evidence the majority of the Supreme Court felt that there was no basis for concluding that the acquisition could qualify for Section 7's exemption of purchases made "solely for investment."

Immediately after the stock acquisition J. A. Haskell, du Pont's sales manager and vice president, became General Motors' vice president in charge of the operations committee, and documentary evidence revealed his intention to pave the way for more general adoption of du Pont products. In the Court's words:

Haskell set up lines of communication within General Motors to be in a position to know at all times what du Pont products and what products of du Pont competitors were being used. It is not pure imagination to suppose that such surveillance from that source made an impressive im-

[12] Quoted in *U.S. v. du Pont*, p. 602.

pact upon purchasing officials. It would be understandably difficult for them not to interpret it as meaning that a preference was to be given to du Pont products. Haskell also actively pushed the program to substitute Fabrikoid artificial leathers for genuine leather and sponsored use of du Pont Pyralin sheeting through a liaison arrangement set up between himself and the du Pont sales organization.[13]

The Court concluded that whereas prior to the stock acquisition du Pont's sales to General Motors were relatively insignificant, as a result of the foregoing activities "du Pont quickly swept into a commanding lead over its competitors, who were never afterwards in serious contention."[14] By 1921, 4 of General Motors' 8 operating divisions bought from du Pont their entire requirements of paints and varnishes; 5, their entire requirements of Fabrikoid; 4, their entire requirements of rubber cloth; and 7, their entire requirements of Pyralin. The Fisher Body division for many years refused to use du Pont products. The explanation probably is that when General Motors acquired the Fisher Body Company a voting trust was established which gave the Fisher brothers more autonomy in the management of the business than was enjoyed by the other operating divisions and enabled them to withstand efforts of high-ranking du Pont and General Motors executives to induce them to switch to du Pont from their accustomed sources of supply.

The Court conceded that "Competitors did obtain higher percentages of the General Motors business in later years although never high enough at any time substantially to affect the dollar amount of du Pont's sales."[15] It also conceded that "considerations of price, quality and service (of the products concerned) were not overlooked by either du Pont or General Motors" but held that the wisdom of this business judgment could not obscure the fact "plainly revealed by the record, that du Pont purposely employed its stock to entrench itself as a primary supplier of General Motors' requirements for automotive finishes and fabrics."[16] Finally, the fact that the executives of both companies acted fairly and on the basis of honest conviction concerning the best interests of their respective companies, without any attempt to overreach du Pont's competitors or anyone else, was dismissed as irrelevant on the ground that it was not necessary to show intent to restrain competition or to create a monopoly in order to prove a violation of Section 7.

Justice Burton, joined by Justice Frankfurter, in a long minority

[13] *Ibid.,* p. 603.
[14] *Loc. cit.*
[15] *Ibid.,* p. 605.
[16] *Ibid.,* p. 606.

opinion attacked the reasoning and conclusions of the majority. The minority's reading of the legislative history of the Clayton Act convinced them that vertical acquisitions of stock were to be reached, if at all, only by provisions of the Clayton Act other than Section 7. They pointed out that for 40 years this interpretation of Congressional intent had been followed administratively by the Department of Justice and Federal Trade Commission and contended that this should be regarded as persuasive evidence of the proper scope of Section 7. They objected to the majority's ruling that lawfulness at the time of the suit rather than at the time of the stock acquisition was controlling, on the ground that "The result is to subject a good-faith stock acquisition, lawful when made, to the hazard that the continued holding of the stock may make the acquisition illegal through unforeseen developments," and that "such a view is not supported by the statutory language and violates elementary principles of fairness."[17]

The majority were accused of being guilty of a logical fallacy in concluding that "because du Pont over a long period supplied a substantial portion of General Motors' requirements of paints and fabrics, its position must have been obtained by misuse of its stock interest rather than competitive considerations."[18] In support of the contrary interpretation the minority pointed out that each of the General Motors operating divisions bought independently and that the volume of purchases from du Pont varied greatly from one division to another; that although du Pont is General Motors' principal paint supplier the latter in recent years had bought as much as 30 per cent of its paint from competitors; that du Pont has had much less success than its competitors in selling to General Motors products other than finishes and fabrics; and that the fact that du Pont supplies a larger proportion of General Motors' requirements of finishes and fabrics than of other automobile manufacturers can be explained on grounds other than its stock interest in General Motors. For example, Ford follows the policy of making most of its own finishes and fabrics, with du Pont supplying most of these materials which the company itself does not manufacture. Chrysler follows the policy of selecting for each product a single supplier to whom it can be the most important customer, choosing Pittsburgh Plate Glass for paint and Texileather for fabrics.

The minority dismissed the documentary evidence relied upon by the majority to prove du Pont's intent to secure noncompetitive preferences—the letters and reports of Raskob, Haskell, and others—as being in each case "a matter of disputed significance which cannot be evalu-

[17] Ibid., p. 622.
[18] Ibid., p. 643.

ated without passing on the motivation and intent of the author."[19] It was said that, read in the context of the situations to which they were addressed, they were consistent with the District Court's conclusion that no restriction was placed on General Motors' freedom to buy as it chose and that General Motors' buyers did not regard themselves as being in any way restricted. The minority also contended that the Supreme Court should have accepted the lower court's interpretation of the facts in the case, since its findings were supported both by contemporaneous documents and by oral testimony and since the question of the credibility of the witnesses was of great importance and the trial judge was in a position to evaluate their credibility at first hand.[20]

Finally, economists may be interested in the minority's criticism of the Court's concept of the relevant market. They held that the record did not support the majority's decision that the relevant market was that for automotive finishes and fabrics; on the other hand, the record did show that other finishes and fabrics were competitive with those in question, and that therefore the relevant market was that for all industrial finishes and fabrics. The majority were criticized for including sales of du Pont's "Dulux," which is not used on automobiles, in computing du Pont's share of the market for automotive finishes and for excluding the sales of its "Duco" automotive finishes which are made for nonautomotive uses.[21] The comment was made, à la Schumpeter,[22] that "If Duco is to be treated as a separate market solely because of its initial superiority, du Pont is being penalized rather than rewarded for contributing to technological advance."[23] On the basis of the minority's definition, du Pont's share of the relevant market in 1947 was less than

[19] *Loc. cit.*

[20] Two principal criticisms of the *du Pont* decision from a legal standpoint are suggested by the above discussion of the minority opinion. One concerns the proper scope of judicial review; it is said that the lower court's findings of fact should have been sustained since they were not clearly erroneous and were supported both by oral testimony and contemporaneous documents. The other criticism is that in holding that the original Section 7 applied to vertical as well as horizontal acquisitions and that lawfulness at the time of bringing suit rather than at the time of acquisition was controlling, the Court disregarded 40 years of administrative interpretation and all the precedents except one lower-court decision. Underlying this criticism is the contention that regard for precedent gives predictability to law and guards against capricious or illogical changes in rulings, and that if precedents become outmoded as a result of changing conditions or changing social policies the proper remedy is legislative action.

[21] If the relevant market were taken to be that for all du Pont finishes for both automotive and nonautomotive uses General Motors' share in recent years, according to the minority, would range from 14 to 25 per cent. While this is considerably smaller than the share as computed by the majority's method it would still seem to be "substantial."

[22] J. A. Schumpeter, *Capitalism, Socialism and Democracy*, 3rd ed. (New York, 1950), Ch. 7, 8.

[23] *U.S. v. du Pont*, p. 651. .

3.5 per cent for finishes and about 1.6 per cent for fabrics. From this it was concluded that the Clayton Act was not violated by du Pont's stock acquisition because it did not foreclose competitors from a substantial share of the relevant market or significantly limit the competitive opportunities of others trading in that market. ·

Economists would disagree with the minority's contention that automotive finishes are indistinguishable from other industrial finishes or that Duco is indistinguishable from other finishes. The mere fact that the latter was a patented product is at least presumptive evidence that it is significantly differentiated from other finishes. Moreover each such differentiated product has a distinct market; whether the "relevant market" for antitrust purposes is to be considered the market for a single product in the narrowest sense or as comprising a group of markets of closely related products is necessarily a policy question depending upon what degree of monopoly power is regarded as permissible.

While the present decision extends the scope of the antitrust laws in ways described above this conclusion is subject to certain qualifications. First, the holding which extends the Clayton Act in its pre-1950 form to vertical stock acquisitions is not likely to be of great importance. It was early discovered that the original Clayton Act covered acquisitions of stock but not of assets;[24] in view of this loophole it is unlikely that there were many vulnerable stock acquisitions in the pre-1950 period. For the period since 1950 the holding is superfluous in view of the Celler amendments which extend the coverage of the Clayton Act to all types of mergers and acquisitions, vertical, horizontal, and conglomerate, whether achieved by the purchase of stock or the purchase of assets. Second, there are definite limitations on the retroactive aspect of the holding which permits suit to be brought at any time, following a stock acquisition, when a probable violation of the Clayton Act can be established. This is both because of the probable fewness of vulnerable stock acquisitions in the pre-1950 period and because of the need on the part of the antitrust enforcement agencies to conserve their time and resources for dealing with important current cases. Where acquisition of assets is involved there is the additional limitation that suit must be brought before the assets become scrambled to such an extent as to make divestiture impracticable. However, the holding in the present case is important in that it will enable the enforcement agencies to deal retroactively with some recent, post-1950, acquisitions, and by removing the time factor, especially in the case of stock acquisitions, should materially strengthen enforcement in the future.

[24] See *F.T.C. v. Western Meat Co., Thatcher Mfg. Co. v. F.T.C., Swift and Co. v. F.T.C.,* 272 U.S. 554 (1926); *Arrow-Hart and Hegeman Electric Co. v. F.T.C.,* 291 U.S. 587 (1934).

The significance of the present case, as indicated at the outset, perhaps depends less upon what it decided, important though these points are, than upon the answers which will ultimately be given to the numerous important questions which it poses. For the case raises more questions than it answers. Since the decision rests to a large extent upon facts peculiar to the du Pont-General Motors relationship much uncertainty has been created concerning the extent to which a similar conclusion will be reached in future cases under Section 7 of the Clayton Act which involve different fact situations.[25] One area of uncertainty concerns the definition of the relevant market which will be adopted in future acquisition cases. Will a narrow concept of the relevant market comparable to that adopted in the present decision be retained in future Clayton Act cases? Will a narrow concept of the relevant market be adopted in Clayton Act cases while a broader definition is retained (following the precedent of the *Cellophane* decision) in Sherman Act cases?

A second area of uncertainty concerns the percentage of ownership by one corporation in another which will be vulnerable under Section 7. Under the circumstances of the present case du Pont's 23 per cent ownership of General Motors stock was held to be sufficient to establish the fact of control. However, it cannot be assumed that this is necessarily a firm figure; the percentage of ownership which will be held to establish the fact of control may be higher or lower in future cases under Section 7 depending upon the degree of interference by the acquiring corporation in the affairs of the corporation a stock interest in which has been acquired, the absolute size and market shares of the acquiring and acquired corporations, and other factors.

A related question concerns the extent to which the rulings in the du Pont case with regard to the relevant market and percentage of ownership necessary to establish the fact of control will serve as precedents in future cases involving joint stockholdings by two or more corporations in a third corporation. Another related question concerns the possible bearing of the present case upon vertical integration involving the acquisition of the entire capital stock or assets of a customer or supplier corporation. In view of the fact that since 1950 the Clayton Act has applied to acquisitions of assets as well as of stock the question arises whether a corporation will be more or less vulnerable if it acquires, or has acquired, a customer or supplier corporation outright than if it acquires merely partial stock ownership. More specifically, will a concept of the relevant market comparable to that adopted in the

[25] See also Betty Bock, "Antitrust Polarity: The Two du Pont Decisions," *Conference Board Bus. Record,* July 1957, XIV, 325-31.

present case be adopted in cases involving complete vertical integration? In this connection much importance attaches to the outcome of the pending Federal Trade Commission complaint against the acquisition by the Union Carbide and Carbon Corporation of the assets of the Visking Corporation, one of its principal customers. This is the Commission's first action against a so-called "forward-vertical" merger.

A third area of uncertainty concerns the bearing of the absolute size and market shares of the acquiring and acquired firms upon the vulnerability of acquisitions under Section 7. Would the present decision have been different if one or both of the industries represented by du Pont and General Motors were less concentrated, or if the market share of either or both of these firms had been smaller? What is the minimum market share which could qualify as "substantial" within the meaning of the Clayton Act? Would the decision have been different if the absolute size, as distinct from the market share, of du Pont and/or General Motors had been materially smaller?

A final question concerns the remedies which will be invoked to give effect to the Supreme Court's decision in the du Pont case. The Supreme Court in remanding the present case to the District Court noted that the latter had "wide discretion" in adapting remedies to the requirements of the individual case. The question of remedies in the present proceeding is particularly difficult because of the magnitude of the divestiture involved. The sale of du Pont's holdings of some 63 million shares of 'General Motors, currently worth about $2.4 billion, would represent the largest government-ordered disposal of property ever made. Sale of the stock in the open market over any short period would drastically depress the price of the shares and might have other undesirable repercussions. Even assuming that it were possible to dispose of the stock on the open market without appreciable depression of its price this solution would be undesirable from du Pont's standpoint because of tax considerations. The Company would be subject to a huge capital gains tax—estimated at between $500 and $600 million—while the du Pont stockholders would pay income taxes at the regular rates on such part of the proceeds as might be distributed in dividends.

The government has filed a divestiture plan intended to accomplish the eventual disposal of the General Motors stock while minimizing the foregoing difficulties. The plan calls for depositing all of the General Motors stock with a court-appointed trustee who, in turn, would parcel it out over a ten-year period to du Pont stockholders other than Christiana Securities Company, Delaware Realty and Investment Corporation, and stockholders of the latter. The distribution would be made in proportion to each shareholder's interest in the du Pont Company.

Christiana, Delaware, and the stockholders of Delaware, together accounting for about 40 per cent of the du Pont stock, would be excluded from the distribution by reason of their representing the interest of the du Pont family. The government proposes that these parties' share of the General Motors stock be disposed of at public or private sale over a ten-year period, with the other du Pont stockholders being given the first opportunity to purchase the stock. During the ten-year divestiture period voting rights to the General Motors stock would be lodged with du Pont stockholders other than Christiana, Delaware, and the stockholders of Delaware. The voting rights attached to the latter parties' share of the General Motors stock would be prorated among the other du Pont stockholders until the stock is sold. The purpose of these arrangements is to prevent an important residue of common control of du Pont and General Motors from remaining in the hands of the du Pont family.

The government's plan prohibits du Pont, Christiana, and Delaware from acquiring or holding, directly or indirectly, any General Motors stock or from exercising any kind of influence or control over General Motors. Du Pont and General Motors are prohibited from having interlocking directorates and common officers; from entering into any agreement, understanding, or arrangement for joint ownership or operation of any commercial or manufacturing enterprise; and from granting exclusive patent rights to each other. In addition, du Pont and General Motors are prohibited from entering into any contract, agreement, or understanding which requires that General Motors purchase any specific percentage of its requirements of any product from du Pont, or which grants to du Pont the first or a preferential right to manufacture or sell any chemical discovery made by General Motors.

Du Pont's plan for divestiture has not yet (January 1958) been filed, but it is reported unofficially that the Company favors placing its holdings of General Motors stock in a nonvoting trust without any requirement that the stock be sold. Unless an agreement can be reached between the parties it is highly probable that the District Court's decision with regard to remedies will be appealed to the Supreme Court.

The foregoing analysis, in the opinion of the writer, demonstrates that the *du Pont* decision materially increases the ability of the enforcement agencies to maintain if not enhance the strength of competitive forces in the economy, and that potentially its influence in this direction could be much greater. The decision will, therefore, be applauded by those who favor a strong antimonopoly policy, but even those who oppose such a policy and the larger number who are defeatists concerning its possibilities would find it difficult to deny that stockholdings of

COMMUNICATIONS

Theoretical Note on Time-Preference, Productivity of Capital, Stagnation and Economic Growth

Among the many factors which determine the growth or stagnation—as the case may be—of a national economy, its rate of saving out of current income and the subsequent increase in income resulting from the investment of these savings play an important role. A relatively simple method of graphic presentation and analysis makes it possible to articulate, without explicit recourse to algebra or calculus, the various effects which different configurations of these two determinants can have on the state of the economy and its development over time. Like any other purely theoretical inquiry, this analysis only helps us to draw certain, possibly not immediately obvious, conclusions from alternative sets of hypothetical assumptions.

Figure 1 depicts the preferences of a given national economy between present and future levels of consumption in terms of a conventional set of social indifference curves. It deviates only in one respect from the graph used by Irving Fisher in his classic exposition of his theory of interest. The variables, Y and C, whose magnitudes are measured along the horizontal axis, represent respectively the level of real income and the amount of goods consumed in the present period. The variables Y' and C' measured vertically describe future income or consumption; "future," however, not in the sense of a single "second" period —as shown on Fisher's diagram—but in the sense of a steady, even flow which, beginning with the year following the present one, can be maintained in equal annual amounts in perpetuity.

Accordingly, every point between the coordinate axes in Figure 1 denotes a specific combination of a given present year's income (or consumption) level with a fixed level of annual income (or consumption) flow to be enjoyed in perpetuity from the next year on. Each indifference line represents a set of equally desirable combinations of present consumption levels and future consumption streams, the positions on higher indifference lines being naturally preferable to those on the lower.

The movement, from right to left, along any one of the negatively sloped straight lines, such as $P_1P'_1$ or $P_2P'_2$ accordingly describes an exchange of a batch of present goods for a constant stream of future goods or, in other words, the exchange of a capital sum for a perpetual series of equal annual interest payments. The (absolute) magnitude of the slope of each one of these exchange lines can consequently be interpreted as representing an annual real rate of interest. Given a free choice between alternative positions on a given exchange line, the income receivers would accordingly reach the highest attainable— under the given circumstances—level of welfare at tangency points, such as P'_1, P'_2 or P'_3.

Any point, such as P_1, P_2 or P_3, situated on the 45° line drawn from the origin, describes a stationary position in which the present (Y) and the future (Y'') levels of income and consumption are identical. Actually faced with a choice between the maintenance of such a stationary state and a movement to some other position located along the exchange line which goes through it, income receivers will perpetuate the stationary state only if, as at P'_3, it happens also to be the point of tangency between the exchange line and the indifference curve which passes through that point. In other cases, they can improve their welfare by consuming less than their entire present income in order to secure a higher level of future income and consumption streams. Or, on the contrary, they might improve their situation by borrowing against the future, so as to allow the present consumption to exceed the rate of current revenue.
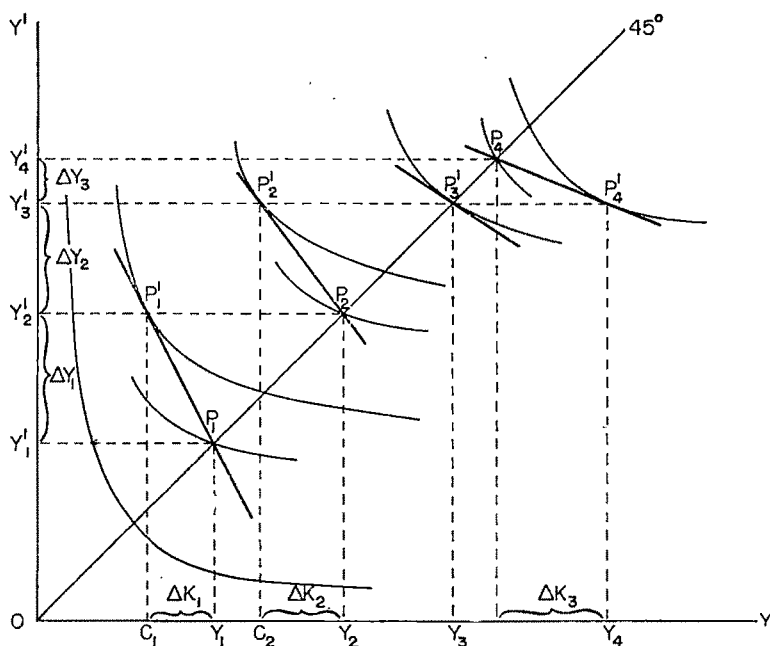
FIGURE 1

Thus, starting, for example, from the initial position P_1 and facing the exchange line which passes through that point, the representative independent income recipient or the central planning authority—whichever it may be—will move from P_1 to P'_1. It will allocate to immediate direct consumption that part of present income OY_1 which is measured by the distance from O to C_1; the rest of it, C_1Y_1, or ΔK_1, will be saved and exchanged against future income. The rate of the potential income stream to be received in the next, and all later, years will be raised by ΔY_1 from OY''_1 to OY''_2. Point P_2, again located

on the 45° line thus represents the prospective position of the country in the second year.

Before pursuing further the sequence of given income, saving and increased income, let us turn to Figure 2 which describes the relationship between the total stock of capital invested and the net output (income) which it can produce on the basis of the existing technology in cooperation with the given supply of all other factors. Along the horizontal axis, we measure from right to
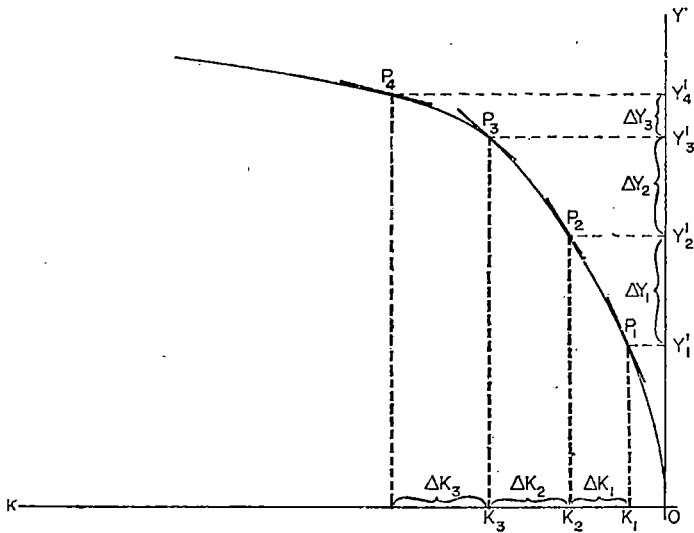
FIGURE 2

left (in order to facilitate the subsequent comparison with Figure 1) the total stock of capital, and along the vertical axis the annual rate of net output, *i.e.*, net income. The bending of the curve describes the well-known technical relationship between the stock of productive capital and the flow of output produced with its help. The slope of that curve at any point represents the marginal productivity of the particular amount of capital which corresponds to it.

Point P_1 in Figure 1 refers to the same state of the economic system as point P_1 in Figure 2. In this position, the total stock of capital amounts to K_1 and it produces a net income flow of Y_1 units per annum. The potential increase in output, which could be brought about by an increase in the existing stock of capital, can be read off Figure 2: Specifically, the ratio between the amount of invested savings and the resulting rise in future income flow is represented —at least for changes which are not very large—by the slope of the capital-output curve at point P_1. It is that slope which, when transferred to Figure 1, describes the real rate of interest, the ratio at which the present consumption can be foregone in favor of additional future income, or, in other words, the slope of the line along which the country moves (see Figure 1) from P_1 to P'_1. The saving, ΔK_1, when added on Figure 2 to the original amount of capital,

K_1, increases the total stock to K_2 and the corresponding annual rate of income flow—from Y'_1 to Y'_2. Measured on Figure 1 along the horizontal axes this is the increase from Y'_1 to Y'_2. Measured on Figure 1 along the horizontal axes this is the increase from Y_1 to Y_2.

The income and consumption, represented by the position of point P_2 in Figures 1 and 2 could be maintained, as far as the country's productive power is concerned, from now on into the future without any further change. The combination of the marginal productivity of capital and time-preference, as shown at point P_2 in Figures 1 and 2, is, however, such that instead of consuming all of that increased income our developing economy will move on to point P'_2, i.e., save and invest again, increase its stock of capital from K_2 to K_3 and its income from Y_2 to Y_3. By the third year, it thus will find itself at P'_3. The slope of the indifference curve passing through that point in Figure 1 has been drawn so as to be equal to the slope of the capital-output curve at the corresponding point (P_3) in Figure 2. Hence the marginal productivity of capital is equated to marginal time-preference if the representative consumer, i.e., the country as a whole, chooses to consume neither more nor less than its entire current income. It is, in other words, an equilibrium position, a stationary state which can and will be maintained ad infinitum as long as no new factors enter the picture. Such a new factor might be a shift of the structural conditions, i.e., a change in the form of the production function in Figure 2 or a variation in the shape of the indifference curves in Figure 1. Or it might consist in the creation of new "initial conditions": sudden destruction—as the result of war—of some part of the existing stock of capital or, on the contrary, acquisition of additional capital from foreign sources, a developmental grant received from abroad.

On our graphs, the creation of such new initial conditions would be described, for example, as a shift from P'_3 to point P_2 or, say, to point P_4. In either case, if left to its own devices, the economy would return at once or by successive steps to its original position at P'_3. The difference between the movement from P_2 to P'_3 and from P_4 to P'_3 is that, in the latter case, having been pushed beyond the point of stable equilibrium, the system will come back to it through a process of capital consumption, i.e., by sacrificing some of the future income stream in order to be able to maintain during the transitory period a "present" level of consumption above its "current" income; while in the former case it would approach the stable equilibrium position, P'_3, from below, i.e., through a process of capital accumulation.

The economy of course does not necessarily find an equilibrium position. It might have none, or more than one, but in the latter case unstable as well as stable equilibria will necessarily be present. We call a state of unstable equilibrium one in which, in the absence of any change in its internal structure and without even the slightest variation in the initial conditions, the system would maintain itself ad infinitum, but from which it would tend to depart on the slightest provocation. It is analogous to the position of the proverbial egg, precariously balanced on its narrow end.

To work out in full the implications of the previous analysis, let us now turn

to Figure 3. Along the horizontal axis, we measure the national income, Y. Of the two interlaced curves, MP represents the marginal productivity of capital, *i.e.*, the slope of the capital-output line (Figure 2) as it gradually bends toward the horizontal with the increase in Y. TP measures the marginal time-preference, *i.e.*, the slope of the indifference curves as they cross the 45° line in Figure 1 at various levels of income Y. The third curve below, identified by the letter D, represents the vertical distance (difference) between the first two (*i.e.*, the excess of TP over MP); the points, a, b and c, at which the D-curve crosses the zero axis mark those income levels at which the marginal productivity of capital is equal to the marginal time-preference when the country consumes exactly its entire income. They mark, in other words, the possible equilibrium positions of the system. The D-curve passes below the zero line at those income levels at which the marginal time-preference (or more precisely the slope of the indifference lines at points where they cross the 45° stationary income locus) is smaller than the corresponding marginal productivity of capital. As can be seen from Figure 1, in all such cases there will be some positive amount of saving. And as a result of it, the income will necessarily grow. Over all those intervals in which the D curve rises above the zero line, current consumption, on the contrary, will exceed net current output, the stock of capital will be diminished and income will consequently fall. The direction of the ensuing upward, or downward, change in income is indicated in Figure 3 by arrows.

To simplify the explanation of the interplay of the two sets of basic structural relationships represented, respectively, in Figures 1 and 2, the functioning of the economy has been viewed as if it had proceeded step by step. Such
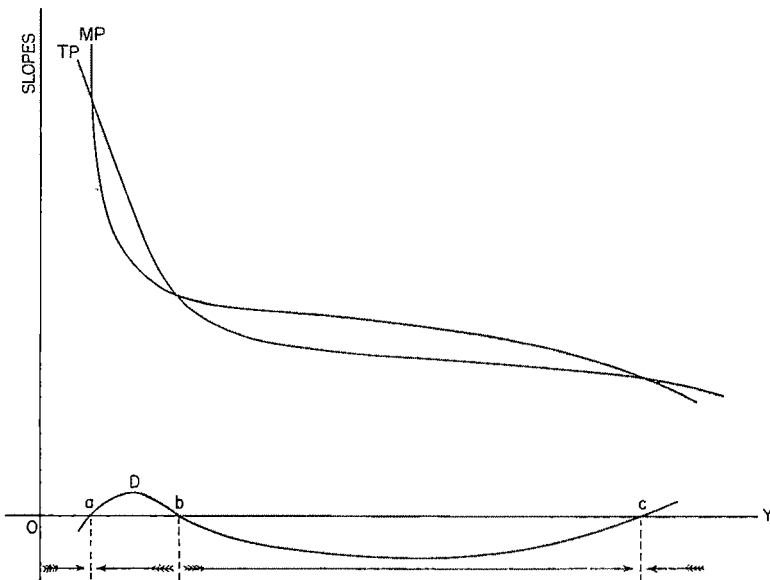
FIGURE 3

period analysis introduces, however, complications of its own which would be absent if the processes of production, consumption and investment were described in continuous terms. With due apology to the mathematically interested reader (who, however, should be able to work out all intermediate details himself), we will now interpret the curves in Figure 3 as if they reflected, as they well might, the properties of a continuous process. [The reader will note that the specific shapes of these curves do not actually correspond to those of the particular set of consumer-preference relationships and the production function depicted in the other graphs. While the combination of the structural relationships shown in Figures 1 and 2 yield only one equilibrium position (P'_3), Figure 3 shows the existence of three such positions, a, b and c.]

Starting with a very small stock of capital and income inferior to that corresponding to the lowest equilibrium point, a, the system would proceed to expand toward a. If in its initial position the economy were located some place between a and b, it would also tend to move toward the former point. In this case, the process is a regressive one characterized by gradual diminution of the stock of productive capital, reduction in the rate of output (income) and incidentally—as the MP curve shows—an increase in the real rate of interest. Once a is reached in either way, the system would "stagnate" at that low but stable equilibrium position. When pushed to the left by the action of some outside force, such for example as an accidental loss of productive capital, it would move back again toward a, but not beyond. If, as beneficiary of a foreign loan or gift, it should find itself in the possession of some additional capital and correspondingly increased income, our country would at once proceed to "live above its means," $i.e.$, consume its capital and gradually reduce its output until the stationary state at a would again be reached. Even a constant flow of foreign aid could, in such case, do no more than help the system to maintain its income and consumption at some point between a and b, without, however, releasing any tendency toward further growth.

These latter observations apply, however, only to gifts or loans not large enough to push the rate of output beyond b. Once on the other side of that unstable equilibrium position, the economy would begin to save, accumulate and increase its revenue; in short it would proceed to develop under its own power. According to the graph, a new stable equilibrium would be approached at the much higher income level, c. Had the structural conditions been such as to keep MP above TP, and thus the D-curve below the zero line, throughout its entire stretch to the right of b, the process of economic growth—once that threshold had been passed—would go on indefinitely.

By way of a concluding observation, one might suggest, without detailed explanation, how the three graphs can also be used to trace through the possible effects of changes in the basic structural conditions of the economy. For instance technological advance, described as an upward shift of the capital-output curve in Figure 2, might—and most likely actually would—affect the shape of the MP and the D-curve in Figure 3. The equilibrium positions a, b and c would shift. Depending on the magnitude and the nature of the change, some of these positions of stationary state might even disappear or new ones might be created.

To the extent to which a rise in the productivity of capital enables the economy to increase its income without any addition to its stock of capital, technological advance will shift the system at once to the right along the horizontal axis in Figure 3 from whatever position it had previously occupied. In fact, however, new technology as a rule requires a new type of equipment and different kinds of skills. That means that its introduction will depend itself on the current rate of saving and accumulation.

A further pursuit of these speculative arguments must clearly yield diminishing returns. The effort involved in construction and interpretation of more complicated graphs might better be spent on observation and explanation of the real world.

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A Proposal for the Automatic Neutralization of Currency Flows

A change in the public's preference for currency relative to its demand for deposit money results in a multiple change in the total potential money supply. This observation is an accepted principle in current expositions of the credit expansion-contraction process and is a characteristic of the monetary system which has been the subject of much criticism.

The Federal Reserve System has had its share of difficulties with banking crises caused by currency drains. The most spectacular manifestation of the difficulty was the wave of bank failures which struck the banking system in the early 1930's, culminating in the bank holidays of March 1933. Enough has since been learned in the use of monetary policy to prevent any widespread closing of banks or even any very serious stringencies for the banking system caused by the movements of currency.

Out of the banking breakdowns of the past have come many reforms and suggestions for reform. The most drastic proposal seriously considered was the 100 per cent reserve plan which would definitely solve the problem of currency drains, but which was so extreme as to be politically inexpedient.[1] Another suggestion, described by E. S. Shaw, is to return the currency-creation function to the commercial banks, and require the same fraction of reserves behind the two money liabilities, currency and demand deposits.[2] A less revolutionary solution has been adopted by utilizing the powers of the Federal Open Market Committee. In principle, open-market operations can be employed to offset any conceivable volume of currency withdrawals by the public.[3]

[1] Irving Fisher, *100 Per Cent Money*, 3rd ed. (New Haven, 1945); A. G. Hart, "The 'Chicago Plan' of Banking Reform," in F. A. Lutz and L. W. Mints, ed., *Readings in Monetary Theory* (Philadephia, 1951), pp. 437-56.

[2] E. S. Shaw, *Money, Income, and Monetary Policy* (Chicago, 1950), p. 156.

[3] R. V. Roosa, *Federal Reserve Operations in the Money and Government Securities Markets* (New York, 1956), pp. 70-71.

This solution is a workable one, but it is not perfect. One of the shortcomings is that of any policy which requires accurate prediction. In order for the action to have the desired effect the currency flows must be correctly anticipated. Because of the regular seasonal pattern of currency in circulation it is one of the more predictable variables with which economists have to work. Yet, as Robert V. Roosa of the Federal Reserve Bank of New York says,

> Much of the time, the precision of the projections is very close. But there are generally three or four periods in any year when something happens to enlarge or spread out the currency outflow, or its return, and at these times there is again no substitute for the "feel" of the market in interpreting the immediate repercussions of such developments upon the existing state of bank reserves.[4]

Errors may exist in the magnitudes involved and the timing of the offsetting policy when "feel" is the guide. Even if accurate prediction were possible there would remain the questions of the pervasiveness of open-market operations and the length of time required for the effects to be felt outside of the major money-market cities. Is it not conceivable that a heavy flow of currency into circulation might be taking place in one area of the country whereas the purchase of securities could be taking place more markedly in other districts? Or again, the currency movements may predominate in one class of banks and open-market operations may more heavily influence the reserve position of another class of banks with different reserve requirements. In other words, while open-market operations are unquestionably more pervasive in effect and more fluid in their impact than the layman could imagine, yet they may not be as refined as many money-market specialists seem to imply.[5]

I. The Proposal

The plan to be presented is offered as a method of offsetting the monetary effects of currency flows which is superior to open-market operations. Although the exposition is limited to the banking structure of the United States, and to the currency-drain problem, the principle is applicable to other banking systems and to other significant problems such as time-deposit changes and Treasury-balance changes. The scheme should not be confused with the security-reserve plans which were so popular during the period immediately following the second world war. There is in fact no kinship.

The heart of the proposal is that every member bank be required to maintain a secondary-reserve-fund account in the Federal Reserve Bank of its district. This income-yielding account would be used *only* in connection with currency flows into and out of circulation. Whenever a bank deposited currency with its Federal Reserve Bank, its legal reserve would be increased by

[4] *Ibid.* p. 71. See also, S. L. McDonald, "Some Factors Affecting the Increased Relative Use of Currency Since 1939," *Jour. Finance,* Sept. 1956, XI, 313-27.

[5] Differences of opinion on this question ultimately must be based on divergent views on the mobility of loanable funds, geographically and among competing uses of funds. Since the justification for the plan to be presented does not depend upon this argument, it is not pursued further.

the reserve requirement (r) times the amount of currency; the remainder, 1-r times the amount of currency, would be credited to the depositing bank's secondary-reserve-fund account. Whenever a bank withdrew currency from the Federal Reserve Bank, its reserve account would be decreased only by r times the withdrawal, and its secondary reserve fund would be reduced by 1-r times the amount of the currency withdrawn.

The two flows can be illustrated by the effects on the consolidated balance sheet of the commercial banks. A movement of currency into circulation results in a reduction of deposits and vault cash. The commercial banks replace the withdrawn currency by ordering supplies from the Federal Reserve. If we assume, for illustrative purposes, a 20 per cent reserve requirement and a $500 million currency drain, the net result on the banks' balance sheet under the plan offered in this paper is: Deposits in Federal Reserve Banks, minus $100 million; Secondary Reserve Fund in Federal Reserve Bank, minus $400 million, and Deposits of the Public, minus $500 million. The effect of the return of currency out of circulation is just the reverse.

In neither case are excess reserves or deficient reserves created. Automatically the economy's adjustments between the two forms of money are compensated in such a way as to maintain a constant supply of money. The Federal Reserve System is then free to influence the monetary environment according to its policy goals without concern for the effect of currency flows.

It would probably be best to permit flexibility in the composition of the assets which would make up the secondary reserve fund. At the present time government bonds, of which member banks hold $39 billion, would constitute a very acceptable choice. No hardship would be imposed on the banks of the system, for the member banks would simply reduce the amount of governments in their general asset portfolio and increase by that amount their secondary reserve fund. The net result on any bank's position would be advantageous to the bank since the secondary reserve fund would yield a satisfactory return, and at the same time be completely liquid for currency-withdrawal purposes, thus relieving the bank from the necessity of holding in its own portfolio the more liquid, lower-yielding or nonearning assets for that contingency.

The fund could be composed of securities other than government bonds. It might be advisable to shift the fund into other forms of assets if the yield on government bonds were to decline markedly relative to that which banks could earn on discretionary assets. This would mitigate attempts by individual banks to avoid the fund mechanism. Another alternative is for the Federal Reserve to issue its own marketable obligations in amounts required by the secondary-reserve-fund system.

Determination of the amount each bank would be required to contribute to the fund should be the subject of an empirical study. According to the author's calculation, each member bank's contribution should be in the order of 2.5 to 3 per cent of total deposits, which would have made the size of the total fund if inaugurated at the end of 1956 between $4.075 and $4.890 billion. Currently, government bonds held by member banks are almost 25 per cent of their total deposits. However, no serious problems would arise from establish-

ing the fund at a size less than the optimum if it were to be inaugurated at the proper time. The size of the fund at its origination should be large enough to withstand any possible extremes in seasonal withdrawal of currency and to prepare for the anticipated secular expansion of the public's demand for currency. A critical level should be established in the size of its fund for each member bank at which point the bank would be required to contribute additional securities to its secondary reserve fund. Normally these bonds would be taken from the member bank's own portfolio. In the event the bank did not possess sufficient quantities of the eligible bonds, it could buy them with excess reserves or convert other assets into the acceptable form. An upper limit should also be set in order to prevent continual expansion of its fund by a bank whose business was such that net currency inflow was the normal situation.

Table I permits the reader to follow the behavior of the fund had it been in operation for the past 10 years, beginning January 1, 1947. In the table it is assumed that the amount of securities in the fund totaled $3 billion on that

TABLE I.—ESTIMATED SIZE OF SECONDARY RESERVE FUND, 1947-56, IF PROPOSAL
HAD BEEN IN EFFECT[a]
(in millions)

	1947	1948	1949	1950	1951	1952	1953	1954	1955	1956
Jan.	3,630	3,930	4,530	5,230	5,130	4,130	2,930	2,830	2,930	2,630
Feb.	3,530	4,030	4,630	5,030	5,130	4,130	2,830	2,830	2,930	2,530
March	3,630	4,130	4,630	5,130	5,330	4,030	2,830	2,830	3,030	2,530
April	3,630	4,330	4,830	5,130	5,130	3,830	2,730	3,030	3,030	2,730
May	3,630	4,330	4,730	5,030	4,830	3,730	2,730	2,930	2,930	2,330
June	3,431	4,092	4,464	4,545	3,954	3,256	2,361	2,637	2,355	1,446
July	3,730	4,230	4,830	5,330	3,630	3,530	2,530	2,930	2,630	2,330
Aug.	3,630	4,130	4,630	5,230	4,430	3,430	2,430	2,830	2,430	2,230
Sept.	3,430	4,030	4,830	5,230	4,330	3,130	2,230	2,930	2,530	2,330
Oct.	3,530	4,030	4,830	5,130	4,030	3,030	2,330	2,930	2,430	2,030
Nov.	3,330	3,830	4,630	4,830	3,930	2,330	1,830	2,830	1,830	1,730
Dec.	3,354	3,651	4,315	4,332	3,427	2,236	1,639	2,830	1,445	1,330

[a] It is assumed that the plan started on January 1, 1947 with a fund of $3 billion. June and December data are call-date figures, so can be for any day of the week. Data for all other months is for the last Wednesday of the month.
Source: *Fed. Res. Bull.*, "Currency outside Banks."

date. Notice that the lowest point reached in the 10-year period was $1,330 million, which means that a fund of $1,670 million would have been just sufficient to handle the flow of currency into circulation. However, the decline in the fund from April 30, 1953 to December 30, 1953 would have been $1,585 million indicating that a sizeable cushion is advisable.

The variation in currency in circulation between the end-of-month dates may be substantial at certain times. This is especially true between the end of November and the end of December since the December call date is always after Christmas when currency has already started its return to the

banks. Also, the data is for Wednesdays except on call dates, which means that the heavy currency-flow days, particularly Fridays and Mondays, are not included.

II. *Comparison of the Proposal with Present Practices*

In contrast to the present use of open-market operations as the tool to offset currency flows, under the secondary-reserve-fund plan no prediction is required. The degree of offset is perfect; for every dollar of reserve lost through the currency drain there will be a reduction of reserves sufficient to decrease demand deposits by precisely one dollar, which action must have taken place when the currency was put into circulation.[6]

Not only is the absolute magnitude of the offsetting action accurate, but the timing is virtually perfect. Also, the offsetting actions strike at the very place where the currency outflows and inflows occur; it makes no difference to the monetary system that banks with different reserve requirements are involved. The total money supply will remain constant. The banks out of which the currency is being withdrawn will lose reserves and secondary reserve funds in amounts necessary to meet this obligation. If, instead, open-market operations were used, these banks might not get any of the reserves supplied by Committee purchases. Indeed the new reserves might be going to banks which are experiencing a net inflow of currency. Notice that if there is some fundamental cause for the currency run on particular banks, the banks must make the proper adjustments under both systems. However, under present conditions disruptive liquidation might be forced on such a bank; whereas under the secondary-reserve-fund proposal an orderly transition to the new conditions is possible.

The resources of the Federal Reserve System devoted to the currency problem could be redirected to the more basic problem of influencing credit conditions and the two remaining mechanical problems of float and Treasury-balance offsets. Thus the actual effect of over-all monetary policy is likely to be closer to the desired effect than under the present policy. This is in addition to the greater inherent accuracy of the automatic actions under the proposed system. For those who object to discretionary actions by government agencies this plan represents a step in the right direction.[7] It has one of the important advantages of the 100 per cent reserve plan, but does not have the disadvantage of requiring a reconstitution of the monetary institutions and is, therefore, unlikely to encounter as severe political opposition.

The credit-creation multiplier is enlarged under this plan by virtue of the elimination of the two important leakages, the vault-cash drain and the currency-in-circulation drain. Therefore the effect of any given change in excess reserves by Federal Reserve or Treasury action will have magnified effects.

An attractive feature of this plan is that the soundness of individual commercial banks would be increased by the existence as an asset of the secondary

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[6] This statement is accurate only if the deposits referred to are demand deposits. The correction for time deposits is made below.

[7] H. C. Simons, "Rules vs. Authorities in Monetary Policy," Lutz and Mints, *op. cit.*, pp. 337-68.

reserve fund itself; this should instill further confidence in banks and should make this plan appealing to the Federal Deposit Insurance Corporation. In the event of a system-wide liquidation crisis the machinery by which the central bank would be able to provide the needed liquidity would already be in operation and would thereby augment the existing rediscount facilities and open-market purchases.

III. *Some Possible Problems*

It may be objected that the proposal has ignored the distinction between demand and time deposits. When currency flows into banks, the amount of new reserves they can thus acquire is the amount of reserves required on demand deposits, even though the currency may originally have been deposited in a time-deposit account. The flaw is not a serious one. Consider an inflow of currency into the banking system for which the public desires time deposits. Under the secondary-reserve-fund plan new excess reserves would be created because the banks would receive sufficient reserves to back an equivalent amount of demand deposits but their required reserves would increase only by the time-deposit requirement. Under existing conditions the magnitude of excess reserves created by the same transaction is considerably larger, the full amount of the deposit minus only the reserve requirement on time deposits. Thus the plan decreases substantially the effect of a shift from (to) currency to (from) time deposits, but does not eliminate it.[8]

The plan works well enough in the simple cases presented above; will it operate as desired in more complex situations? Suppose the following case: a commercial bank is experiencing a net gain in currency at the same time that its deposits are being withdrawn through the clearing process. Under present arrangements if the increase in currency deposits exactly equals the adverse clearing balance this bank would require no adjustments other than the conversion of the currency into legal reserves. Under the proposed system the general assets of the bank would be decreased by $(1-r)$ times the amount of the shift in deposits, but its secondary reserve fund would be increased by the same amount. No other changes would be required

In general, under existing arrangements currency inflows and outflows and clearing house inflows and outflows are essentially equivalents from the standpoint of the individual bank; whereas under the fund plan earning assets could change by $(1-r)$ times the change in deposits via the clearing house, and the fund would change by $(1-r)$ times the change in currency deposits or withdrawals. In any case, for a single bank if the fund is included as a part of earning assets, the two systems produce identical balance sheets. For the sys-

[8] The same basic device could be used to solve the problem of the time-deposit drain. Commercial banks might be required to maintain a secondary reserve against time deposits equal to the difference between the legal reserve required against time deposits and that required against demand deposits. Thus, if the reserve requirement on demand deposits and time deposits were 20 and 5 per cent respectively, a bank receiving new time deposits of $100 would require $5 in deposit at the Federal Reserve Bank, $15 in secondary reserve fund, and $80 of excess reserves. If the deposit were in the form of currency the changes would be: $5 deposit at the Federal Reserve Bank and $95 in secondary-reserve fund.

TABLE II.—BALANCE SHEET CHANGES CAUSED BY A DEPOSIT OF CURRENCY AND A SHIFT IN DEMAND DEPOSITS UNDER THE SECONDARY-RESERVE-FUND PLAN

Transaction	Commercial Bank A Assets	Commercial Bank A Liabilities	All Other Banks Assets	All Other Banks Liabilities	Federal Reserve Banks Assets	Federal Reserve Banks Liabilities
A	Currency +100	Deposits +100				
B	Legal Reserves −100	Deposits −100	Legal Reserves +100	Deposits +100		Deposits of Bank A −100 Deposits of Other Banks +100
C	Currency −100 Legal Reserves + 20 Secondary Reserve Fund + 80					Federal Reserve Notes Outstanding −100 Deposits of Bank A + 20 Secondary Reserve Fund + 80
D	Earning Assets − 80 Legal Reserves + 80		Legal Reserves − 80 Earning Assets + 80			Deposits of Bank A + 80 Deposits of Other Banks − 80
Net Change	Currency 0 Legal Reserves 0 Earning Assets − 80 Secondary Reserve Fund + 80	Deposits 0	Legal Reserves + 20 Earning Assets + 80	Deposits +100		Federal Reserve Notes Outstanding −100 Deposits of All Banks + 20 Secondary Reserve Fund + 80

Transaction A. Currency of $100 is withdrawn from circulation and deposited in Commercial Bank A.
Transaction B. Deposits of $100 are withdrawn from Bank A, and deposited in other banks.
Transaction C. Bank A sends its currency to the Federal Reserve Bank of its district.
Transaction D. Bank A makes the necessary additional adjustment of its reserve position by selling earning assets or calling loans. Implicitly, all other banks have also adjusted their reserve positions.

tem of banks, however, no multiple changes in deposits or earning assets will take place through changes in currency and deposits when the fund plan is in operation.

Table II demonstrates the case just discussed. In order to illustrate the effect of the plan in operation it has been assumed that the currency deposited in the individual bank has come from circulation rather than from withdrawal from other banks.

A question arises as to whether the correspondent relations existing among banks would be altered. Are the proposed changes such that banks could gain from changing their established methods of operation? It is the writer's opinion that they would not. It is true that bankers would probably prefer deposits of checks to deposits of currency, since the former would permit greater freedom of asset selection and possibly a higher return on the funds. For this reason the profitability of certain depositors' accounts may be altered; for example, retail grocers whose deposits are mainly in currency and whose withdrawals are through the clearing house. However, since they certainly would prefer deposits of currency to no deposits, and since differential profitability is expected to be slight, no alteration in behavior is anticipated. Member banks would still operate as intermediaries between nonmember banks and the Federal Reserve Banks.

It is worth while inquiring into the effect of this plan on the so-called vault-cash drain. Under present regulations, every dollar added to the amount of cash commercial banks hold in their vaults decreases the banking system's ability to create deposits by some multiple, or in a situation of zero excess reserves forces a multiple contraction. Under the proposed arrangements, however, an increase in bank holding of currency necessitates contraction of the public's holding of money (in either form) by exactly an equivalent amount. Conversely, given no change in total reserves, banks can expand the public-held portion of the money supply only by the amount of currency which the banks release from their vaults.

The suggestions for reform of the United States banking system now current usually include the proposal that vault cash held by banks be included as acceptable legal reserve for members of the Federal Reserve System.[9] This innovation is reasonable under the present structure, but would be inconsistent with the aims of the secondary-reserve-fund plan. If banks could count currency as legal reserves when held in their vaults, and receive only the required reserve percentage when sent to the Federal Reserve Banks they would be able to circumvent the intent of the proposal by holding currency. In fact, they would find it profitable to do so if the yield on loans were higher than that earned by the secondary reserve fund.

LLOYD M. VALENTINE*

Intermediaries and Monetary Theory: A Criticism of the Gurley-Shaw Theory

The Gurley-Shaw interpretation of the effects of financial intermediaries and their implications for monetary theory and policy[1] rests upon certain basic theoretical innovations of the authors. It seems desirable to have some explicit discussion of the ideas in question. Unless they are correct, the major Gurley-Shaw argument and its important and widely discussed policy conclusions would seem to be untenable. On the other hand, if these ideas are valid, they call for wholesale revision of our thinking about monetary theory and economic stabilization.

The thesis of this paper is that the Gurley-Shaw innovations discussed below are not valid and, therefore, that the authors' argument cannot be accepted. It is impossible here to summarize or to comment on all matters dealt with in the articles in question. This paper limits itself to discussion of certain theoretical innovations that seem to the present writer to be of fundamental importance and to play an essential role in the Gurley-Shaw argument. The main issues can be summarized in two questions: (1) Do commercial banks differ from financial intermediaries[2] in their ability to create credit in any sense that is significant from the point of view of financial control over the economy? (2) Are the concepts of "direct debt" and "indirect debt" useful tools for analyzing financial developments and an appropriate basis for formulating government financial policies to combat instability?

I. Are Commercial Banks Unique in Their Ability to Create Credit?

Gurley and Shaw explicitly propose a departure from the usual way of looking at the role of banks in the economy:

> We are deviating from conventional doctrine in regarding the banking system as one among many financial intermediaries, sharing with the others the functions of indirect finance. We take exception to the view that banks stand apart in their ability to create loanable funds out of hand, while other intermediaries in contrast are busy with the modest brokerage function of transmitting loanable funds that are somehow generated elsewhere.[3]

This difference of view, whatever we may decide is its exact nature, would seem to have policy implications. If commercial banks are not unique, then

[1] J. G. Gurley and E. S. Shaw, "Financial Aspects of Economic Development," *Am. Econ. Rev.*, Sept. 1955, XLV, 515-38 (hereafter referred to as "Aspects") and "Financial Intermediaries and the Saving-Investment Process," *Jour. Finance*, May 1956, XI, 257-76 (hereafter termed "Intermediaries"). The authors' approach also is developed in "Financial Growth and Monetary Controls," a paper delivered at the November 1956 meetings of the Southern Economic Association, and "The Growth of Debt and Money in the United States, 1800-1950: A Suggested Interpretation," *Rev. Econ. Stat.*, Aug. 1957, XXXIX, 250-62.

[2] In this paper, in contrast with the Gurley-Shaw usage, the term "financial intermediaries" excludes commercial banks, which are referred to specifically as banks. The lines drawn in the discussion are such that this is the more convenient usage.

[3] "Aspects," p. 521. See also "Intermediaries," pp. 257, 259, and footnote on page 263.

why should we apply to them a special apparatus of control? On the other hand, if they are unique in some relevant sense, this casts doubt on the appropriateness of the Gurley-Shaw suggestion that direct quantitative government financial control should aim at all financial intermediaries equally with commercial banks, should aim at controlling the total volume of obligations of banks plus financial intermediaries rather than being limited to bank credit and the money supply.[4]

The generally accepted doctrine seems to be that commercial banks (in their function of creating demand deposits)[5] are unique among financial institutions because of two related facts: (1) Commercial banks are the only private institution whose debt serves as a generally acceptable medium of exchange, as money.[6] Money is a unique asset; some others can substitute for it to some degree in some uses, but there is no fully adequate substitute. (2) Because it creates money, the banking system can affect the volume of its liabilities and can create or extinguish credit, or loan funds, in a way that no other financial institution can. This second point, though a familiar one, will be developed somewhat further as a basis for examining the Gurley-Shaw arguments.

The usual view is that intermediaries (using our definition of intermediaries) cannot themselves create credit or loan funds, but rather play a middleman role in conveying to their ultimate users loan funds brought into being by others. Intermediaries perform a function parallel to that of the merchant in other lines; they transmute the debt created by borrowers into something more attractive to lenders. Intermediaries cannot control the volume of their outstanding obligations, although they can influence it in the same ways that other businesses can influence their sales, by altering quality of service, intensity of advertising, or profit margins.

Basically, what the intermediaries do is to maintain a standing offer to the public to provide certain services, as does a broker or a merchant. Sometimes the character of this offer may change appreciably in the short run because of changes on the side of the intermediaries themselves: if a new type of institution suddenly appears on the scene, an innovation in service is offered, or some units go out of business. Such changes usually are not an important factor in short-run financial developments. Thus such variations as occur in the rate of growth of outstanding obligations of intermediaries arise mainly out of changes in economic and financial conditions that affect the extent to which customers desire to take advantage of the standing offer of the inter-

[4] This is implied in "Aspects," pp. 537-38. It is stated explicitly in "Financial Growth and Monetary Controls."

[5] In referring to "commercial banks" in this paper, we will be concerned specifically with the banks' operations in their distinctive function of creating demand deposits. Actual commercial banks, of course, also deal in time deposits. In this function, they do act as financial intermediaries, playing the same role as mutual savings banks.

[6] By "money" in this connection we mean instruments that have such general acceptability as a means of payment that they can be directly "spent" by the holder without the necessity of taking them back to the originator for conversion. Money in this sense ordinarily includes only currency and deposits subject to check.

mediaries, rather than out of any particular decisions or actions on the part of the intermediaries themselves. Short-run changes in the volume of obligations of intermediaries do not seem in the past to have played an important role in cyclical variations in the economy.[7]

Over longer periods, there can be and have been major changes in the volume of outstanding obligations of the various intermediaries and in the types of services offered to the public; such changes can have an important effect upon the financial structure of the economy. However, again, it is probably more correct to think of these developments as arising out of underlying changes in incomes and in the whole economic environment within which the institutions operate rather than out of decisions made at the volition of the managers of the intermediaries.

Discipline over financial intermediaries is exercised in an immediate and direct manner by their creditors. Until someone brings money into a savings and loan association to exchange for its obligations, the association cannot lend money. When the creditor comes back to the association and requests the return of his money, the association must similarly contract its loans. A change in the volume of demand deposits, in contrast, is initiated by banks when they change the volume of their debt holdings; the banks' creditors, as such, play no active role in the process. The banking system "creates credit" by acquiring debt and creating demand deposits to pay for it. The commercial banks do not need "to borrow loanable funds from spending units with surpluses" in order to extend credit ("to transmit the borrowed funds to spending units with deficits . . .").[8]

Once demand deposits are in circulation, they are subject to no controlling mechanism parallel to that governing the obligations of intermediaries. Creditors of intermediaries correct the existence of an excess amount of their obligations by taking them to the originating institutions and exchanging them for cash; the holder of a demand deposit already has cash. He can adjust his cash balance by any sort of variation in his expenditures for consumption, or investment, or in his income. Except in special cases, he does not adjust his personal money balance through any sort of transaction with a bank that causes a corresponding adjustment in the total money supply. While an excess supply of any type of nonmonetary debt or physical asset tends to depress its price and limit its creation, an excess supply of money tends rather to enlarge the spending stream, and potentially to cause inflation. Surely the basic fact, so often illustrated in history, that the public does not and cannot

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[7] This is not to say that such a thing is impossible. Aggravated cyclical variations in obligations of intermediaries, particularly those that are liquid and serve to some extent in place of money balances, could be a matter for concern, in the same way as could fluctuations in the volume of other liquid assets. However, a superficial examination of the pattern of growth of obligations of the major intermediaries suggests that they have participated to a lesser extent than have most types of financial assets in cyclical fluctuations in the economy. The major variations in their rate of growth have been longer run. The Gurley-Shaw argument is not based on any factual finding that the volume of obligations of intermediaries has been peculiarly sensitive cyclically.

[8] See "Aspects," pp. 519-20.

prevent an inflationary expansion of the money supply (or a destabilizing contraction) is at the heart of all of monetary theory.

As a corollary of this, the commercial banks cannot perform the characteristic role of an intermediary in taking loan funds created elsewhere and passing them on to ultimate borrowers. An extension of credit by a commercial bank is not preceded by or contingent upon any private act of saving or of commitment of funds to investment use. If a person saves and accumulates demand deposits, this in no way affects the ability of banks to lend.[9] Banks (in their demand-deposit function) cannot act as a loan-fund broker.

Let us see, then, what arguments Gurley and Shaw give to support their departure from this doctrine. To begin with, they define loanable funds in terms of a social accounting framework, identifying the supplying of loanable funds by an economic unit with an excess of its income over its spending on goods and services, or with a net increase in the unit's financial assets less its liabilities. Money is treated as simply another financial asset. When one accumulates demand deposits, thus, by definition he supplies loanable funds.[10]

The authors have a right to their terminology, but redefinition of words cannot refute existing banking theory. What is at issue is a question of substance: what decisions and actions mainly determine changes in the volume of demand deposits and associated bank credit? The usual view is that the volition ordinarily is exercised by the banking system, rather than by deposit holders. The *ex post* accounting approach itself casts little light on the question of where the relevant decisions are made; one's choice of accounting definitions must stem from his analysis of the nature and location of the governing economic decisions. Thus, the usual banking theory would call for social accounting definitions representing creation of loan funds as involved in both of two types of actions: that of the banking system in acquiring additional debt by creating demand deposits (and of the government in adding to currency in circulation) and that of other economic units in increasing their (net) holdings of nonmonetary debt.

Another relevant line of argument given by Gurley and Shaw is this: There is only one money supply, they argue, that is consistent with given levels of expenditures; thus, if the monetary authority effectively performs the task of maintaining economic stability the money supply is really determined ultimately by the decisions of the public. This argument is applied specifically to a shift in the willingness of the public to hold obligations of intermediaries in lieu of money, but the argument applies equally to any shift in the attitude of the public towards holding money balances.[11]

However, this does not really contradict the traditional view that the bank-

[9] A decision of the public to increase holdings of idle demand deposits, by decreasing the expenditure-income stream, might be said to increase the amount of money that the banks *ought to create* if they are to assume responsibility for maintaining the earlier income level, but this is quite another matter.

[10] "Aspects," pp. 516-17.

[11] "Intermediaries" pp. 261-62. The authors may intend this argument to apply only to the long-run growth of banks, rather than to be a part of their refutation of the orthodox view of the role of banks in the economy.

ing system, including the central bank, determines the amount of demand deposits. If its objectives and all relevant variables were completely given, the central bank necessarily would act in a certain way; but it has the power, if it chooses, to act in a different way. It is true that if banks were controlled by an omniscient authority they would, in a sense, behave the way that intermediaries behave without any such control, since the public's decisions about holding demand deposits then would immediately affect the quantity of them. This does not argue that banks and intermediaries are subject to the same natural discipline and therefore should be subjected to similar types of controls.

In order to sustain their argument, and their explicit rejection of accepted banking theory, what the authors have to show is that, contrary to common belief, there is a mechanism through which the public directly exercises a control over the volume of actual demand deposits parallel to that which it exercises over the volume of obligations of intermediaries. Such an argument seems to be what they intend to convey by the following illustration:

> Suppose that excess money balances, resulting from a shift in spending units' demand away from money balances to alternative forms of indirect financial assets, are not destroyed by central bank action. They may be used to repay bank loans or to buy other securities from banks, the result being excess bank reserves. At the prevailing level of security prices, spending units have rejected money balances. But cannot banks force these balances out again, resuming control of the money supply? They can do so by accepting a reduced margin between the yield of primary securities they buy and the cost to them of deposits and currency they create. But this option is not peculiar to banks: other intermediaries can stimulate demand for their debt if they stand ready to accept a reduced markup on the securities they create and sell relative to the securities they buy. The banks can restore the money supply, but the cost is both a decline in their status relative to other financial intermediaries and a reduction in earnings.

> The banks may choose to live with excess reserves rather than pay higher prices on primary securities or higher yields on their own debt issues. . . . With their competitive situation improved, non-monetary intermediaries have stolen away from the banking system a share of responsibility for sustaining the flow of money payments. . . . They have reduced the size of the banking system at the given income level, both absolutely and relatively to their own size, and their gain is at the expense of bank profits.[12]

[12] *Ibid.*, p. 262. In this example, and also elsewhere, the authors begin by assuming that the public decides, for no specific reason that is given, to shift from holding cash balances to holding obligations of intermediaries. "Suppose that savings and loan shares become more attractive relative to bank deposits, resulting in an excess supply of money" (*ibid.*, p. 263). There is no doubt that such a shift in investor behavior is expansionist, but this is true of a decision of investors to shift out of money and into any type of debt or physical asset (a fact that reflects, in itself, the unique position of money in the system). It is not obvious that such a shift is more likely to occur from money to the obligations of intermediaries than to other types of investment assets or that its effects are different in the two cases.

This does not seem to show that commercial banks cannot "create credit" in the sense in which it has been supposed that they could create credit, in connection with their demand deposit function. Under unusual conditions a lack of *suitable* debt for banks to acquire, rather than a lack of reserves, may be the limiting factor on expansion of bank credit; it is common to point to the 1930's as an example of this.[13] However, it seems clear that ordinarily changes in the volume of bank credit are caused mainly by changes in bank reserve positions. They are not ordinarily caused by changes in the supply of debt available for banks to buy brought about by changes in the supply of demand deposits relative to the demand for them.

If it could be argued successfully that the banks can only issue that volume of money that the public wants to hold (in some sense), this would appear to mean that expansion of bank credit cannot be a destabilizing force in the economy. Such a view would be difficult to square with our experience, which provides plentiful examples of inflationary booms caused by overexpansion of bank credit. Specifically, such inflationary expansions of bank credit are not brought to a halt by the failure of bank profits because of the low interest rates brought about by the redundant money supply (which is the controlling mechanism implied in the passage quoted above, as I understand it). Within a framework of static analysis, such a relationship may seem reasonable, but in the real world, where other things are *not* held equal, an increase in the money supply may or may not reduce interest rates. The effect of an increase in bank credit in increasing the flow of expenditures, spending incentives, and the demand for funds can more than offset its effect in increasing the supply of loan funds. Periods of inflationary bank credit expansion can be periods of high interest rates.

Looking at the authors' argument in another way, what it seems to imply is that the supply of debt to banks is so inelastic that they are forced to limit themselves to the holding of only some externally determined volume, since an attempt by them to acquire more would force yields down so severely as to damage their profits. Our experience does not seem to justify either part of this argument, that is: (1) The supply of debt to banks, including open-market debt that they hold in competition with other investors of all types, does not seem to be nearly so inelastic as this argument requires. (2) Even if banks in the aggregate had a profit incentive to keep their deposits and earning assets below the level permitted by available reserves, there is no evident mechanism whereby this would influence the decisions of individual banks,

[13] What is relevant is availability of *suitable* debt. The total amount of debt in the economy that would be potentially available if banks were willing to purchase it is so great that any absolute shortage of debt for them to acquire is inconceivable. The problem of the 1930's was one of imbalance in the structure of outstanding debt; an unusual shift in the debt structure had sharply reduced the supply of liquid short-term debt. The eagerness of banks to increase their holdings of such debt was evidenced by the fact that they bid yields on it down close to zero. The situation could have been corrected and the money supply made responsive to changes in bank reserves by a federal debt-management policy that insured the availability of a sufficient volume of short-term government debt for banks to acquire. This interpretation of the experience of the 1930's is developed in my article, "The Term Structure of Interest Rates," *Quart. Jour. Econ.*, Nov. 1957, LXXI, 485-517.

which undoubtedly act competitively in relation at least to open-market debt.

None of these arguments, thus, seems successfully to contradict the orthodox doctrine. In the sense in which it has been supposed that commercial banks can create credit, they can create credit. In the sense in which it has been supposed that they are unique, they are unique. If Gurley and Shaw were right in saying that commercial banks and financial intermediaries were functionally identical, this would support their suggestion that the same general type of control ought to be applied to all of them.[14] If they cannot sustain this argument, we must hold to the familiar view that the distinctive properties of commercial banks call for special controls over them.

II. *"Direct Debt"-"Indirect Debt" or Liquidity-Illiquidity?*

The novelty of the Gurley-Shaw argument seems to stem largely from the fact that the authors analyze the effects of financial assets in terms of the dichotomy of "direct debt" and "indirect debt" rather than, as is usually done, on the basis of a grouping of assets according to their liquidity. The validity of their interpretation of long-run trends in the structure of financial institutions, their theory of interest, and their conclusions regarding monetary policy rest upon the concepts of "direct debt" and "indirect debt."

"Indirect debt" is defined as including obligations of all "financial intermediaries" including banks; thus it includes demand deposits. "Direct debt" includes all debt other than that of "intermediaries."[15] The usual approach to financial analysis would undertake to rank assets according to the role that they play in the financial position of the holder—mainly according to their liquidity—irrespective of whether the assets are those of intermediaries or of other debtors. The underlying question is basically a factual one: Does the effect of a given type of financial asset on the behavior of the holder depend upon whether the asset is created by an intermediary or by another debtor, or does it depend upon certain characteristics of the asset itself, mainly its liquidity? A related question is whether it is appropriate to throw demand deposits into the same category with all other "indirect debt" or whether their position in the economy is so distinctive as to merit for them a special place in the analysis.

The approach of the authors to analysis of the effects of obligations of intermediaries is to ask whether they are substitutes for money or for other types of financial assets.[16] This is one valid approach to the problem. In order to justify using the concepts of "direct debt" and "indirect debt," then, the authors must show that all types of obligations of intermediaries substitute for money to a greater degree than do all types of "direct debt." Indeed, to justify completely the use in quantitative analysis of a concept of "indirect debt" by which demand deposits are lumped on the same basis as all obligations of intermediaries, they would have to show that all types of "indirect debt" substitute perfectly for money.

The authors state that: "In view of the increasing variety of financial assets,

[14] "Aspects," pp. 537-38; "Financial Growth and Monetary Controls."

[15] "Aspects," pp. 518-22, 530-38; "Intermediaries," pp. 259, 265-66, 269-71.

[16] See "Aspects," pp. 527-28, 533; "Intermediaries," pp. 261, 264, 266.

it does seem appropriate to abandon liquidity as the pivotal factor in interest theory."[17] Their reliance on the "direct-debt" "indirect-debt" framework would indicate that they in fact have abandoned liquidity entirely as the basis of their analysis; it seems obvious that if one wanted to break financial assets into two categories according to their liquidity (or liquidity plus some other specific characteristic) he would not do it in this way.[18] Just what principle the authors visualize as underlying their system of classification of financial assets does not seem clear. They assert that there is a "diversification demand" for "indirect debt," and that if too much "direct debt" accumulates in relation to the amount of "indirect debt" interest rates will rise. But the underlying question is why people should be concerned with the relative proportions of "direct debt" and "indirect debt" as such; what need or motive causes this concern?[19] There is a definite logic behind liquidity analysis: the logic behind "diversification demand" does not seem clear.

I do not see how the realism of the "direct-debt" "indirect-debt" framework can be defended. It is well known that some types of "direct debt" and "indirect debt" that are in the same liquidity class are treated by people as close substitutes. Treasury bills, time deposits, savings bonds, and savings and loan shares substitute for one another and are among the closest substitutes for cash balances. Common stocks, bonds, and shares in mutual funds substitute for one another, but not to any considerable extent for money balances. It is well known that Treasury bills, a type of "direct debt," are considered by a large class of investors as one of the closest substitutes for money. On the other hand, it is obvious that there is little substitutability between such types of "indirect debt" as demand deposits and equities in retirement systems and between such types of "direct debt" as Treasury bills and speculative common stocks. The question at issue is, indeed, a factual one, and our knowledge is not perfect. But it seems clear that the "direct-debt" "indirect-debt" framework does not correspond to the distinctions actually made by asset holders. The usual analysis based upon liquidity, whatever its limitations, is more realistic than this.

It might well be argued that the usual liquidity approach is too narrow and that, perhaps in part because of irrational investor behavior, some finan-

[17] "Aspects," p. 527.

[18] Nevertheless, the authors sometimes do use the liquidity criterion, and even imply that the difference between "direct debt" and "indirect debt" is a liquidity difference. "Direct debt" is identified with "illiquidity" in "Aspects," p. 530. On the other hand, in observing that "short-term public debt may displace money, and debt management may displace monetary controls" (ibid., p. 535), the authors use liquidity analysis in a way that conflicts with their own system (in making a type of "direct debt" a close money substitute).

[19] The authors point out that obligations of intermediaries may include services: They are "also competitive with money narrowly defined, offering less in liquidity perhaps but offering as well security, interest, insurance, and other services" (ibid., p. 527). It is evident that the special features of obligations of intermediaries, the services embodied in them, serve to make them attractive to the public. It is not apparent, however, that they cause these obligations to be held in the place of money rather than of other financial assets.

cial assets actually serve as substitutes for money to a greater degree than would be indicated by their liquidity; thus, some adjustments might be suggested in our rankings of assets according to the degree to which they substitute for money.[20] Doubtless evidence could be found to support some such revisions. But it is clear that this is not what the authors are suggesting. Such an argument would not seem to justify the "direct-debt" "indirect-debt" framework or the authors' policy conclusions. To assert that more attention ought to be paid to changes in the volume of certain obligations of intermediaries that are liquid and serve as substitutes for money—this also would not be unorthodox, and surely it is not what the authors mean to say. .

It does not seem possible to make the "direct-debt" "indirect-debt" framework more realistic without losing what is distinctive about the authors' analysis. It is not a question of using a less simple framework and disaggregating: "direct debt" and "indirect debt" cannot be disaggregated. If the important characteristic of a financial asset is whether or not it is "intermediated," there are only two types of financial assets.

If one undertakes to improve the concepts of "direct debt" and "indirect debt" by recognizing that the various types of "indirect debt" differ greatly in their ability to serve as a substitute for money, and that none of them (and no "direct debt") is a perfect substitute; if one admits that the various types of "direct debt" differ similarly from one another; if one agrees that some forms of "direct debt" substitute as closely for money as any forms of "indirect debt"—if in the interest of realism one undertakes to give effect to these distinctions, where does he find himself but back with something like the conventional analysis? He cannot interpret financial developments on the basis of changes in the relative amounts of "direct debt" and "indirect debt." He cannot argue that the objective of government financial policy is to control the total volume of "indirect debt." The unrealism of the authors' analysis of financial assets appears to be not an incidental matter, but the basis of their unorthodox theory and conclusions.

III. *Applications of the Gurley-Shaw Theory*

The authors' main applications of their theory, other than to questions of economic policy, are to analysis of the long-run rate of growth of commercial banks and to changes in long-term interest rates.

The "retrogression" of commercial banks. Gurley and Shaw argue that the commercial banking system has "retrogressed," that "banks have lagged be-

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[20] One factor that the authors bring up is the role of insurance in obligations of some intermediaries. I should think that the increasing use of insurance (including that implicit in pension systems) is better taken into account, not by treating the reserves of insurance companies and pension systems as a money substitute, but rather by considering these systems as among the many institutional, social, and economic factors affecting the needs against which people find it appropriate to protect themselves by the holding of various types of financial assets. There are two reasons: (1) The size of insurance reserves is a poor measure of the protection afforded by the insurance system: pure insurance, such as term life insurance and health insurance, involves only small reserves. (2) In the absence of the insurance, most people would hold other financial assets rather than money against the risks involved.

hind the pace of general financial development," and they suggest that this is likely to continue in the future.[21] Therefore, financial control through banks becomes less effective. This is one reason why similar controls should be ap-plied to other financial institutions.[22]

Whether one can accept this argument depends upon whether he accepts the Gurley-Shaw theoretical framework. The argument arises out of the authors' theoretical innovations rather than new factual discoveries. If one agrees that commercial banks are functionally identical with financial inter-mediaries and that demand deposits play the same role in the economy as other types of "indirect debt," then banks have retrogressed. They have grown less rapidly than some, and the total of all, financial intermediaries. On the other hand, if one believes that commercial banks and demand deposits play a unique role in the economy, then they have not retrogressed. In rela-tion to the size of the economy as measured by total outstanding debt or by national product, they have maintained or improved their relative position.[23]

Gurley and Shaw argue not only that banks have retrogressed, but that their retrogression was caused by the rapid growth of intermediaries. The rapid growth of intermediaries meant that the increase in the money supply consistent with economic stability was smaller than it otherwise would have been, and thus the growth of banks was restricted (by the central bank, or by the public through the mechanism discussed above). Again, on the basis of the Gurley-Shaw theoretical framework, this is plausible (though in interpreting the past it is not realistic to assume that economic stability always was main-tained, or that changes in the money supply carried the full burden of such stabilizing force as was exerted). However, if one does not accept the Gurley-Shaw theoretical framework, he must have reservations. He would agree that disproportionate growth in the liquidity-enhancing services of savings banks and savings and loan associations could tend to some extent to reduce the money supply consistent with a given income level, but he would want to

[21] "Aspects," pp. 515, 522-23; "Intermediaries," pp. 260-62, 269, 273-76.

[22] "Aspects," pp. 536-38.

[23] The ratio of demand deposits and currency to gross national product, despite its decline since the end of the war, remains larger than during the 1920's, and apparently larger than in any earlier period. The data given by Gurley and Shaw on the behavior of the money supply as they define it (including time deposits at commercial banks, which have grown less rapidly than demand deposits since the 1920's) indicate that it was larger in relation to total outstanding debt during the postwar period than at any earlier time and as large in relation to income as during any period except the preceding years begin-ning with the 1930's ("The Growth of Debt and Money in the United States," p. 258).

Thus if monetary policy is most effective when the money supply is large in relation to other financial magnitudes, we might argue that monetary policy should have been more effective during recent years than during most of our history, and that it should have been unusually effective during the 1930's. A more convincing argument probably can be made to the opposite effect, viz. that monetary policy is most effective when the economy does not have excess liquidity and when money balances are being intensively utilized. Actually, what is relevant is not the size of the money supply as such, but the potential responsiveness of the economy to such short-run variations in the money supply as it it is feasible to produce by monetary policy. This depends upon a good many factors.

treat this in the same manner as disproportionate increases in the supply of Treasury bills and savings bonds, which are quite left out of account in the Gurley-Shaw analysis. On the other hand, he might want to argue that disproportionate growth in the assets of a financial intermediary such as the social security system, a compulsory retirement system applicable to low-income groups, would tend to increase rather than decrease the money supply consistent with a given income level: it is generally believed that the net effect of the early growth in assets of such a system is contractionist rather than expansionist in its effect on the economy. In the framework used by Gurley and Shaw, the assets of the social security fund serve as dollar-for-dollar substitutes for money: both are "indirect debt." Analysis within the conventional framework would seem to lead to an interpretation of the past behavior of the money-income ratio quite different from the one that results from use of the "direct-debt" "indirect-debt" approach.

One who did not accept the Gurley-Shaw theoretical framework would not be disposed to agree that banks have "retrogressed" in a sense that is meaningful or that raises important problems for monetary theory or policy. That is not to say that he would not want to keep account of changes in the volume of close money-substitutes created by financial intermediaries, by the Treasury, and by private borrowers, and of the possible need to use debt management policy and perhaps other measures to offset any unstabilizing variations that occurred in the volume of highly liquid debt in the economy.

The Gurley-Shaw theory of interest. The authors advance and put to a test a theory that explains changes in long-term interest rates as the consequence of variations in the relative amounts of "direct debt" and "indirect debt." The public is represented as having some "desired direct finance ratio" that can be assumed not to change significantly when averages for cycles are considered. When the actual "direct finance ratio" is large (which means that it is larger than the "desired direct finance ratio"), long-term interest rates rise, and when it is low they fall. The theory is supported by a finding that for the period 1898-1930 (more recent periods were not tested) there is a very high correlation between actual "direct finance ratios" and changes in long-term interest rates (averages in each case for 3- to 7-year periods each of which is supposed to include a cycle).[24]

Unfortunately the fact that A and B are closely correlated does not necessarily prove that changes in A are caused by changes in B, or that changes in B are caused by changes in A. In this case, I do not believe that we could accept the Gurley-Shaw theory as presently formulated, even if the extension of their test to include the 25 years that have passed since the period on which this correlation is based produced equally favorable results. Their theory conflicts with some things that are quite as factual and relevant as the data on which their correlation is based.

A basic difficulty with this theory is that it requires that the volume of obligations of intermediaries be determined externally, so that people can find

[24] "Intermediaries," pp. 269-71.

themselves with a greater or smaller volume than they want to have. In fact, the volume of such obligations is determined quite immediately and directly by the holders of them. Most intermediaries (compulsory pension systems are an exception) cannot maintain outstanding any more of their obligations than people want to hold, nor would they keep the volume below what people want to hold. They are responsive to coustomer demand, and within rather wide limits the terms on which they are willing to offer their obligations are little affected by the volume of demand for them. Thus, it does not seem that short-run changes in interest rates can be explained by changing discrepancies between the desired and the actual "direct finance ratio." It does not seem possible for such a discrepancy to exist (except in the case of demand deposits and compulsory pension systems).

A second type of problem, as suggested above, is that the "direct-debt" "indirect-debt" framework is inconsistent with a weighty body of evidence on the motives that govern individuals and institutions in allocating their funds to alternative financial assets. If there were no major shifts in the internal composition of either "direct debt" or "indirect debt," we might still be able to make something of an analysis using them because of a probable difference between the two types of obligation in average liquidity, or substitutability for money. In fact, however, the average composition of the categories has changed significantly in the short run. For example, in the 1928-30 period an unprecedented decline in the relative position of the banking system was largely offset in its effect upon the total "indirect finance ratio" by an equally unprecedented increase in the relative importance of investment companies. The short-run substitutability of bank deposits and shares in investment companies being what it is, it is difficult to believe that this sort of shift is without significance. Similarly, if the analysis were extended up through the depression and war periods to the present, one would want to take account of the effects upon financial positions, the interest-rate structure, and long-term interest rates, of debt management operations shifting liquidity of the government debt. But within the authors' framework such changes in the composition of "direct debt" cannot be taken into account.

As an example of the interrelations among the variables affecting interest rates, which are such that the correct line of causation cannot readily be inferred from the existence of a correlation, we could take the periods 1915-21 and 1922-24, the two extreme points in the scatter diagram, upon which the authors' correlation depends rather heavily.[25] The Gurley-Shaw theory asserts that long-term interest rates rose during the 1915-21 period because people were getting less than their usual proportion of "indirect debt" in relation to "direct debt." A more orthodox explanation would be that the rise in interest rates was caused mainly by the unusually rapid increase in the volume of outstanding debt associated with the financing of the first world war. The "indirect finance ratio" presumably fell because an unusually large proportion of the new debt was government debt, which people were willing to hold

[25] *Ibid.,* p. 273.   •

directly, and because the distribution of wartime saving and securities purchases was what it was. Similarly, during the succeeding period, the adjustment from the financial conditions of the war period together with the relatively slow increase in outstanding debt could explain both the high "indirect finance ratio" and the decline in interest rates. Undoubtedly, the true explanation of interest rate changes in these years is not as simple as this; the example may serve to suggest that it is probably both more complex than and different from what Gurley and Shaw have argued.[26]

IV. *Conclusions*

The theoretical innovations upon which Gurley and Shaw have built their argument are radical. If we accept them, we should undertake a drastic reconstruction of banking theory, debt management theory, financial analysis, and the theory of economic stabilization. I cannot, however, see that Gurley and Shaw justify their theoretical innovations by the arguments and the facts that they adduce in these articles; indeed, the effort that is given to attempting to support them is scarcely proportionate to their importance or to the accumulated weight of argument on the other side. If the authors would continue to build on their unorthodox theory, they should offer a more thoroughly worked out position in support of it.

On the other hand, if the authors cannot convince us of the validity of their theoretical innovations, their main argument and conclusions are untenable. Then, the banking system has not "retrogressed." Special quantitative control over the volume of demand deposits continues to be indicated. There is no reason to apply the same type of control to obligations of intermediaries, or to attempt to control the volume of "indirect debt" as such.

I am not arguing that present monetary theory is perfect, that liquidity theory can stand no revision, or that the activities of intermediaries are without relevance to the problem of economic stabilization. There is no denying that financial analysis must go beyond the money supply and take account of changes in the structure of outstanding investment assets, including obligations of intermediaries. The question is whether the Gurley-Shaw theoretical framework is a valid basis for the inquiry, and is an improvement over our accustomed tools of analysis. It seems to me that, on the contrary, it represents a step backward.

J. M. CULBERTSON*

[26] In "The Growth of Debt and Money in the United States," the authors use their theory as the basis for a model of long-run money growth, which they test against past United States developments. Evaluation of this analysis would be a separate job and is not attempted here. I do not believe that the authors' findings in this paper can be argued to establish in any definite way the validity of the theoretical concepts under discussion here, although the paper does provide another example of an analysis based upon their theory that is consistent with a certain body of data.

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Reply

Culbertson's main points appear to be: (1) we fail to treat money as a unique financial asset; (2) our concepts of direct debt and monetary and nonmonetary indirect debt are unrealistic, theoretically inept, and inferior to the concepts of liquidity and illiquidity; (3) our theory of interest rests on the objectionable division of assets and debt into direct and indirect forms, and it assumes disequilibrium positions, which rarely appear, in the market for the obligations of nonmonetary financial intermediaries; (4) we do not appreciate the unique position of commercial banks among financial institutions; and (5) in view of the above failings, our suggestion for extending controls over indirect debt generally is unsound. Our comments follow this order.

1. Money *is* unique in the sense that no other financial asset is exactly like it. But as much, or rather as little, could be said for any financial asset. The important consideration here for monetary, interest, and price-level theory is how the demand for money is affected by demand for and terms of supply of other financial assets. And on this point we share the general belief with Culbertson that money has many close substitutes. He cites as examples treasury bills, time deposits, savings bonds, and savings and loan shares. To the extent that such substitutes are available, then, the demand for money is less than it otherwise would be. This is the essential point: the "uniqueness of money" is a red herring.

2. The manner in which financial assets are to be grouped or ranked depends on the problem. A way of grouping financial assets that is appropriate for one array of problems does not preclude other ways that bear on different problems. Our main interests are in analyzing the relationship between real growth and financial growth, in isolating the function of intermediation for special study, and in considering the relative roles during the growth process of monetary and nonmonetary intermediaries. Since the accumulation of direct (or primary) debt almost inevitably accompanies real growth, and since a principal function of the monetary system and other financial intermediaries is to purchase direct debt from and issue indirect debt to nonfinancial spending units, the framework of direct and indirect debt seems admirably suited to our interests.

We do not object to other schemes for classifying financial assets. The difference between equity and debt, ambiguous though it is, can be useful in studying business organization, business incentive and motivation, or stability of business income and spending. The difference between short-term and long-term securities, arbitrary as it is, is relevant to study of the interest-rate structure. A ranking of financial assets along a liquidity spectrum may be useful in explaining asset-holders' behavior on the commodity markets, which may or may not be the problem Culbertson has in mind (he never says) when he advocates such a procedure. All of these methods can be useful, but to imply, as Culbertson does, that one has to settle once and for all on money-bonds, liquidity-illiquidity, direct-indirect, or some fourth or *n*th grouping and stick with it through thick or thin without stipulating the purpose for which it is used, is to miss the mark completely.

Our classification implies that we are interested in relative supplies of direct and (monetary and nonmonetary) indirect debt and in the public's choice between these financial assets. On what basis is this choice made? With a few reservations (*e.g.*, "irrational behavior"), Culbertson believes that it is made solely on the liquidity-illiquidity basis. We disagree. The term liquidity preference suggests that allocations of funds to money balances are determined by the marginal advantage of liquidity over illiquidity. But in highly differentiated security markets, this is only a part of the story. As we have used the term, diversification (which is "portfolio balance" for others) is meant to call attention to the fact that money is not desired just because it is more liquid than something else, and other financial assets are not desired simply because their illiquidity has been overcome by the bond rate of interest. Rather, a particular stock of money is wanted because the marginal dollar in money balances provides more utility, in terms of liquidity, than the same marginal dollar would provide, when invested in other financial assets, in terms of income, liquidity, corporate control, patriotic satisfaction, insurance, or dozens of other portfolio attributes. Liquidity preference does not do justice to the complexities of choice between money and more or less highly differentiated financial assets.

After all, economists do not reason so narrowly when it comes to the allocation of funds in other ways. They do not have an "intoxication-preference" theory of liquor; a dollar may be allocated to liquor because of its capacity to intoxicate, but if instead the dollar is allocated to clothing it is not because the price of clothing has overcome the lesser ability of clothing to intoxicate. "Intoxication preference" would not do justice to the intricacies of consumer choice in allocating the consumption budget, and by the same token liquidity preference falls short of explaining how much money is desired in financial portfolios.

We group financial assets to fit our problems and then explain the public's choices on the basis of the many attributes—liquidity being only one—that highly differentiated financial assets have. Direct debt may become more or less attractive, for many reasons, relative to monetary or nonmonetary indirect debt. A changing mixture of direct debt hence can affect demands for indirect debt and so the growth of financial intermediaries. That we have taken this into account is both acknowledged and denied by Culbertson (pp. 129, 130 and note 18). Moreover, the usefulness of our framework, for studying the conformation of financial growth, does not require, as Culbertson insists, that the public think in terms of "direct" or "indirect" assets. People choose between assets according to their (explicit and imputed) yields and prices and, accordingly as this choice favors assets we choose to call indirect, financial intermediaries experience relatively rapid growth. Culbertson credits liquidity-preference analysis with "definite logic," though he would hardly be prepared to allege that the terminology of liquidity-preference analysis was taken over by economists intact from the market place.

When indirect debt is broken into its monetary and nonmonetary components, it is clear that the public's choices between direct and indirect assets have significant consequences not only for financial intermediaries generally but also for the monetary system. Depending on the relationship between

direct debt, nonmonetary indirect debt, and money—whether they are substitutes, complements, or unrelated goods—an increase in the supply of the first two lowers, raises or does not affect the demand schedule for money. If one is interested in explaining the growth of all financial intermediaries including the monetary system, as real growth generates direct debt, our tripartite framework is useful whatever the relationships among the three assets. Culbertson is wrong, then, in saying, "In order to justify using the concepts of 'direct debt' and 'indirect debt,' then, the authors must show that all types of obligations of [nonmonetary] intermediaries substitute for money to a greater degree than do all types of 'direct debt.'" If this special relationship did hold, it would mean one thing for the growth of the monetary system and of other financial intermediaries. Other possible relationships, including complementarity between money and nonmonetary indirect debt, would yield other results. The usefulness of the analytical framework does not depend on any particular set of relationships, as we attempted to point out in "Aspects" (pp. 528-29).

3. Culbertson offers two criticisms of our comments on the theory of interest. To make these clear, suppose that there are four goods in the economy, commodities (A), direct debt (B), money (C) and nonmonetary indirect debt (D). Interest theory bears on the price of B. Culbertson's first argument is that the A-B-C-D framework is unrealistic because the composition and average liquidity of B and D change over time, and these changes cannot be taken into account within our framework. Since we have taken them into account in our published work, though no doubt imperfectly, and since Culbertson is aware of the fact in some degree (note 18), we turn to his second argument.[1]

The second argument is that shifts in demand schedules for D, or changes in amounts demanded as a result of shifts in supply schedules, cannot lead to excess demands or supplies at prevailing yields in B and C unless the market for D moves into disequilibrium (p. 130). And since issues of D are apparently tap issues, this market cannot be in disequilibrium. Hence changes in amount of D demanded cannot affect the rate of interest on B—or the price level on A. This argument misses the point that an increase in demand for D may be partly at the expense of money and so create an excess supply of money (and an excess demand for direct debt, as nonmonetary intermediaries bid for it), even though the market for D is in equilibrium.[2] Given a budget restraint on the public's acquisition of financial assets, an excess supply of money and

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[1] Reference to changes in the quality of direct securities and to the impact of such changes on demand for money may be found not only in Culbertson's own citations to our work but also, for example, in "The Growth of Debt and Money in the United States, 1800-1950; A Suggested Interpretation," *Rev. Econ. Stat.*, Aug. 1957, XXXIX, 252 and 259. Studies nearly ready for publication explore this matter at length. The article cited above is even more explicit on the subject of changes in the quality of nonmonetary indirect debt. See, for example, pp. 252, 255, 259, and 261.

[2] Under given conditions of equilibrium on markets for goods and bonds, let the demand for money be $100 and the demand for savings and loan shares nil. Assume a shift to the left in the money-demand function, a shift to the right in demand for shares so that money demanded is $90 and shares demanded are $10. Money-holders turn the $10 in

an excess demand for bonds may "originate" in D when that market is in partial equilibrium.[3]

4. Culbertson claims that commercial banks are unique among financial institutions in several ways. First, they create money. We have to agree with that. Second, they create loanable funds, while other financial intermediaries are mere middlemen in this respect. As Culbertson defines loanable funds, we also have to agree with that. His account of how banks create loanable funds—"acquiring additional debt by creating demand deposits" (p. 121)— is only a reiteration of the first point above, that banks create money. Whatever the combination of words one uses to make the point, banks do create money.

But something more than semantics is involved here. Culbertson considers it a piece of whimsy on our part that we define loanable funds as savings, in the sense of income earned and not spent on consumption, of an increase in savers' net worth and of an increase in their net financial assets. He prefers the conception that loanable funds are *payments of money* to investors in capital goods. These payments, for which investors issue direct debt, derive from two sources. One is "private act[s] of saving" (p. 122), and money flowing from this source may flow directly from saver to ultimate borrower or indirectly through nonmonetary financial intermediaries and bank time-deposit departments. The second source of moneyflows to investment is money creation by the banking system, through its demand-deposit departments, on its own initiative and with no by-your-leave to any saver. This definition, Culbertson indicates, is a piece of "existing banking theory" (p. 122), and we regret that he is probably quite right in thinking so.

In Culbertson's image of the saving-investment process, saving is the act of paying out money, and savings are decreases in money balances accompanied by accretions of nonmonetary assets. One saves by getting rid of money, and money rejected by savers *plus* money created by banks are the loanable funds that finance investment.

If we interpret Culbertson correctly, that the saving process which finances investment consists of replacing money balances with "nonmonetary debt" (pp. 121, 122), it must be concluded that his views on saving are disastrous for monetary theory and are incompatible with income and employment theory. On his rule that savers repel money, holdings of money can be no part of

money balances over to the savings and loan associations, receiving in return $10 in shares. The public's demand for bonds is not changed; the banks' demand for bonds is not changed; but the savings and loan associations express a demand of $10 for bonds (assuming the associations wish no reserves) that is excess demand at the initial bond rate of interest. The market for shares is continuously in partial equilibrium, but it "originates" a general disturbance in markets for money, bonds, and possibly goods.

[3] Culbertson advances a third line of attack against our views on interest theory by comparing our interpretation of interest-rate behavior in 1915-1921 with a "more orthodox explanation" (p. 130). The rise in rates over these years he attributes to "the unusually rapid increase in the volume of outstanding debt." We are wondering how this version of "orthodoxy" would account for the fall in rates during 1940-1945 when the volume of outstanding debt rose even more rapidly. For a monetary economist, Culbertson is remarkably reticent about the role of monetary policy as it affected interest rates in 1915-1921.

the desired increments in net financial assets that balance excesses of income over consumption. Savers do not want money, and investors want it only to pay it out on the commodity markets. In short, there is no demand for money, and all money is excess money. In their own distinctive ways, J. B. Say and J. M. Culbertson have wrought havoc with the money-demand function. Say would have savers content with any amount of real balances. Culbertson would have savers demand no real balances. According to the earlier of these landmarks of monetary theorizing, the price level is indeterminate: according to the latter, money is desired by neither savers nor investors, there is no demand for money, money is valueless, and the price level is infinite.

The profession of economics has two conceptions of the saving-investment process. In income and employment theory, savings are a difference between income and consumption, a rise in savers' net worth, a gain in their net financial assets. The net financial assets are tickets of participation in investment, supplied to savers by investors. On this definition, savings may take the form partly or wholly of *increases* in money balances (one variety of ticket for savers to accumulate), and the monetary system as an intermediary transmits real purchasing power over current output (*not* money balances) from savers to investors. This is the conception of saving-investment that Culbertson, with us in mind, dismisses as "their terminology" (p. 122). In Culbertson's "existing banking theory," savings are *decreases* in money balances of savers and increases in savers' nonmonetary financial assets, and the monetary system has unlimited power to enlarge the flow of money to investment. In the former view, savings are always equal to investment *ex post*, and there may be equality *ex ante* when the money supply is rising, falling, or unchanged. In the latter, savings are equal to investment only when the money supply is valueless. "Existing banking theory," one gathers, has not been touched by developments in income and employment theory these past twenty years and more. For so long as savings are interpreted by (financial) economists as increases in any asset but money, and by (income-employment) economists as the margin between income and consumption, little progress can be expected in relating the world of finance to the world of goods.

Culbertson offers a second reason for thinking that the banks are unique. They are not inhibited by "the public's decisions about holding demand deposits" (p. 123). True intermediaries, on the contrary, "cannot control the volume of their outstanding obligations" (p. 120), and the total of their debt is determined for them by savers. In short, there is no relevant demand function for money and no relevant supply function for other indirect financial assets.

As a monetary economist, Culbertson is impelled to qualify the dictum that demand for balances is irrelevant to the money supply. It is not relevant in a way that is "direct" or "immediate" or "natural," and it is not determining unless the central bank is "omniscient" (p. 123). "In a sense," the authority's choice of a price level and the public's choice of balances at that price level inhibit the banks, but then the "mechanism" of inhibition is not the same as that to which other intermediaries are subject. The authority's choice of price level and the public's choice of balances at that price level

indicate the money supply that the banks "ought to create . . . but this is quite another matter" (note 9). It worries Culbertson that his logic deprives the market for money of a demand schedule, but he does not retract the view that banks are unique in being able to create as much money as they like.

It worries Culbertson, too, that his contrasts between banks and other intermediaries deprive the market for the latter's debt of a supply schedule. After stating categorically that intermediaries "cannot control the volume of their outstanding obligations," he adds, "although they can influence their sales, by altering quality of service, intensity of advertising, or profit margins" (p. 00). If this means anything, it means that the volume of debt produced by these intermediaries is determined by a supply as well as by a demand function and that the intermediaries are not, after all, mere taps to be turned on or off by the saving public.

The supply function, then, is not wholly determining on the money market. The demand function is not wholly determining on the market for indirect financial assets. Moreover, Culbertson concedes, the latter assets can be substitutes for money (note 7 and p. 129), and their creation can result in excess money. When Culbertson has finished with these concessions, what remains of his view that the banking system is unique and that our conception of the banking system is "radical"?

A decade ago, when stabilization of long-term interest rates was the goal of monetary policy, it was frequently said that the Federal Reserve had "lost control" of the money supply. But it loses control in precisely the same way when it pursues a policy of price or output or employment stabilization. The money supply is a "tap" issue, adapted to a policy goal and to the demand for money in a way compatible with that goal. If the monetary system is unique, the reason cannot be that its output is independent of demand while the output of other intermediaries is controlled by demand.

5. Under present financial arrangements, banks are subject to a form of control that is not applied, in peacetime, to other industries, financial or nonfinancial. One raw material, "reserves," is rationed or allocated to the banking system. The input coefficient relating reserves to money output is specified, as a reserve requirement. These direct controls define production capacity which, the assumption goes, is always short of output that the banks would wish to produce: the banks are a "disequilibrium system." Such controls may be familiar to the banking system alone in peacetime, but few industries escape them under wartime conditions.

We have suggested that comparable direct controls should be applied to the supply function of other financial intermediaries. In some degree, the debt they create is a substitute for money. In some degree, hence, their expansion results in excess money. Perhaps such expansion can and should be restrained—by rationing of reserves and stipulation of a reserve requirement, so that the entire brunt of protecting bond and commodity markets against excess demands will not be borne by the monetary system.

Culbertson does not approve this suggestion. First, he interprets it to mean that we would exempt the banks from control, lifting the harness of direct controls onto other intermediaries (pp. 124, 131). That is not our position. The

banks can create excess money, and the fact that other intermediaries do it too is no excuse for lifting bank controls.

Second, he interprets the suggestion to mean that, in our view, a dollar's worth of expansion in any nonmonetary indirect financial asset has the same inflationary potential as a dollar's worth of monetary expansion. That is not our position. Our proposed equation of demand for money explicitly provides for varying rates of substitutability between money and other financial assets, both direct and indirect.

Third, he interprets our suggestion to mean that we would not be concerned with effects of the changing composition of direct debt on excess money (pp. 129, 130). We have repudiated this interpretation at an earlier juncture in the present paper. In fact, we consider public-debt management, held in high esteem by Culbertson, to be a prime illustration of the case we are making—and of the case that Culbertson is condemning—for financial controls in addition to monetary control. Replacement of Treasury bonds with Treasury bills tends to reduce demand for money, and complements easy-money policy. Funding does the reverse, and complements tight-money policy. It mystifies us that Culbertson should write so fondly of public-debt management and, at the same time, regard with horror the notion that management techniques should be applied to other assets than Treasury bills that displace money in some degree (pp. 120, 121).

It may be appropriate, in connection with this discussion of debt management, to suggest that Culbertson is astride two theoretical horses in his essay and is riding rapidly in opposite directions. Through his page 124, a Treasury bill would not partake of the uniqueness of money. It would resemble a time deposit or a savings and loan share as a savings medium, with its volume outstanding depending on demand by savers. As a vehicle for matching planned saving with planned investment, it would be noninflationary in character. Since the public gives up money to get a Treasury bill, bill issue would be contingent on a private act of saving, a private act of abstention from demand for goods. On the first set of principles enunciated by Culbertson, bill issue should be as free of management as issues of all other assets that do not qualify as money in the sense of means of payment.

Culbertson mounts his second steed on page 125. Henceforth savers save by *holding* money as well as other financial assets. And money, no longer unique, differs from other assets only in the degree of its liquidity. Its "liquidity-enhancing services" (p. 128) place it at an extreme of the liquidity spectrum of assets, but this position is not so distinctive that such fairly close substitutes as Treasury bills should escape the network of financial control. For Culbertson remounted, financial intermediaries create close money-substitutes (p. 129). And now he would "want to keep account" of changes in the volume of these substitutes and of the possible need for regulating them not only by public-debt management but by other devices as well. This is the horse that we too are riding. We welcome Culbertson aboard.

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BOOK REVIEWS

General Economics; Methodology

Mathematical Economics. By R. G. D. Allen. (New York: St. Martin's
Press. London: Macmillan. 1956. Pp. xvi, 768. $10.00.)

Economists must be grateful to Professor Allen for the tremendous effort
this book represents. The task it essays of presenting the major branches of
economic theory as well as their associated mathematics is formidable indeed.
It can be questioned whether this is a reasonable ambition for one book.
Certainly there are some indications that what defects the book has arise
from a canvas which is too broad. Nonetheless, there is no doubt that such
a portmanteau volume will be extremely useful in spreading acquaintance with
the subject matter among people who do not have the opportunity or the
leisure to examine special texts in the particular fields. A large number of
problems for the reader to solve are liberally sprinkled through the book and
add substantially to its value.

If I have major criticisms to make, they are, first, that there is an insuffi-
cient attention to careful statement and scrupulous accuracy which irritates
and, for the uninitiated, may mislead, and, secondly, there is some failure to
remold the material as found in the literature so as to show more clearly the
unity of subject and method which should result from the common focus on
economic behavior in competitive markets.

I have thought that the greatest service I could pay both reader and author
would be to take up the chapters one at a time and mark the points of dis-
agreement which I have found. The widespread use which the book is likely to
have in graduate instruction for many years justifies this type of examination.

Before launching into this enterprise, however, a brief initial survey of the
field may be desirable. The message of the book is that there has been an
impressive transformation of economic theory in recent years, mostly since
the second world war, and a great expansion in the range of economic theory,
especially that part of it which seems operational. There are three principal
areas of development: aggregative theory along Keynesian lines, linear activity
analysis of production, and game theory applied to market structures. In the
first category fall the trade-cycle model of Samuelson, Goodwin, and Phillips,
and the growth models of Harrod and Domar, as well as the static equilibrium
models of Hicks and Modigliani. In the second category are Leontief's input-
output analysis, the linear programming of Dantzig, and the activity analysis
of Koopmans. The third category should include von Neumann's model of the
expanding economy, much of the recent analysis of competitive systems by
economists such as Arrow and Debreu, and the application of game theory to
situations of duopoly and bilateral monopoly by Nash and Shubik. However,
the third category is rather neglected in this book. Only the theory of games
proper and the von Neumann model are presented. The exciting explorations

of properties of competitive equilibrium, its existence, uniqueness aspects, and optimality properties, are not considered. Allen deliberately leaves aside the theory of decision-making, which is closely allied to game theory, and all types of sector analysis, except for the Leontief model itself. In view of the already excessive bulk of the material to be presented, it is hard to quarrel with these last omissions.

The discussion of dynamics in the first three chapters covers the single-commodity market, the multiplier, and the acceleration principle. The more complicated models of cyclical movement are reserved for later treatment following a series of chapters on difference and differential equations. In the first chapter, where the single-commodity market is treated, we find the traditional cobweb model and a discussion of dynamic stability along lines made familiar by Samuelson. An interesting feature of this discussion is the use of continuous models, which have continuously distributed lags, alongside the models with discrete lags. There is also a general discussion of distributed lags in anticipation of the more complex models of economic fluctuation designed by Phillips on which the emphasis in later chapters is very heavy. This discussion is somewhat marred by the particular form given to the continuous version of the cobweb. In the demand function the price and its rate of change appear with the same coefficient (p. 7), presumably negative. This means that a more rapid rate of price increase will reduce demand at a given price, an assumption which I find difficult to accept. There is also a paragraph on general features of these simple dynamic models which analyzes the meaning of *ex ante* and *ex post* as they are used in the Swedish literature. I am not sure that these notions are much needed in the discussions which follow.

In the second chapter simple Keynesian models like those of Hicks and Modigliani in which the multiplier is a central feature are described at some length. These are highly aggregated general equilibrium models which contain a commodity market, a money market, and a labor market. Commodity demand is divided between consumption and investment. These models differ from later cyclical models on two counts. They contain a money market, and investment depends on current output and on the interest rate, not on an accelerator effect. Moreover, they seem better adapted to comparative static analysis than to a full dynamic development with lag patterns. I think it is unfortunate that the Modigliani model is presented in the original form. Consumption spending is given as a function of money income (p. 39), but this has unusual consequences unless the price level is constant. No effort is made to introduce the Pigou effect, and the demand for idle balances is treated as a function of the interest rate alone without regard to the prices of goods. The question is not broached whether the system as given has a solution for the case of flexible wages (the "classical" case). It is in this chapter that the view, erroneous in my opinion, is first expressed that the Harrod-Domar growth model is defective primarily because it supposes saving plans to be fulfilled. The chapter concludes with a clear statement of the three lags in the circular flow of income described in a celebrated article by Metzler.

The second major principle of the cyclical models appears in Chapter 3 on the acceleration principle. An emphasis on continuous lags is apparent from the first. However, the role of uncertainty and expectation in creating the lag pattern is not given its proper place (p. 64). The acceleration principle is applied to the Harrod-Domar growth model. However, this model is treated, illegitimately I think, as complete and deterministic rather than as describing an equilibrium path which the economy is capable of pursuing on certain assumptions (p. 68). The next stage of elaboration is the Phillips model of the multiplier-accelerator. It has the peculiarity that the rate-of-investment increase depends on the current deficiency of capital stock (p. 72). The view with respect to the Harrod-Domar model referred to above is repeated here (p. 75 ff.). Alexander's formalization of Harrod's theory is nicely summarized. The Samuelson-Hicks model of interaction between multiplier and accelerator, where the lags are discrete, is clearly described and compared with the continuous Phillips model. My only query is as to an odd emphasis on outlays as compared with deliveries of goods in the interpretation of the variables which appear in these models. So far as I can grasp what is meant, this seems to me ill-taken.

The plan of the book alternates chapters of economic theory and mathematical preparation. Chapters 4 through 6 are the first group of mathematical chapters. They provide the background in difference and differential equations needed in the solution of models of cyclical change like those of Hicks and Phillips. On the whole these chapters are well done. It is sometimes not clear, however, to what level the argument is pitched, and there is a bit of repetitiousness. Rather more serious are a few lapses of rigor; for example, in the discussion of arbitrary constants in functions (p. 136). But these defects are compensated by the careful selection of topics and their lucid development. Perhaps the outstanding feature of these chapters is the application of the Laplace transform to the solution of differential equations. This technique is unfamiliar to economists, but it is very useful in handling the equations which arise in models with continuous lags like those of Phillips and Tustin. I confess that I find the discussion of the use of the variable p (pp. 170-75) a little mysterious.

Following this mathematical preparation the author returns to the main thread of his argument, and the next three chapters develop the theory of cyclical models and their damping by government action. The first chapter describes in greater detail the characteristics of the multiplier-accelerator model in period form as proposed by Lundberg, Samuelson, Hicks, and Metzler. There are excellent analyses in the literature on which to draw and the presentation is quite good. The dependence of solutions on values of parameters is illustrated by very helpful graphs. I wonder, however, if one should say that statistical evidence suggests that the "reduced power" of the accelerator (the acceleration coefficient minus the propensity to spend one period after the receipt of income) is less than one for the United States (p. 217). This would mean that Hicks' model is not explosive. The error made by Kaldor, that output is constant at the ceiling in Hicks' model, is

repeated. Of course, one must recall that the ceiling itself slants upward (p. 220).

In the next chapter some more complicated cyclical models are analyzed, the nonlinear model of Goodwin, the mixed difference-differential equation of Kalecki, and Phillips' model of economic regulation with continuous lags, including a lag of output behind demand, as in Lundberg and Metzler. The solutions of these systems are carried through and described with exactness and clarity. The author appears to favor the Phillips model (p. 242). However, the Phillips model is linear and thus cannot express the different mode of operation of the accelerator when output is expanding and when it is contracting. Only Goodwin handles this feature within his formal system. On the other hand, Phillips does not allow for the effect of the size of capital stock on investment, in contrast with Kalecki. It would seem to me that both these features are essential to a realistic model, and I am not convinced that the presence of capital stock in the model is equivalent to a lagged accelerator (p. 252). The importance of capital stock has been much emphasized also by Kaldor, who does not state his theory in mathematical notation. The strength of the Phillips model is the introduction of stabilization policies and their critical evaluation. Allen summarizes his results in this direction very nicely.

The third of this series of chapters deals with stabilization policy from the viewpoint of the engineers' theory of automatic control systems. It follows the work of Tustin and Phillips. All the earlier models are represented by the engineers' diagrams and the engineers' methods are applied to their solution. These methods are very powerful so long as the system is linear and the parameters are assigned numerical values. However, nonlinearities such as those in Goodwin's model are not so easily accommodated and qualitative results must be mapped out with particular cases. The advantages and drawbacks of the engineers' approach are very fairly stated by the author (p. 303 ff.). As one might expect, the approach is best adapted to a redesign of the system (by introducing government policy) to eliminate instabilities. This chapter is the high point of the first part of the book. There is a strong suggestion in it that the fruitful line of development for cyclical models lies in a further elaboration of the engineers' approach (pp. 311-12).

The remainder of the book, that is, about 400 pages out of 700, is devoted to static theory and its supporting mathematics. The dynamic theory culminating in models of the trade cycle springs from the Keynesian "revolution" of the 'thirties. These models represent a formal elaboration of the trade-cycle theory proposed by R. F. Harrod soon after the *General Theory* came out. The static theory, on the other hand, is both older and more recent. The analogue to Keynes is Walras who comes nearly 50 years earlier. But the Walrasian theory was renewed and developed in the late 'thirties by Hicks, and it was given an empirical application by Leontief in input-output analysis. Even these developments were overbalanced by the theory of games and linear programming which appeared in the middle and late 'forties. The result is that the second part of the book gives much the stronger impression of novelty and yet lies closer to the main stream of economic thought. The only comparable innovation in the dynamic theory is the use of the engineers'

analysis of control systems. But even here, it is a question of transplanting a technique developed in another context, while the methods and even the mathematics of game theory and programming were recently devised in response to the challenge of economic problems.

The first two chapters of the second part are concerned with earlier developments. Chapter 10 describes the general equilibrium system, essentially, of Walras, as extended by Hicks (and Allen) and Samuelson. The economic system is represented in a series of equations whose solutions are possible equilibria. The equations arise from utility maximization by consumers, profit maximization by producers, and equality of demand and supply (p. 321). It is assumed that the utility and production functions possess sufficient differentiability or smoothness properties. Although he sometimes corrects himself, Allen frequently leaves the impression that the consistency of this system can be determined by counting equations and variables (pp. 314, 316, 319, 322, 329). Of course this is not so. The attempt to straighten the matter out in a special section (10.4) is both confusing and incomplete.

In any case, not much that is significant can be said about the possibility of equilibrium unless the variables are assigned meaningful ranges in the various functions. Moreover, it violates the economic interpretation of the system to suppose the functions continuous and differentiable even over the restricted ranges of definition. I should conjecture that the equation system at the bottom of page 321, which purports to represent the economic system, will only be applicable to an economy in which every consumer can consume any amount of any good and supply any amount of any productive factor, in which every firm can produce any quantity of any output and use any amount of any factor, and where both consumers and firms always do, in fact, use or supply every good. The system does not allow for limitations on capabilities or tendencies to specialization for either consumers or firms. With the material which has been in the literature for some time this subject could have been handled much better.

There is an attempt (p. 320) to combine free entry and normal profits with the Hicks assumption of a finite number of entrepreneurs or firms. Since the firms must be supposed to own unmarketable resources, it is not clear what is meant by free entry and normal profits in this context. The relation between the linear activities model and the production function is sketched in this chapter. However, the author evidently does not recognize that the marginal analysis is defective in principle because it cannot handle specialization (p. 341). This defect is not overcome by merely noticing simple marginal inequalities.

Chapter 11 presents the input-output analysis of Leontief. It is not true that Leontief assumes only one factor (p. 343). This assumption has been used by others to justify fixed coefficients for the industries in the Leontief model, but Leontief himself makes the assumption of fixed coefficients, as did Cassel and the early Walras, without such support. Perhaps it should be mentioned, too, that one small piece of economizing does remain in the Leontief model (p. 361). The elimination of unprofitable industries prevents the operation of industries which do not make a net contribution to the

economy. There is a strange distaste shown for an expanding economy (pp. 366, 369, also p. 489), despite the examples of von Neumann's system and the Harrod-Domar model. Indeed, the author is so disturbed over the explosive propensities of the dynamic Leontief model that he assumes the matrix of coefficients of the interindustry flows to be singular (where the households form an industry). This implies that no net output, which might be saved and invested, is possible. Any accumulation of capital in one sector must be balanced by the decumulations of an equal value of capital in other sectors. As common sense would suggest, capital accumulation depends on consumer saving. This means that the household industry is "profitable." But then the flow matrix is nonsingular. Allen might have avoided these errors had he investigated the significance of the zero profit condition for the solvability of the Leontief system. The only significant behavior an economic system can have, if it is closed and linear, is either to remain in a stationary state, to explode, or to contract to zero.

There is now another mathematical interlude, of three chapters, in preparation for the more advanced topics of game theory and linear programming. While the earlier mathematics was drawn from analysis, the traditional mathematics of physics, these chapters depend chiefly on the algebra of vector spaces, with just a taste of set theory. The first chapter introduces vectors and matrices, or better, vector spaces and their linear transformations. Actually "linear transformation" never seems to be defined. I think the development might with profit have been a bit more systematic and formal. There is some suggestion (p. 385) that convexity is a topological invariant. Probably it is meant that convexity is preserved under linear transformations. Proofs are sometimes omitted despite their simplicity (for example, the proof that a set which contains all points lying between any two points of the set contains all convex combinations of points in the set, p. 386). The reader is thus deprived of examples of rigorous reasoning. The term "polyhedral" is introduced without definition (p. 387). A definition is given for the determinant which depends on a proof of consistency which is not supplied (p. 400).

The second of this group of mathematical chapters (Ch. 13) contains a sketch of matrix algebra. Again there are some errors. For example, in deriving the inverse of a matrix from the elementary operations which reduce the matrix to the identity there is no reversal of the sequence (p. 441). Also (p. 445) a square matrix is not decomposable because it has one zero element in the lower left-hand corner (unless it is 2×2).

Finally, Chapter 14 describes some of the mathematical (and economic) applications of vectors and matrices, such as linear equations, quadratic forms, and their uses in economics (stability of equilibrium and Leontief systems). There is a rather loose discussion of the transformation of a symmetric matrix to diagonal form by an orthogonal matrix (p. 470). I would make the general criticism of these chapters that they do not have the unity or the simplicity that might have been obtained had the notion of a vector space and its linear transformations played a larger role. The fact that an n-dimensional vector space is spanned by any n linearly independent vectors is barely introduced and then only in the third chapter. The linear trans-

formation is often referred to, but it is not defined, and there is no abstract development on the basis of it. On the other hand, the algebraic postulates satisfied by vectors and matrices as abstract entities are presented in some detail, even to the point of repetition. This approach seems particularly inappropriate when the major applications will be to game theory and linear programming where the algebra of matrices is of minor importance.

The remaining chapters are concerned with economic theory. Chapter 15 describes elementary game theory. I think this is one of the better chapters. However, it seems a bit quixotic to describe linear programming as a field of application for game theory. Chapter 16 presents linear programming. There is a brief consideration of convex sets toward the end of the chapter. The notion of a basis of linearly independent vectors is first introduced here. It is stated that a function achieves its maximum over a convex set at an extreme point (p. 555). Of course, this holds in general only for certain sorts of functions, in particular linear functions. The simplex method of solution is outlined. It would have been appropriate in a book on economic theory to develop the version of this method which computes at each stage the shadow prices which are implied by the basis. These are the prices for primary resources which give the basis activities zero profits. Then a new activity is introduced if (and only if) some activity earns a profit at the shadow prices. The activity chosen for inclusion is the most profitable, and the activity dropped is the one first squeezed out as the level of the new activity is raised. The shadow prices are more meaningful to the economist here than in the solution of the dual problem.

Chapters 17, 18, and 19 are dominated by the use of linear programming to deal with traditional economic problems. These are the allocation of resources, the theory of the firm, and the theory of value. The source of Chapter 17 is activity analysis, largely developed by Koopmans. The Leontief model is examined as an elementary case of linear programming (p. 572). However, as earlier, it is not noticed that a genuine allocative problem does exist even in this model, namely, to avoid wasting labor on industries which, as a group, cannot contribute net output. The price system solves this problem exactly as it solves the general efficiency problem for activity analysis. There is no set of positive accounting prices which will allow the offending industries to break even, unless some other industry earns profits.

On page 588 it is said that an output combination is efficient if and only if it is impossible to increase all outputs together. This is not so, since it may well be possible to expand one output and not any other, without encroaching on any other, so that the output combination is inefficient. The author seems to suggest that this (false) statement immediately implies that a positive normal exists to the output cone at such a point. However, that a positive normal exists for an efficient output is the fundamental theorem of the subject and has to be proved. There is a nice discussion of efficient facets (p. 590).

In discussing the dual problem there appears to be some confusion between ideas appropriate to linear programming and those of activity analysis. The basic theorem on efficiency prices is stated wrongly. The notion of efficiency is made relative to the accounting prices and it is said (p. 592) that activities

which are unprofitable at a set of accounting prices are inefficient (at these prices?). The confusion is increased by the implication that even some activities earning zero profits are inefficient. The dangers of vagueness are illustrated in the discussion of efficient allocation with limited resources which are not desired goods in themselves (p. 599). There is a loose statement of the role of accounting prices. Then in the following paragraph it is asserted with considerable emphasis that it may normally be expected that the number of activities and scarce primary factors will be the same. It is not easy to decide precisely what correct theorem is being adumbrated here, but it seems likely that the case of ordinary linear programming is in mind where by virtue of the objective function, which may represent net profit, there is, in effect, only one final good. When there are many final goods (or intermediate goods, if they appear explicitly) it would seem rather probable that more activities than scarce factors will be needed, at least for a randomly selected bill of goods. Consider the Leontief model, for example. This question is handled better on page 644.

The remainder of Chapter 17 discusses models of general equilibrium over time (with no attention to a very important reference [Malinvaud in *Econometrica*, Apr. 1953]). A summary of the von Neumann model which is given is rather misleading. It is not true that the model does not allow substitution in consumption. The industry producing labor can have alternative processes as well as any other industry. Indeed, the household activities can be interpreted to refer to customary living standards which permit a whole range of substitution in consumption. The isoquant of the household production process is then an indifference surface. There could be different processes for different types of labor. More serious than this misinterpretation is the complete omission of the restrictive assumption used by von Neumann to prevent his system from falling into sectors advancing at different rates. He assumed that every good was represented in every process as an input, or an output, to at least some small amount. Without some such assumption the system may have no solution, and if the assumption is weakened, the growth rate may not be unique.

The extent of the revolution in traditional economic analysis is strikingly shown in the following two chapters which take up two main themes of static theory, the firm and the consumer. Most of the space is used for applications of linear programming. The traditional theory is given, but it occupies few pages. However, it is presented in very traditional form. There is no trace of the great simplifications suggested by Samuelson's logic of maximization. There is the old-fashioned rash of determinants which we now know to be superfluous. The problems of programming the firm with certain fixed factors and prices for its products is described along the lines laid down by Dorfman. It is stated in the course of this discussion that if a factor is used up to its limit of availability its price is positive (p. 634). This price may be positive *or* zero. The same error is committed on page 645.

The second division of traditional static theory, the theory of consumer's choice, is treated in Chapter 19. As before, the traditional analysis, cloudy with matrices, and unillumined by revealed preference, is given first. One

might also have expected some use of the approach of Allen's *Economic Journal* article of 1950, which can be extended to replace the utility function in the theory by the minimal income needed to attain a given satisfaction level. The newer approaches also dispense with some of the assumptions of differentiability of the indifference surfaces. There is a mention of the revealed preference approach but it is brushed aside for reasons which are not clear (p. 670). Evidently the author thinks the strong axiom of revealed preference does not guarantee integrability. But this is true only in case the indifferent directions of change from a point fail to exist. Then, under appropriate continuity assumptions, the integration is performed but the result is what Little termed a behavior surface rather than an indifference surface.

The problem of measurable utility is discussed in terms of ranking differences and in terms of choices with risk. There may be some failure to emphasize that the ranking of differences is not correlated with the choice of commodity bundles and is therefore meaningless from the viewpoint of demand theory, and that a utility measure for choices with risk has no cardinal implications for choices with certainty. However, these qualifications are not entirely overlooked (p. 676). The final topic in the chapter is an application of linear programming to demand analysis. The technology matrix is extended to include activities which convert goods into utility. I am not certain that this device will prove useful.

The final chapter deals with the problem of aggregation. Nearly all the discussion is concerned with Theil's analysis of the statistical problems of linear aggregation. This represents a unique departure from the author's declared purpose of avoiding econometrics. There is little reference to the justification of aggregation by special circumstances, in demand theory, and for Leontief models, though this would have been closer to the style of the rest of the book. Also there is no discussion of optimal aggregation in the input-output model. The final three pages of the chapter purport to cover the field of welfare economics.

Glancing back over the great range of theory which this book essays, I think the second part (from Chapter 10) makes the better claim to scientific interest. I cannot but feel that dynamic model building in macroeconomics can be profitably done only in close contact with statistical data. It is econometrics and forecasting, or it is largely sterile. Of course, much of the abstract theorizing that has been done in simple models can be defended as a preparation for the econometric work, but the impression should not be given that these models have an independent interest. The same thing should not, in my opinion, be said of the general equilibrium and microeconomic theory of the second part of the book, largely, no doubt, because of the optimal properties which the equilibria possess, from the viewpoint of individual or social action. There is also a mathematical or logical interest in this theory which cannot be claimed by models borrowed from engineering.

<div align="right">LIONEL W. MCKENZIE</div>

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Introduction to Mathematical Economics. By D. W. BUSHAW and R. W.
     CLOWER. (Homewood, Ill.: Richard D. Irwin. 1957. Pp ix, 345. $7.00.)

 This book seeks to introduce the economist to mathematical economics. It
is not aimed at the beginning economics student, but one who is fairly well
advanced. The preface says: "We feel that our book is likely to be of value
mainly to individuals who already have considerable training in economics;
and it should be particularly useful to graduate students and teachers of eco-
nomics who have only a moderate proficiency in mathematics and who wish
to acquire a modest reading knowledge of mathematical economics" (p. viii).

 The first part of the book is called "The Economics," and the second part,
"The Mathematics." The mathematical part "has been designed to provide
just enough information about mathematics to enable the serious reader (who
has already reached a tolerable level of accomplishment in elementary alge-
bra) to understand the entire text and to work out a few simple exercises"
(p. viii).

 The economic section has two objectives. It illustrates some of the mathe-
matical techniques which economists use, and it presents a systematic devel-
opment of price theory. The section covers not only static equilibrium for
both the consumer and the firm, but also the problems of dynamic equilibrium
and the stability of these equilibria. Furthermore, it considers explicitly the
problems which arise if individuals desire to hold goods as stocks as well as
wanting them for consumption purposes, and some of the problems which
arise in achieving equilibria in the markets for many commodities simultane-
ously.

 The book does not, however, start by introducing the economist to the
mathematical solutions of familiar economic problems, so that the student can
get used to the mathematical presentation while he is dealing with problems
which he knows and understands well. The authors apparently feel that such
an approach would interfere with an orderly presentation of price theory, and
therefore consider unfamiliar economic problems from the beginning, with
the result that even an advanced student of economics will find the economic
content of the book quite difficult. However, the advanced student who is
prepared to master the mathematics involved should find the economic section
both stimulating and rewarding.

 This reviewer cannot, of course, claim to be an expert in the teaching of
mathematics, and perhaps it would be more prudent simply to say that the
mathematical section seems to cover the necessary ground in a very small
space. However, the increasing use of statistical and mathematical methods
in economics (as well as in the other social sciences) makes the problem of
the teaching of mathematics to economists a rather important one. A common
solution to this problem is the one followed in this book: the authors assume
that the reader knows only college algebra, and they present any additional
mathematics which are required for the economic or statistical material to be
discussed. The book devotes 140 pages to the presentation of the necessary
mathematical techniques. It starts with a relatively elementary discussion of
"Functions, Graphs, and Equations," and covers such topics as the "Stability
for Differential Equations," and the use of Lagrange multipliers for problems

of "Conditioned Extrema" towards the end. A student of college mathematics would take at least four courses to cover the topics involved. There is, to be sure, no indication of how long the authors expect their "serious reader" will need to master the material, but it is fair to presume that they do not expect even a serious reader to devote this much time to it.

This reviewer is very skeptical of the proposition that economics students can master mathematics more easily and quickly than mathematics students. If anything, one would think that economics students would need even more problems and drill, and this is one of the things which are foregone in the condensed presentation for economics students. Furthermore, the economics student who learns his mathematics in this way is constantly using mathematics which he has not fully mastered and which he does not understand well. This means that he is likely to be so worried about the mathematical problems which he encounters that he does not devote adequate effort to determining the economic meaning of the operations which he is performing, even if he would be capable of discovering the economic meaning were he to spend the time at it. Mathematics should be a tool which the economist uses in order to gain insight into economic problems. It is not always easy even for a competent mathematician to understand the economic meaning of a complicated analysis, and is very likely to prove impossible for anyone who is operating at the very frontiers of his mathematical knowledge.

The conclusion of this argument is that this reviewer thinks that economics students should learn their mathematics the hard way, in the mathematics courses offered in the mathematics department. Furthermore, if possible they should take some courses beyond the level of the mathematics which they expect to use as economists so that they will feel that the mathematical techniques which they do use are not too difficult. The authors of this book are probably in agreement with most of these ideas, except that apparently they believe that a "serious student" could learn enough mathematics from their book to be able to handle the economics which they present, while this reviewer doubts it.

DIRAN BODENHORN

University of Chicago

Economic Models—An Exposition. By E. F. BEACH. (New York: John Wiley & Sons. 1957. Pp. xi, 227. $7.50.)

This book is supposed to serve two purposes: (1) To introduce the good non-mathematical economic theorist to some of the mathematical and statistical problems of modern economics. (2) To transmit to the gifted student of economics some knowledge in this field.

Part I deals with mathematical models. The idea of variables and equations is introduced and illustrated with the help of some results of Henry Schultz' analysis of demand. Linear and nonlinear relations between the variables are discussed. Linear models are then treated in more detail and a Marshallian model of partial equilibrium is discussed and solved. The distinction between endogenous and exogenous variables is introduced. This leads to a discussion of the reduced form. Then a simple Keynesian macro model is introduced, and

the theory of these models is illustrated by the model of Colin Clark for the United States economy, 1921-41. Next comes a simple discussion of derivatives, and the introduction of nonlinear models. Stability of equilibrium is discussed for a market under partial equilibrium and various Keynesian models are introduced, especially the systems of Hicks and Modigliani. Dynamic models (*i.e.*, models which involve time) come next. A result of Henry Schultz which involves a trend is presented, as well as macroeconomic models by Domar, Samuelson, Littler. This leads to a discussion of linear differential equations. Difference equations are introduced in the next chapter and illustrated by the dynamic macro model of Harrod and the cobweb theorem, as well as the model of Samuelson for the interaction of the multiplier and acceleration principle. All the models in Part 1 are deterministic.

Part II deals with econometric models. The concepts of probability and of the probability distribution are given and illustrated, sampling, estimation, small samples and tests are introduced and illustrated. Next comes regression and correlation theory. Special attention is given to the technique of multiple regression. The concept of confluence analysis (R. Frisch) is introduced and illustrated. Autocorrelation is discussed and the tests developed by R. L. Anderson and von Neumann presented. Multiple relations among independent and dependent variables are discussed and the concept and technique of identification presented, according to the ideas of Haavelmo and Koopmans. These ideas are illustrated by Haavelmo's derivation of the consumption function for the United States and a theoretical model involving one static demand and one static supply function, given by Koopmans. The elaborate model of Klein for economic fluctuations in the United States, 1921-41 is briefly discussed and its criticism contrasted with the more successful models for the demand functions of various commodities constructed by Stone for England and Wold for Sweden, which use classical least squares regression. There are some indications of the relationship of model building and statistical verification to prediction and policy and some comments on difficulties in economic statistics.

There are many excellent exercises in all chapters and short but pertinent bibliographical notes, which should help the student in his endeavour to gain greater proficiency.

The book is well written and as indicated above contains a great many examples of mathematical and econometric research in economics. The mathematical and statistical ideas are perhaps presented too briefly, but the student or reader should gain a fair idea of their importance from the examples. On the whole it may be said that the author has achieved his goal fairly well, and a reader of mature economic knowledge or a somewhat advanced student of economics should learn a great deal which is useful and interesting about modern mathematical economics and econometrics from this book. It might be used in an advanced undergraduate or in a beginning graduate course. It is, however, a pity that the mathematical subjects of determinants and matrices and the economic applications in input-output models and linear programming have not been included. These subjects are not intrinsically more difficult than the subject matter of this book and their presentation would

have enabled the student or reader to get acquainted with some of the most
modern developments and most useful ideas and techniques in modern eco-
nomics. This omission can perhaps be repaired in a second edition.

<div align="right">GERHARD TINTNER</div>

Iowa State College.

Economics. By JOHN A. GUTHRIE. (Homewood, Ill.: Richard D. Irwin. 1957.
    Pp. xi, 537. $6.00.)

Professor Guthrie has prepared a well-written textbook on economics that
has much to commend it. The author's three main objectives are: (1) "to
present the core of economics in as compact a manner as possible"; (2) "to
make it as understandable as possible to the student"; and (3) "to point out,
wherever possible, practical applications of the theory that is being discussed."
He has succeeded in attaining these objectives, warranting the publication of
yet another textbook in this field.

The book logically divides into four sections: introduction (21 pp.); mone-
tary theory and macroeconomics (188 pp.); microeconomics (255 pp.); and
a final section devoted to special problems and issues (81 pp.). The resultant
product of 537 pages is considerably shorter than competing textbooks now
in favor (*e.g.,* Blodgett, 973 pp.; Samuelson, 753 pp.; and Harriss, 816 pp.).
The "opportunity cost" of such brevity is quite high in terms of completeness,
and accounts for most of the work's shortcomings.

The development of monetary theory is difficult to follow. The student is
told in Chapter 2 that "increases or decreases in total bank deposits may pass
unnoticed by the average person, although they may be of great significance
to his welfare" (p. 31). With little preparation other than this challenging
statement, he finds himself studying the theory and technique of controlling
the money supply, without the orientation deriving from an understanding of
the problems of fluctuating price levels, the subject matter of Chapter 5.

In discussing the limitations on the ability of a banking system to create
demand deposits, Guthrie notes that if banks keep excess reserves, total ex-
pansion will be reduced. No further explanation is developed, leaving the im-
pression that bankers follow such policies of their own volition. Concentrat-
ing on the circumstances giving rise to the holding of excess reserves would
be informative, and useful in development of the following chapters devoted
to monetary and fiscal policy, and business cycles.

Guthrie discusses open-market operations as a means of controlling the
money supply and regulating consumer spending, but does not analyze the
mechanism involved. He does not treat their effects on investment spending
via induced changes in the interest rate. It is worthy of note that in his de-
scription of the Open Market Committee he does not consider the significance
of its membership composition for the location of ultimate authority in our
monetary system.

In Chapter 5 a brief paragraph relates monetary policy and inflation
(p. 78), but deflationary policy is postponed for consideration in connection
with fiscal policy (Ch. 10). Careful study of this chapter reveals but one
pertinent statement on deflationary policy: "As we have seen, no one of the

four methods open to the Federal Reserve banks . . . is very effective in stimulating recovery from a depression" (p. 157). Examination of the earlier chapters reveals but two short sentences supporting this statement: one asserting that monetary policy appears to have been more effective in curtailing price increases than price drops, and the other observing that in the drastic price declines of the 1930's monetary policy was probably not very effective in alleviating the situation (p. 58). (The author had pointed out that the Federal Reserve did not have the power to alter reserve requirements until 1935 and did not raise them until 1936-37, p. 55). There is no further analysis clarifying the inability of monetary policy to deal with deflation.

Other theoretical gaps are apparent: for example, the effect of foreign investment on domestic income levels, the foreign trade multiplier, the theory of the balanced budget multiplier (mentioned but never detailed), and the theory of the substructure of demand.

The above criticisms are, however, relatively minor and can serve as points of departure for the instructor's supplementary lectures. The work contains many excellent features. The section on antitrust is especially clear and concise, displaying an admirable sense of the significant, and conveying a genuine appreciation of the nature and importance of current problems in the design and implementation of antitrust legislation.

The section on price theory is well done, incorporating succinctly much contemporary thought on oligopoly and monopolistic competition. The problem areas dealing with international policy, agriculture, economic systems, and economic growth are well developed and supplemented with adequate factual data to permit effective analysis.

Guthrie's pedagogical technique of opening each section with a topical outline and concluding it with an excellent summary of material covered will be greatly appreciated by both students and instructors. Its style is straightforward, lucid, and effective. As an introductory textbook designed for freshmen and sophomores, it merits serious consideration.

DON V. PLANTZ

University of Kansas

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Activity Analysis and the Theory of Economic Equilibrium. By H. MAKOWER. (New York: St. Martin's Press. London: Macmillan and Co. Ltd. 1957. Pp. xiv, 193. $5.75.)

For the past ten years anyone could embarrass me by asking, "Where can I find out about linear programming? And please remember I don't know anything about matrix algebra or that sort of thing." Now I have an answer. It is the first seven chapters of Dr. Makower's book. They are a clear, straightforward exposition of linear programming. They contain no mathematics beyond arithmetic and the kind of geometry economists are inured to. They put the emphasis where it belongs, namely on the formulation of the problem, its relationship to familiar economic concepts and formulations, and its interpretation. They omit the intricacies of solving linear programming problems by

the simplex method or any other method. And they include the major concepts illustrated by ingenious examples.

I have to confess, though, to one lurking reservation. Perhaps Dr. Makower makes things seem too simple. To achieve apparent simplicity an author must thread a narrow path between a bog of technical complications on one side and a bramble of inaccuracies on the other, and sometimes the path fades out completely, and a choice must be made between the two evils. When such choices arose she, probably wisely, tended to prefer the bramble to the bog.

Her most serious lapses in this regard resulted from her anxiety to avoid inequalities. Inequalities are a nasty and treacherous kind of mathematical relationship, yet they are essential to many economic problems. In the theory of consumption, for example, it is not true that total expenditure on consumption must equal the budget allowed, but only that expenditure cannot exceed the budget. Consumption theory has not, in fact, suffered much from pretending that the relationship between consumption expenditure and the budget restraint is an equality instead of an inequality—though the inequality has to be admitted when satiety sets in—but in any problem where there is more than one restraint the difference between equalities and inequalities becomes decisive. The most obvious example is utility maximization subject to both a monetary and a point-ration budget.

Dr. Makower's policy in face of this complication is to avoid inequalities at almost any cost. In Chapter 3, for example, inequalities threatened most dangerously to obscure the clear path of her exposition. She therefore arbitrarily wrote strict equalities in place of two of the inequality constraints that arose naturally in the example around which that chapter is written. This forcing of equalities, of course, simplified the exposition but also, by the same token, it narrowed the permissible range of choice and, quite possibly, excluded the true optimal solution to the problem. In addition, Dr. Makower wished to avoid the technical problems of handling the inequalities that remained. She therefore assumed that they also could be replaced by equalities, *i.e.*, that conditions were such that all resources were fully utilized in the optimal solution. But this assumption amounts to assuming that the problem is more than half solved in advance. One of the important aspects of a linear programming problem is the ascertainment of which resource limitations are genuinely restrictive and which resources are available in excessive supply. By suppressing this part of the problem Dr. Makower suppressed the whole issue of free goods. One remark on this topic seems to be in order.

Dr. Makower makes it clear that linear programming problems, like married people and complex numbers, always come in pairs. Whenever there is a problem of optimal resource allocation there is an associated problem of determining imputed prices for the resources and intermediate products involved. The intimate connection between allocation and value imputation is self-evident to economists. But what is not self-evident is that if the allocation problem is a linear programming problem, then the imputation problem is also a linear programming problem, called the dual of the allocation problem. There are a number of fascinating and revealing connections between any programming problem and its dual. One of them is the following: It is quite common in solving a linear programming problem to find that some of

the available activities are not used at all. The analogous behavior in terms
of the dual is to find that some of the resources have zero imputed price, *i.e.*,
are free goods. But, clearly, this can happen only when in the solution of the
allocation problem it turns out that the quantities used of some of the re-
sources are less than the quantities available. And this is just the possibility
that Dr. Makower has assumed away in the interests of simplicity. She has, in
short, assumed away part of the symmetry between allocation problems and
imputation problems. This is unfortunate, but even more unfortunate is the
impression conveyed that economic problems of the linear programming type
can be discussed adequately in terms of equality restraints or, at least, that
the use of inequalities is an inessential refinement and complication. In fact,
the recognition of·the importance of inequalities is the conceptual root of the
whole modern algebraic approach to economic analysis.

So much for the expository part of the book. The remaining two-thirds con-
sists, first of the most unqualified recorded defense of the linearity assumptions
of linear programming, and second of a series of essays in which this tool is
applied to a variety of problems in economic theory. I shall not be able to
comment on these ingenious and interesting essays except to note that they
clarify a number of hard-to-understand points but do not lead to any new
results. The attempt to justify linearity, however, is too important to be
passed over.

The linearity assumptions of linear programming have two aspects. First,
linearity in production, which amounts to an assumption of constant returns
to scale. Second, linearity in consumption, or, in more familiar vocabulary,
constant marginal utility. Dr. Makower's defense of constant returns to scale
in production is conventional. If successive doses of identical inputs do not
yield identical amounts of output it *must* be because of an additional, unmen-
tioned and perhaps unnoticed, input whose quantity has not been varied. Thus
the assertion of constant returns to scale becomes a tautology rather than a
substantive claim. Her treatment of constant returns to consumption is more
daring and original. She conceives of consumption activities, each of which
consists of consuming a certain bundle of goods in fixed proportions and as-
serts that the utility derived from such a consumption activity is strictly
proportional to the level at which that activity is carried on, *i.e.*, to the quan-
tity consumed of any of the goods in the bundle. She admits, naturally, that
"Common sense tells us that wants are satiable and that utility diminishes
with increased consumption" (p. 60). But, just as in the case of production,
this can be attributed to the presence of some unidentified consumption goods
whose quantities do not vary when the quantities of the identified ones do.
She rather despairs of discovering what these unidentified consumption goods
are, but I think she is likely to be driven to such concepts as "appetite." I can
conceive that if I am twice as hungry on Monday as on Tuesday I can de-
rive twice as much satisfaction from two pounds of steak on Monday as I can
from one pound on Tuesday. But such semantic refuges can serve only to
save a form of discourse, and permit us to state sensible things in paradoxical
ways. It seems better to admit nonlinearity and to stick to genuine and ob-
servable variables.

I feel like an ingrate. Dr. Makower has added an extremely useful and venturesome book to the literature. The economist who wants an introduction to activity analysis should be encouraged to start with it; the specialist in this field must take account of its insights and contributions. Thus I am in her debt and I am aware that my comments hardly do justice to my appreciation of her work. But then, only a worthwhile book is worth criticizing.

ROBERT DORFMAN

Harvard University ·

Problems of Capital Formation. Studies in Income and Wealth, Vol. XIX. By the Conference on Research in Income and Wealth. (Princeton: Princeton University Press, for the National Bureau of Economic Research. 1957. Pp. xi, 609. $7.50.)

Various aspects of capital formation—its measurement, its relation to productive capacity, and its controlling factors—are considered in the fifteen papers that make up this volume. There is no doubt about the value of most of them. Original data are presented: the series on residential construction is carried back to 1889; concepts and measurements are subjected to critical examination—for example, those relating to capital consumption; the difficulties inherent in the notion of the capital coefficient are explored, and re-explored; and naive notions of the accelerator are tested and, fortunately, found wanting. The findings, while frequently negative, are authoritative and anyone whose investigations bear upon this subject will have to refer to this volume.

A few contributions are outstanding. Franco Modigliani who served as chairman of the Program Committee and as editor of this volume, has some interesting remarks about the firm's reasons for holding inventories, and their implications for macroeconomic analysis; an excellent critique of Bert Hickman's tests of the accelerator; and in his introduction, a very fair review of the other papers. Robert Eisner's account of "Interview and Other Survey Techniques in the Study of Investment" is most useful especially for those who plan to find out why firms invest simply by asking their managers. Finally, David Blanc and Louis Winnick have made available detailed information about residential construction, in current and constant dollars, on both a gross and net basis for the period 1889-1950.

This by no means completes the list of useful papers. But the tone of most of the rest is so full of caution, and there are so many warnings of pitfalls, that the reader who looks for usable relationships is more likely to be paralyzed than stimulated. Such caution is, of course, natural. The economy is complicated and the simplest of notions is likely, when examined carefully, to turn out to be immensely complex and difficult. But, is it necessary to emphasize it? Roy Campbell's blast at some South African poets seems appropriate:

> They praise the firm restraint with which you write.
> I'm with you there, of course.
> You use the snaffle and the curb all right.
> But, where's the bloody horse? •

The big advances in economics have been made by those who have found it possible to suspend caution, at least selectively. Marshall was as aware as anyone that demand and supply are not strictly independent, but his method, which has certainly yielded results, neglected their interdependence.

No such spirit informs the papers in this volume. Instead, we are told that the value of the capital coefficient in one industry after another depends upon whether expansion is balanced or not, upon the product's grade, changes in the mix, upon scale, upon the nature of technological change, and so on. The significance of any of these variables is undeniable. If, however, we are interested not in the precise effect on one industry's market of any expansion somewhere else, but instead, in estimating the scale of the accelerator effect, it would not be unreasonable to by-pass most of these complications.

With some other suggestions, serious difficulties are encountered. Ruth Mack, to take one example, urges that inventory changes should be classified as "intended," "unintended," and "passive." This suggestion is not, of course, new, except for the inclusion of a third member of the family. But it is difficult to believe that it constitutes anything more than a new name for an old problem, and for that matter, that the whole classification has merit. Unintended investment is to be distinguished from intended by its being subject to reversal. But, unless we suppose that inventory decisions are made only at widely separated dates the very notion of inventories accumulating against the firm's desire is difficult to understand for they could always be checked by a change in selling policy. Inventory investment regarded at one date as "unplanned," at a later one may have to be called "planned." And it is scarcely interesting, and not even accurate, to give it a name only afterwards, depending upon whether the previous movement has been reversed or not. Once the event which permits us to give it a name has occurred, we do not need the name. The problem posed by the notion of "passive" investment is even greater, for there is no way of distinguishing it from intended investment.

Earlier, I had complained of excessive caution, but in this connection, caution about the proliferation of boxes that seem fated to remain empty would be in order. Despite these doubts, there can be no question that Ruth Mack's essay and most of the others make important contributions.

                                                                    LORIE TARSHIS

Stanford University

Some Aspects of the Acceleration Principle. By POUL WINDING. (Amsterdam: North-Holland Publishing Co. 1957. Pp. 254. $8.00.)

As usually defined the acceleration principle states that the value of some dependent variable, say net investment, varies directly with the change over time of some other, independent variable, say net national income. In this form it has the advantage of simplicity, and appears to be extremely useful in the explanation of cyclical movements and the magnification of fluctuations in the level of the investment goods industry compared with general industry.

Unfortunately, it has two major disadvantages. First, it has been pointed

out by Baumol, and in this book at much greater length by Winding, that magnification is not a necessary consequence of acceleration. Second, statistical tests carried out, for example by Kuznets and Tinbergen, offer little support for the principle and tend to show that it is not very helpful in the explanation of actual industrial fluctuations.

It is clear, therefore, that the time is now ripe for a detailed analysis of the principle of acceleration. This Winding has endeavored to carry through. His book is divided into two parts. In the first five chapters there is a discussion of the relationship between fluctuations in sales and fluctuations in new orders placed. The remaining twelve chapters are devoted to the acceleration principle as an explanation of fluctuations in the demand for and production of fixed capital equipment. On both these subjects Winding has interesting things to say.

His method of approach is microeconomic, and pertains to the behavior of the individual retailer or producer. The economic system is not analyzed in terms of macro-concepts, and at the micro-level only a partial equilibrium treatment is attempted. To illustrate the numerous alternative workings of the acceleration principle Winding constructs a large variety of comparatively simple models explaining many of them by arithmetic examples. His object is to show that the extent to which fluctuations in orders or the production of fixed capital equipment are magnified or diminished cannot be determined solely from a qualitative or abstract analysis of the problem, but depends on the actual values of the parameters of the model which is built.

In this he succeeds admirably. He shows, for example, that magnification of orders relative to sales is not a necessary consequence of the acceleration principle, and that, in particular, if entrepreneurs form some idea of a normal level of sales based on past experience, fluctuations in orders may be diminished relative to sales. Moreover, he points out that if the possibility of entrepreneurs reacting to increased sales by varying prices and profit margins is allowed for, once again the fluctuation in orders may be diminished relative to sales. His final conclusion is that the relationship between variations in orders and variations in sales cannot be explained satisfactorily by the acceleration principle.

This is not the case in regard to fixed capital equipment, where he tends to come down rather more heavily in favor of the acceleration principle. He argues that the producer, in order to minimize the cost of meeting fluctuations in production, will keep the average lifetime and degree of utilization of his equipment constant, allowing new installations to fluctuate according to needs. As a result the demand for equipment will tend to fluctuate more widely than production. He then goes on to show that in the long run neither the echo effect nor reinvestment resulting from the varying lifetimes of individual pieces of equipment tend to dampen the fluctuations in new installations. Since his analysis pays no attention to the rate of interest, however, these conclusions must be interpreted with great care. Winding admits that liquidity and the rate of interest will affect the entrepreneur's behavior, but is inclined to treat these factors as of secondary importance.

An interesting point in the second part of the book is that it may be more

relevant for business cycle theory to study fluctuations in total outlays on capital equipment (including maintenance and repairs) instead of fluctuations in installations. Along these lines it is shown that total outlays on capital equipment may fluctuate less widely than installations, and possibly even less than production.

In conclusion, it may be said that, while there may be disagreement on a number of individual points, this book represents a step in the right direction. Perhaps the only major criticism of it stems from its microeconomic approach. Since the acceleration principle is supposed to be relevant to business cycle theory it ought to be formulated in macro terms and analyzed in those terms as well. It by no means follows that what appears to be true for the individual entrepreneur remains so for the economy as a whole. In particular, whereas in a partial-equilibrium model there may be diminution in the fluctuation of orders by a retailer relative to sales, the corresponding aggregated model may involve magnification.

<div align="right">M. H. PESTON</div>

The London School of Economics

A Key to Ricardo. By OSWALD ST. CLAIR. (New York: Kelley & Millman. Frome and London: Butler & Tanner, Ltd. 1957. Pp. xxv, 364. $6.00.)

Doctoral dissertations aside, Oswald St. Clair has published the first volume devoted exclusively to an analysis of Ricardo's economics. Fragmentary interpretations of Ricardo and his legacy to theory have been both numerous and diverse in the writings of J. S. Mill, Marshall, Marx, Bortkiewicz, Hollander, Cannan, Keynes and Knight. However, Ricardo's theoretical formulations have usually served as points of departure or, as Knight contends, "aberations" from "correct theory." St. Clair is unique in developing a "key" for the comprehension of Ricardo. In this sense, his book may be viewed as analogous to the "keys" to Keynes by Dillard, Hansen, and Kurihara.

A retired businessman, St. Clair is not an academician and his purpose is to make Ricardo "clear to the average reader." This is carried out by interpreting and rephrasing some of the more difficult conundrums of Ricardo's *Principles of Political Economy and Taxation.* The *Principles* is St. Clair's work bench. Sixteen chapters of his *Key* are given over to a discussion and exegesis of the major theoretical issues. Seven chapters are concerned with value theory and Ricardo's controversies with Malthus and Say over the validity of the assumption that relative exchange values are proportionate to relative quantities of embodied labor. Another seven chapters take up the functional distribution theory (wages, profits and rents) and the effect that the accumulation of capital has on distributive shares. These problems are discussed within the framework of Ricardo's model of an economy with a rising supply price of land on which to grow corn. One chapter, the longest in the volume, discusses the controversy with Malthus regarding redundant capital, and another grapples with the differences between Ricardo's monetary assumptions in the *Principles* and his monetary analysis of the bullion controversy.

In a foreword, W. H. Hutt writes that the author started twenty years ago "to write an elementary introduction to the theory of value." Somewhere along

the way there was a digression to Ricardo (or perhaps it all began there?) and the deviation culminated in the volume under review. This biographical item is significant because it explains somewhat why so much space is devoted to Ricardo's value theory and, more important, why this is interpreted as it is. St. Clair looks upon value theory as providing the theoretical framework for the explanation of individual prices and, with this in mind, says "Ricardo made a great mistake in assuming that demand only exercises an occasional and temporary influence on price" (p. 350). But was this such a "great mistake"? Joan Robinson, in *The Accumulation of Capital* (Ricardo's major concern), discusses the question of how to deal with normal long-run prices in an economy undergoing growth (accumulation). She concludes, as Ricardo did, that despite the complications introduced by varying quantities of fixed and circulating capital, "the labour theory of value . . . remains valid as a rough generalization, for differences in output per head are much greater, as between one line of production and another, than the differences due to these various qualifications."

If a "key" to Ricardo is desired, it is essential to understand that his starting point was profits. His was an investigation of the effects upon profits when accumulation takes place in a closed economy that faces the dilemma of a rising supply price of the major wage good. Since with an increasing population, a decrease in the marginal facility of producing wage goods requires a higher and higher proportion of the labor force to produce for its own consumption, the net surplus (profit) diminishes over time. The discussion of this aspect of Ricardo's theory is quite adequate, but the weakness of St. Clair's interpretation stems from his failure to relate the theory of value to the aggregate income distribution and growth model, as well as from his inability to appreciate the necessity for simplifying assumptions. The author's nearsightedness in this respect is revealed in such statements as "Ricardo made a capital error when he decided that differences in the quality of labor could be neglected" (p. 51). He is disturbed with Ricardo's methodology since there is never, in reality, a *single* rate of wages or profits. The same difficulty is apparent from St. Clair's failure to comprehend Ricardo's need to assume that the quantities of embodied labor were the major determinant of relative exchange values.

He does not show an understanding of the problems of a theorist *qua* theorist; he does not comprehend that Ricardo's simplifying assumptions were necessary to prevent his major principle from being drastically affected by nonstrategic variables in the system.

Ricardo's *Principles* grew out of a revision of his *An Essay on . . . the Profits of Stock*, a rewriting required by criticism directed at the *Essay*. In the latter, he had begun by looking at the average rate of profit as the ratio of "surplus produce" to the amount of invested capital; he argued that the major determinant of a change in the average rate of profit would be a change in the labor cost of wage goods (of which corn was the index). The cost to society of these wage goods, accordingly, was the labor necessary to produce them, and any residual was profit. Thus a rise in the cost of wage goods would bring a fall in the rate of profit. Now Malthus, among others, argued that

wages could rise without any effect upon profits, since high wages would bring higher prices. If this argument was right, Ricardo said, "it appeared that profits could never really fall, whatever accumulation of capital there might be." As it was Ricardo's contention that a rise in the costs of wage goods would mean lower profits, he had to include in his aggregate model a method of proving that *real* prices could not change when wages rose. Relative prices were proportionate to relative quantities of embodied labor and not dependent upon relative wages, so that when wages changed there were no significant changes in the quantities of embodied labor. The chapter, "On Value," was included in the *Principles* to indicate the degree to which a change in wages would affect relative prices, and while Ricardo was never completely satisfied with the results, still he must be evaluated in these terms and not, as St. Clair has done, in terms of a theory of value dealing with the price considerations of utility theory. While St. Clair has made use of the ten-volume Sraffa edition of Ricardo's *Works*, he has not taken full advantage of the new material published therein, and of Sraffa's enlightening editorial commentary. His *Key* does not succeed in communicating an understanding of what the controversy between Ricardo and his critics was all about.

JOHN P. HENDERSON

University of Illinois

Ocherki sovremennoy burzhuaznoy politicheskoy ekonomii SShA. (Outline of the Contemporary Bourgeois Political Economy of the U.S.A.) By I. G. BLYUMIN. Academy of Sciences of the USSR, Institute of Economics. (Moscow: Gospolitizdat. 1956. Pp. 279. Rbl. 6.75.)

This is the first comprehensive critique of American economic theories that has appeared in the USSR during the last quarter of a century. It consists of three long chapters, covering the history of economic thought in the United States and modern theories of monopoly, business cycles, and antidepression programs. A short epilogue provides a sketch of American communist economic writings. The author, Izrail Grigorievich Blyumin, has since the late 'thirties been the foremost, if not the only, Soviet authority on British and American modern economic thought. He edited the Russian translation of J. M. Keynes' *The General Theory of Employment, Interest and Money* (Moscow: GIIL. 1948. Pp. 398), and wrote *The Critique of the Contemporary Bourgeois Political Economy of England* (Moscow: GIPL. 1953; in Russian), which included among other things a critique of Keynes.

Blyumin's review of the history of American economic thought briefly covers all leading economists from Alexander Hamilton through Thorstein Veblen, with the most space given to the "most famous and influential American bourgeois economist," John Bates Clark. Blyumin's generalization from this history is that, from the very beginning, the advocacy of the utilitarian approach towards the state and the idea of using the government in the interests of capitalism was the most immanent feature of American economic thought.

Blyumin finds that today there are no distinct schools of thought among modern American theoreticians, although many individual differences do

exist. The following are the common characteristic features of modern American economists: they deny the historical character of economic phenomena and of the economic system of capitalism; they ignore the social nature of economics and tend to consider economic processes as purely technical or even mechanical functions; they deny the objective character of economic laws and advocate voluntarism and subjectivism; they unduly exaggerate the significance of psychological factors in economic life; they concentrate on the study of consumption and exchange, and underestimate the importance of production.

In the field of applied economics Blyumin finds that there are today only two broad groups of economists: the advocates of a regulated economy (he lists A. H. Hansen, L. R. Kline, S. E. Harris, A. Lerner), and those who still defend *laissez faire* (F. H. Knight, G. Terborgh, L. von Mises). The former group is said to be a majority today; the latter group Blyumin calls "the last of the Mohicans."

Blyumin's critique of modern American studies of monopoly is more political than economic. Having defined monopoly as just a big business with the predominant share of the market, he thinks that the task of economic theory is to fight monopoly because it is unjust and reactionary. He finds that there are two "tactics" among the American economists today: one tries "to spread the illusion that there is no domination of monopolistic capital in the U.S." (J. M. Clark and G. J. Stigler); the other "attempts to justify monopolies" (F. Machlup, E. Chamberlin, E. G. Nourse, J. A. Schumpeter). The result is that "no American bourgeois economist wants to fight the monopolies." It seems that Blyumin's critique would have profited had he taken into account the progressive (in Marx's opinion, too) aspects of big business, such as the economies of scale, concentration of capital, space for new technologies, "socialization of labor" (using Marx's words), etc.

In his critique of American business-cycle theories Blyumin maintains that American economists have failed to develop a comprehensive and consistent theory of a single and universal cause of depressions. The theories of the multiplicity of causes (J. M. Clark, W. C. Mitchell, A. F. Burns, H. G. Moulton, A. G. Hart) he criticizes as pragmatic and eclectic. He scoffs at the formalistic and mechanistic statistical studies of business cycles. Closely interwoven with this critique is Blyumin's critique of the programs and policies to combat depressions. Since they are based on the theory of multiple causes, they propose multiple ways out. He rejects one by one the main premises of the antidepression programs as insufficient to prevent depressions. He fails to examine, however, what happens if the numerous antidepression levers are used not separately but together: their total impact upon the business cycle is obviously stronger than of each of them individually.

While criticizing the inflationary fiscal policies aimed at the regulation of the business cycle (A. H. Hansen, W. S. Vickrey, *et al.*), Blyumin thinks mostly in terms of the physical composition of national product, but within the static model only. As soon as he proceeds to consider the growth and the role of government expenditures, he begins to have doubts. He concludes that if one continues to think only in terms of the physical composition of national

product, "even a temporary increase in business activity due to militarization becomes incomprehensible." So he attempts to take a step away from the old premises and declares that at least "at the beginning," "at the starting stage" of the cycle defense expenditures produce a "possibility of some growth in total demand," and hence business activity is generated. However, in final analysis, "this means only the postponement of the crisis," because the "militarization of the economy produces only a deforming impact upon the capitalist business cycle, but it never can abolish its basic laws." This is as far as Blyumin has gone in his "revisionism."

It can be noted here that, in fact, it was not necessary for Blyumin to scrap so completely the postulate of the physical composition of national product in order to explain the role of government expenditures in avoiding depression. It was only necessary to start from the dynamic model of national product (the model of enlarged reproduction, speaking in Soviet terms). If overproduction is the main cause of depression, and this is what Blyumin believes, too, and the role of the depression is to periodically eliminate the surplus factors of production accumulated on the eve of the depression, then it is logical to say that permanent defense production and many other unproductive government expenditures fulfill the role of the depression by liquidating through the unproductive use the excess factors of production. The overproduction of commodities and the depression are thus avoided. This explanation, though a Marxist one, may be unacceptable to Blyumin, however, because, firstly, it was suggested as early as 1918 by the late "enemy of the people" Nikolay Bukharin, and, secondly, it casts a shadow of doubt over the unwarranted predictions of the inevitability of future depressions.

Blyumin's book is very often unnecessarily polemical. Polemics is an acceptable way of writing, but it is not always convincing.

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Economic History; Economic Development; National Economies

Architects and Craftsmen in History: Festschrift für Abbott Payson Usher.
    Edited by JOSEPH T. LAMBIE. (Tübingen: J. C. B. Mohr [Paul Siebeck].
    1956. Pp. xiii, 172.)

The scholar to whom these essays are dedicated would be the first to demur at a description of himself as the greatest of American economic historians, not only because of his own inherent modesty, but because he would say—has said—that such superlatives are irrelevant if not meaningless. Largely because of his personal reticence, he has to date missed much of the recognition that his work and personality deserve; but his influence and reputation will continue to live and grow both through his own published works and through the generation of students whom he trained. This slender, unusual volume of homage by a distinguished group of scholars with international reputations goes far to rectify the injustices of indifferent fortune; it, too, will live—a fate infrequently reserved for *Festschriften*.

Rarely have discussions of scope and method been made more palatable

than in this series of bio-bibliographical essays on a dozen of the world's great economic and social historians (including three or four who passed as economists) by nine of the discipline's most able current practitioners. If some matters remain obscure—well, that is to be expected in discussions of methodology.

F. C. Lane, writing on "Some Heirs of Gustav von Schmoller" in the opening essay, and W. N. Parker in his refined and restrained closing essay on Usher himself, provide the most penetrating philosophical discussions of scope and method. Lane successfully shows that while the historical school of economists gradually died out (or became reintegrated with the main stream of economic thought), its basic premise that economic life takes place "within societies differing from each other and given internal coherence . . . expressed primarily in [distinctive] political and economic institutions" (p. 38) has been generally accepted by economists, as well as other social scientists. Parker's remarkably brief and lucid essay permits one to see how, by devoting himself to a search for "limited generalizations" within limited and well-defined but highly significant areas of human endeavor, Usher succeeded in contributing more to an ultimate synthesis of social science than any number of system-builders and world-viewers.

The intervening essays are more directly biographical, and of considerable human interest as such, since in every case the authors were either students or close friends and collaborators of their subjects. Moreover, by telling how each man approached his own work, they too shed some light on the broader issues; in one or two instances, more than a little. Edgar Salin elaborates on "Sombart and the German Approach." Bradford Wells describes how M. I. Rostovtzeff made the study of the economic life of ancient times a subject of great importance for classicists, but could not bring himself to formulate or accept broad generalizations therefrom. The late Lucien Febvre presents a highly personal eulogy of Marc Bloch. Henri Pirenne's celebrated contributions to the study of medieval history are appreciatively reviewed and defended by Charles Verlinden. M. M. Knight's perceptive essay on Henri Sée sheds further light on questions of scientific and historical method, subjects to which Sée addressed himself formally on more than one occasion. By contrast, Arthur Montgomery's purely biographical sketch of Eli Heckscher permits one to see how the latter's environment, personality, and public service were reflected both in his choice of subjects and procedures. W. H. B. Court, in one of the finest essays in a fine collection, shows us how J. H. Clapham came by his "empiricism," a subject doubly interesting in view of the fact that Usher himself has written on it at some length.

In sum, the book does not pretend to be a history of economic historiography, but it does retrace a path among most of the methodological stumbling blocks which have littered the way in the development of the discipline. It will be of great value, therefore, to all those interested in economic history, the novices and the amateurs as well as the professionals. It is particularly gratifying to report the important contribution of one of Usher's own last Harvard students. This, together with the time, patience, and erudition of the distinguished authors, and the international auspices under which the

volume appears (in the series of the Friedrich List Gesellschaft), constitute one of the highest tributes that could be paid to Usher's scholarly career, and render further comment on its significance unnecessary.

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The Political Economy of Growth. By PAUL A. BARAN. (New York: Monthly Review Press. 1957. Pp. x, 308. $5.00.)

This book makes a bold attempt to analyze the state of the world on orthodox Marxian lines. It exhibits to a remarkable degree both the merits and the shortcomings of the Marxian technique of "socio-economic" analysis. Its racy style, its sustained and often brilliant invective, its indignant temper, its tendency to make far-reaching generalizations on insufficient and selected evidence, its capacity to pose important questions, is each so reminiscent of the great founder of Marxism that it might well be hailed as a present day version of *Das Kapital*. Where the book falls short of true Marxian standards is in failing to come to grips with the central issue of the Marxian theory of history—which Marx, if he were alive today, would certainly have tackled —namely, why it is that the proletarian revolution failed to materialize in countries in the advanced stages of capitalism (which, on the Marxian theory, were *ripe* for socialism) and occurred instead in countries like Russia and China where bourgeois capitalism never properly established itself.

The book falls into two main parts, with a loose connection between them. The first part (comprising Chapters 2-4) is concerned with advanced capitalist countries, the second part (Chapters 5-8) with backward countries. The major thesis of the first part is that in the advanced countries of "monopoly capitalism" the problem of overproduction, stagnation, crises, *et al.*, is bound to become steadily greater, and technological progress steadily less. The main thesis of the second part is that it is the very existence of countries of advanced capitalism which prevents the economic development of the backward countries, since any such development is "profoundly inimical to [their] dominant interests." Hence the overthrow of capitalism in the advanced countries is a prerequisite of the removal of the perpetual squalor and misery in which the underprivileged two-thirds of humanity now live.

Both of these are disputable propositions, but it is in dealing with the advanced countries of "monopoly capitalism" that Baran's analysis is at its weakest. He is in agreement with the view that it was the growth of capitalist enterprise which made the rich countries rich, and it was the failure of capitalism to spread to the backward areas which kept them poor. Rising living standards require relatively high rates of economic growth over sustained periods (at moderate rates of growth the consequential increase in population swallows up the benefit); such high rates of growth require a technical dynamism which is associated with capitalist enterprise, and not with precapitalist enterprise. Thus "Marx and Engels accepted in essence the insistence of the classical economists on capitalism's giant contribution to economic development. Yet, not wedded to the now dominant capitalist

class . . . they were able to perceive the limits and barriers to progress inherent in the capitalist system . . . they were able to comprehend the era of capitalism as merely creating the prerequisites for a development of humanity that would lead far beyond the confines of the capitalist order" (p. 5). What Marx and Engels certainly did not perceive or comprehend was that in the course of the development of capitalism (and its transformation from "competitive capitalism" to "monopoly capitalism") not only would the productive powers of man rise to many times their previous level, but the standard of living of the working classes would rise *pari passu* with the rise in production. They certainly did not comprehend that the development of capitalism is consistent with a tenfold rise in real wages per man hour in the course of a single century (to take the admittedly "extreme" case of the United States). On the contrary, the transformation of capitalism into "monopoly capitalism" was to be attended by a growing polarization of society between a small class of exploiters and the vast mass of the exploited, and the growing "immiseration of the proletariat."

Now throughout Baran's laborious attempt (occupying almost 100 pages) to show that the growth of "monopoly capitalism" proceeded exactly as Marx and Engels predicted (plus the further features added by Lenin), one central feature of that process, the "immiseration of the proletariat," is conspicuous by its absence. Yet the "immiseration of the proletariat" is not some inconsequential aberration of Marxian thought which the neo-Marxist can cast aside with a shrug of the shoulder. It is the very essence of the story—both in demonstrating why the progress of capitalism necessarily involves bigger and bigger crises of overproduction and why it must lead to an increasingly revolutionary atmosphere that brings about its ultimate collapse. Baran admits, not that wages ever rise above the "subsistence minimum" in a capitalist country, but that "as a matter of historical fact [the subsistence minimum] is anything but such a firm floor. It is rather a continually moving escalator, and there can be no doubt that what was considered to be the 'subsistence minimum' at any given time—at least in the advanced capitalist countries—has been a *rising* quantity of goods and services" (p. 55; italics in the original). But what determines the movement of this "escalator"? Since "there is considerable evidence that the share of wages in the national income has displayed a remarkable stability during the entire period for which information is available" (p. 56) it is presumably governed by the rise in the productivity of labor!

This admission that the share of wages in income remained constant "in the course of the last five or seven decades—the period commonly associated with monopoly capitalism"—involves the author in a host of contradictions, and is far more damaging to his whole case than he realizes. In the first place, since the growth of monopolies necessarily involves "vaster and vaster profits" —not just absolutely of course, but in proportion to turnover—how can it fail to depress the share of wages in total income? As Baran well knows, taking enterprises in the aggregate, the margin of profit on turnover and the share of profits in income come to the same thing. But if the share of profits did *not* rise, what remains of the contention that the effect of monopolies is to raise

prices (in relation to costs) and to restrict output? The whole problem of distribution is central to his thesis (just as it was central to Marx's theory) but it is left entirely in the air, unexplained.[1]

In the second place, the whole thesis of the Unmanageable Surplus on which Baran rests his case of the "incompatibility of sustained growth with the capitalist system" falls to the ground once it is realized that the "surplus" of production over consumption increases only absolutely, and not proportionately with the growth of production. Marx's (and later Lenin's) view that with the growth of productive powers there is an increasing threat to the capitalist system due to growing difficulty of "realizing surplus value" was predicated upon the assumption that the "intensification of the productive powers of labor" implies a continuous increase in "surplus value" which meant of course an increase in the *share* of profits in output, and not just an *absolute* increase in profits. Baran puts himself in an extremely weak position in upholding the Marxian thesis whilst abandoning the main assumption on which it is based. For unless the "surplus" does increase relatively and not just absolutely the "growing inadequacy of investment in relation to the volume of economic surplus under conditions of full employment" (which is Baran's main contention) cannot be established. As he himself emphasizes in another context (p. 69) there is no basis for the view that the nature of technological innovations and discoveries requires a lesser investment of capital (in relation to production) than, say, a century earlier, so that the proportion of income that requires to be invested to maintain any given rate of growth of production is no smaller today than it was a hundred or fifty years ago.[2]

At one place (pp. 60-61) he appears to seek refuge from this dilemma by arguing that it is not the *size* of the surplus but its uneven *distribution* (between large monopolistic firms and small competitive firms) which is the cause of the trouble. But this again does not answer his problem, unless it could be shown that the investments of large monopolistic firms, in proportion to their turnover are smaller than those of the competitive firms. Since however these monopolistic firms are assumed to be growing faster than the economy as a whole (as they are constantly encroaching on the field remaining to competitive business) and as their capital requirements per unit of production are likely to be as great if not greater (since they are supposed

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[1] In a footnote referring to Marx (p. 58) the stability in the share of wages in the face of growing productivity is attributed to "the efforts of trade unions to offset the pressures of monopoly and to maintain wages in some relation to prices and profits." Yet on the previous page it is denied that trade unions have any power to raise the *share* of wages through increases in money wages so long as producers are free to adjust prices so as to protect their profit position.

[2] Nor is there any statistical basis for the view that the proportion of profits (net of taxes) devoted to capitalist consumption shows a declining tendency. He might have been on firmer ground if he had attempted to deduce a chronic tendency to oversaving from the rise in savings out of wage and salary earners' savings due to the "steady rise in conventional standards of subsistence" (which is Baran's euphemism for a rise in real wages) since this would imply (given the share of profits in income, and given the propensity to consume out of profits) a rising propensity to save for the community as a whole.

to maintain a large volume of excess capacity) no such conclusion can be derived from the premises.

There is finally the argument that the growth of monopolies causes a slowing-down of the rate of economic growth, not on account of excessive prices and profit margins, but on account of the reduction in the speed of technological change and adaptation. "The competitive firm will be *compelled* by competition to introduce the new machine regardless of the concomitant capital losses or be driven out of business by its old or newly arrived competitors now able to produce and to sell more cheaply, while the monopolist is exposed to no such pressure" (p. 79). The danger that a capitalist economy loses its "technical dynamism" as a result of the growth of monopolies certainly exists. But if this occurs it must be reflected in a diminution in the rate of growth of the productivity of labor per man hour. Baran makes no claim anywhere that the rate of growth of labor productivity has slowed down in the "advanced" capitalist economies over the last fifty years.

The plain fact is, that despite the disastrous periodic interruptions to the process of economic growth (such as occurred in the 1930's) the capitalist countries did grow, for the last 200 years or so, at decennial rates that were 30 to 40 times as high as those which prevailed through many previous centuries. Indeed, if this had not been the case, the twentyfold difference in income per head between the advanced and the backward regions could not have arisen. And there is no evidence to show that this process is coming to an end. Despite the growing concentration of production in the hands of giant corporations, neither the rate of technological advance, nor the growth of the purchasing power of the masses has shown any marked slowing-down as compared with earlier periods. While the growth of the giant corporations involved a highly undesirable concentration of power (both political and economic) in the hands of a few individuals, it did not, contrary to all Marx's predictions and Baran's assertions, involve an increase in economic inequality. The difference in living standards between the American millionaire and worker is considerably less, not greater, than it was a century ago; it is enormously less than the difference in standards between the Arabian sheik and the fellah.

By comparison the analysis in the second part of the book on the roots and morphology of backwardness is far more interesting, and it is here that Baran poses really important questions. The main question is, why if the growth of capitalism was a spontaneous process in the present-day advanced countries, did it not extend to the others? Baran's answer is that the growth of industrial capitalism in these areas has been prevented by Western political and economic domination. Just as the industrial revolution in Britain was greatly helped by the plunder of foreign conquest, so the very same plunder, by robbing the conquered areas of their surplus, prevented the emergence of capitalist enterprise. Added to this, the commercial policy imposed on colonial territories was invariably such as to preserve whatever market for manufactured goods existed for the industries of the imperialist powers. The one Asian country which managed to escape Western political domination in the 19th century—Japan—did, in fact, attain capitalist ·development as rapid

as that of any "Western" country, after the shackles of feudalism were shaken off with the Meiji Restoration.

And despite the retreat of the crude 19th century type of colonialism, Western imperialism, in Baran's view, remains the main obstacle to the development of the underdeveloped regions today. For it is solely the aid and support of Western capital and Western governments which keep in existence "a political and social coalition of wealthy compradors, powerful monopolists and large landowners dedicated to the defense of the existing feudal-mercantile order. Ruling the realm by no matter what political means—as a monarchy, as a military-fascist dictatorship, or as a republic of the Kuomintang variety —this coalition has nothing to hope for from the rise of industrial capitalism which would dislodge it from its positions of privilege and power. Blocking all economic and social progress in its country, this regime has no real political basis in city or village, lives in continual fear of the starving and restive popular masses, and relies for its stability on Praetorian guards of relatively well kept mercenaries" (p. 195). The sheiks of Arabia, the military dictators of Venezuela and other Latin American countries, of certain Asian and some European countries, ultimately all owe their existence to the support of American and British capital and political influence. In the absence of Western invasion and penetration, the other backward areas with greater or less speed, would all have gone the way of Japan. "Depending . . . on the internal strength of their pre-capitalist social orders and on the intensity of the antifeudal pressures, bourgeois revolutions and the development of capitalism were more or less effectively resisted and retarded. Nowhere would they have been indefinitely prevented" (p. 162).

This is a suggestive picture which obviously contains an element of truth. But it is doubtful whether it can provide a universal explanation. Not only Japan but Siam too remained outside the sphere of Western domination and influence in the 19th century (largely owing to the rivalry of Britain and France who held each other in check), yet nothing resembling the Japanese development occurred in Siam. The Latin American countries threw off the yoke of Spanish and Portuguese domination not long after the end of British colonial rule in America, yet their subsequent economic history took a completely different turn. Australia, New Zealand and Canada, on the other hand, became high-income countries like the United States though they remained under British colonial rule through the greater part of the 19th century. Lastly, Western penetration and influence could hardly be considered as a major factor in preventing the economic development of Central and Southeastern Europe.

Baran is undoubtedly right in stressing that "a correct answer to the question" of "why it is that in the backward countries there has been no advance along the lines of capitalist development that are familiar from the history of other capitalist countries" is "of the foremost importance" (p. 136). But his own answer concentrates too much on one particular aspect of the question to the exclusion of others. He pays too little attention (in my view) to the importance of a progressive agriculture which alone can provide the growing food surpluses necessary for industrialization; and he under-

estimates the extent to which the reactionary political regimes which hinder social and economic development draw their strength from a native feudal landowning class, rather than from foreign economic interests. Though in some countries Western economic interests are undoubtedly instrumental in maintaining "comprador regimes," it is, to say the least, a gross exaggeration to suggest that the economic development of the underdeveloped countries is "profoundly inimical" to the dominant interests of Western capitalism. The development of Germany, Japan, Switzerland and other relative latecomers of advanced capitalism has raised, and not lowered, the economic welfare of the older industrialized countries, such as Britain and America—both through the consequential increase in the demand for their products and through the enlargement in the scope of international specialization and trade. In the same way the real income of the United States and Western Europe would be considerably enhanced, and not jeopardized, through the economic development of the peoples of Asia, Africa and Latin America.

But aside from the purely economic aspect, the development of the under-developed regions is in the most profound interest of the countries of the West for the sake of the survival of those political and social institutions in which both America and the countries of Western Europe believe. For in the long run rapid industrialization under capitalism is the only effective antidote to communism. The "Praetorian guards of well-kept mercenaries of the comprador regimes" will not, in themselves, prove capable of withstanding revolutionary pressures indefinitely if they continue to serve a feudal-mercantile order which blocks all economic and social progress—not after the experience of Russia and China had demonstrated that the communist combination of economic planning and political dictatorship is capable of securing rapid economic development. The only alternative is to seek to transform the political and social institutions of these countries in directions that foster the development of their potential productive powers, and to provide large-scale economic assistance. It is undoubtedly true that the exigencies of the cold war have recently tended to make the U.S. government dangerously conservative in its attitude towards political and social reform in the backward countries, and the appropriations under the Point Four program, have been very meagre in relation to the size of the problem. This no doubt is shortsighted; but it is a recent (and let us hope, purely temporary) development, and not an inherent and ineluctable consequence of the capitalist system. Though some capitalist countries have been, or are, imperialist, the United States in particular can boast of a long tradition of anticolonialism; and the record of far-reaching social and political reform of the U.S. administration in Japan (under the leadership of that notorious radical General MacArthur) is in itself a sufficient refutation of Baran's thesis that the countries of Western capitalism are invariably the supporters of feudal and reactionary forces.

It is unfortunate that Baran's brilliant intellectual powers could not be employed more constructively—in persuading his countrymen what they ought to do about the underdeveloped countries, instead of telling them that whatever they do is bound to be wrong. By insisting that the economic and social

system in which they firmly believe is not only thoroughly rotten but absolutely hopeless he makes it impossible for many of his readers to take him seriously when he exposes the real shortcomings of American policy.

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The Economics of Under-Developed Countries. By P. T. BAUER and B. S. YAMEY. A volume in the Cambridge Economics Handbooks Series. (London: James Nisbet. Cambridge: Cambridge University Press. 1957. Pp. xiii, 271. 10s 6d.)

This compact book is a useful addition to the literature of economic development. It criticizes in a summary way the major propositions concerning economic growth now in the literature, and every student of growth will have to take account of a number of the arguments presented in it.

The book opens with a discussion of the problems of measuring national income, capital, the labor force, and the distribution of employment. The remainder of Part I, "Descriptive and Analytical," presents various facts about underdeveloped economies and considers various propositions about economic development. These are sorted out under the traditional heads, Natural Resources, Human Resources, and Capital. Part II, "Government and Economic Development," which constitutes somewhat more than 40 per cent of the volume, slices the material somewhat differently. After a statement of the authors' viewpoint and a general appraisal of the role and functions of government come successive chapters that deal with government policies aimed at accelerated capital formation, specific policies affecting agriculture, and specific policies affecting manufacturing. In a symbolic sense, it is appropriate that this volume marks the entry of Milton Friedman into joint editorship of the Handbooks series, for in their general viewpoint concerning the role of government the authors are not only within the same extended family as Friedman; they are his blood brothers.

In the course of Part I, the authors here and there present delightful analyses of a number of standard arguments. They repeat the criticism they have expressed before of Colin Clark's thesis that tertiary industry grows relatively as economic development proceeds. They summarize nicely the rationale of the extended family in a subsistence economy, and argue persuasively that in their economic choices the laborers of an underdeveloped country are as rational and as shrewd as "moderns." Their denunciation of the view that capital is "the" key to development is succinct and cogent, as is their statement of the reasons to doubt the importance of the international demonstration effect.

The last chapter of Part II also deals with a number of theses which have appeared in the literature. In opposing government action as entrepreneur, the authors attack the arguments that a minimum lump of capital is needed to permit development to begin, that industrialization and economic development are equivalent, that the terms of trade run against underdeveloped countries, that balanced growth managed by the government is necessary

for development, and that economic diversification is per se a virtue. While I agree in general with the Bauer and Yamey position concerning most of these theses, yet much of the argument in this chapter seems to me to be much less sturdy than that in Part I.

The chief virtues of the book, it seems to me, lie in Part I. The coverage (as supplemented by Part II) is broad. While many economists will disagree with some of the hypotheses presented, nevertheless the authors present reasoning and evidence not previously stated as neatly in print, of which account must be taken. The authors evidence a refreshing knowledge of the facts of economic activity in underdeveloped economies. In general the reader will come away from the book with a heightened respect for the economic acumen of the peoples of those economies. Finally, the authors do not attempt to explain all economic change or lack of it in underdeveloped countries on economic grounds; they show a pleasing awareness of the role of noneconomic influences. If their comments about these factors are sometimes, in my view, not sufficiently penetrating, yet in general they are a marked advance over those of a number of other economists who have theorized about development.

Part II, on the other hand, after a clear statement of the "individualistic" viewpoint of the authors, is in my judgment so ex parte in advancing that viewpoint as to lose much of its value. Repeatedly a demonstration that a policy does not *necessarily* produce an intended good result becomes sufficient reason for opposing it. For example, an 18-page chapter presents the following arguments against compulsory saving by government to finance development: (1) economic growth is not necessarily desirable; (2) increased capital may not increase the rate of economic growth; (3) compulsory saving may not increase capital formation, for private enterprise may do a better job; and (4) compulsory saving has undesirable effects. The empirical evidence which strongly suggests that compulsory saving has increased the rate of growth is not discussed, nor is the question whether compulsory saving which does increase the growth rate is good or bad. One-dollar-one-vote, without regard to the distribution of income, is assumed to be a desirable mechanism in determining the course of economic development, but beyond an appeal to dispersion of decision-making power the theoretical basis of this position is not discussed. More's the pity, for a strongly reasoned presentation of the viewpoint the authors hold would be a useful contribution to the literature of economic development.

Moreover, unfortunate loose statements appear here and there throughout the book. For example: the volume of foreign direct investment (including presumably that in oil and mineral extraction) "is a reliable index of independent opinion on the economic prospects of the local economy" (p. 143); while price-fixing redistributes income, "it does not reallocate purchasing power" (p. 169).

But let not the reader of this review assume that because of such flaws he can afford not to read the book.

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Economic Development: Theory, History, Policy. By GERALD M. MEIER
    and ROBERT E. BALDWIN. (New York: John Wiley and Sons. London:
    Chapman & Hall. 1957. Pp. xix, 588. $8.50.)

Meier and Baldwin have turned out an extremely interesting and useful
addition to the rapidly growing body of literature on economic development.
As the title suggests, it is a mixture of economic theory and economic history,
and also considers in some detail the noneconomic as well as economic factors
that must be considered in the making of policy decisions. While it does not
contain everything that this reviewer would like to see in a text on the subject,
as far as it goes, it is extremely good.

It is unfortunate that the introduction, "Studying Economic Development,"
is pedestrian in tone and that Table C, which takes up a complete page (p. 11)
and is crammed with all sorts of useful data relating to "per capita income
and related measures" in 53 countries, is not even mentioned much less ex-
panded upon in the chapter. However, fortunately, Part I which follows covers
the theories of economic development offered by major Western economists
in a very creditable manner and more than makes up for the awkward begin-
ning. Smith, Ricardo, Marx, J. B. Clark, Marshall, Young, Cassel, Schumpeter,
Harrod, Domar and Hansen are covered in a mere 125 pages, but the writing
is so precise as to leave this reader with a distinctly favorable impression.

This impression held through Part II, "Historical Outlines of Economic
Development," a "structured history of some of the more significant forces in
international development." While the outlines of the Industrial Revolution
and the growth of a British-dominated international economy in the eighteenth
and nineteenth centuries will hardly come as surprises to economists, the facts
are presented with a perspective which emphasizes the temporal differences in
the rates of development of different nations. The outline of Great Britain's
economic history is especially intended as a check on the theories presented
in Part I, and serves admirably to point out the limits of these theories.

Part III, "Accelerating Development in Poor Countries," deals with the
basic characteristics of poor countries, the obstacles to their development,
their general requirements for development, domestic policy issues (the role
of government, education and health, public utilities, etc.), international
policy issues (tariffs, technical assistance, etc.), and the prospects for develop-
ment of these poorer nations. It is at this point in the general organization of
the text that the authors and the reviewer part company. The authors do an
excellent job of outlining the numerous factors which account for the lack
of development of the less advanced countries. They cite in some detail the
socio-cultural, demographic, natural- resource, and financial aspects of their
economic situations and even go so far as to offer three pages of topics for
case studies which cover the outlines of these topics thoroughly. However,
the authors themselves have not seen fit to include even one case study in
their otherwise excellent text. Given the currently great professional interest
in the plans for economic development of Soviet Russia, Communist China,
India, Pakistan, and other nations, it is surprising to find that not one of
these plans is covered in the book. This is perhaps the text's chief defect:

it is extensive rather than intensive in approach. It is distinctly possible, in the absence of carefully prepared case studies presented in class by the instructor, that the student may become confused as to the way in which the various factors described in Part III come together in real life. For example, in discussing the role of the government, in one paragraph (p. 365) the situations of Ceylon, India, Pakistan, the nonself-governing territories of Africa, and Turkey are briefly mentioned. In another paragraph (p. 393), in a discussion of monetary policy, the situations of Cuba, Ceylon, Egypt, Turkey, the Caribbean countries and Thailand are briefly mentioned. While these are the more extreme cases, they suggest the extensiveness of the approach. This writer would very much prefer to have seen at least one good case study of a "poor" national economy inserted at this point to tie the various topics together and show how they are related.

Part IV, "Maintaining Development in Rich Countries," is redundant in some respects. It considers economic development as a policy goal, it cites the economic characteristics and trends of the wealthier nations, lists their general requirements for maintaining development, and briefly discusses their present prospects. While the order of discussion is different, substantially the same nations and topics are discussed in Part IV as in Part II, and as with Part II, the impression gained is that this is a warmed-up albeit distinctly palatable dish of European economic history. Perhaps this is of no consequence, and it is possible that in some teaching situations, it may even be an advantage.

For those students who may be interested in pursuing the subject of economic development further, a two-page list of periodicals and three appendices listing selected readings in the socio-cultural aspects of development, development programs and plans, and case studies of development, are provided at the end of the text.

As might be expected of a book of approximately 600 pages which covers so broad a subject, there are a number of matters about which a reviewer might quibble with the authors. Suffice to note that the next time this reviewer gives a course in economic development he will unhesitatingly use this text. For all of the criticism which can be leveled at it, the book is comprehensive and well written, and will undoubtedly stimulate interest in the field among those who most need to be stimulated.

SIDNEY KLEIN

Rutgers University

I problemi dello sviluppo economico con particolare riguardo alle aree arretrate. Vol. LIV (New Series) in: Pubblicazioni dell'Universita Cattolica del Sacro Cuore: Contributi dell'Istituto di Scienze Economiche, Serie IV. (Milan: "Vita e Pensiero." 1956. Pp. vi, 430. L. 2300.)

This book contains a collection of papers by nine different authors on various aspects of the problem of economic development. In its preface Professor Francesco Vito, the director of the Institute of Economic Sciences at the Catholic University of Milan under whose able direction the volume was

published, points out that to limit the scope of their analyses contributors have dealt primarily with the problems of "backward" as distinguished from "underdeveloped" and "depressed" areas and have emphasized facts and relationships pertaining to the Italian economy.

In an introductory essay by Vito on the present status of economic development theory and its possible applications to the Italian case, the author states that the ambitious notion that economists may succeed in evolving a theory of economic development valid for all types of economies, whatever their social, institutional or cultural background, must be abandoned. Equally untenable is the assumption of a basically uniform process of development through which the world's economies are supposed to pass. In Vito's view the economic development of the West is a special case not likely to be repeated. Classical doctrine holds that the competitive system tends to equalize income levels ·of various countries. Yet, experience shows that differences stubbornly persist. Why is this so? To facilitate analysis, one must distinguish, Vito says, between a theory of development for backward areas and a general theory of economic development. The first must seek to explain the apparent inability of certain economies to break "the closed circle of poverty"; the latter must deal with variations in the rhythm of economic development in different societies at any one time or in the same society at different times.

After discussing narrowness of local markets, lack of fixed social capital, lack of savings, monopolistic or quasi-monopolistic advantages possessed by foreign enterprises seeking to exploit local natural resources and the hindrances to development of backward areas deriving therefrom, Vito turns to an analysis of the Italian case which is, in the main, that of the still economically backward South. What, he asks, are the "strategic" factors which can best further development there? His reply serves as an introduction to the papers contained in the remainder of the volume which are mainly devoted to detailed analyses of individual factors of importance in the Italian development process.

Nino Andreatta follows Vito's essay with a paper on technological factors in economic development, in which the importance of a constantly high degree of technological efficiency in the productive process is emphasized. There is then a study by Corrado Bonato on the social and economic aspects of mechanization in Italian agriculture in which, after some interesting international and regional comparisons, the effects and problems of agricultural mechanization in Italy in recent years (from 59,000 tractors in 1949 to 119,000 in 1953) are discussed. Ercole Calcaterra analyzes the problem of more intensive agricultural development versus an early effort at industrialization and stresses the importance of agriculture in the early stages of the development process. Four papers deal with the financial aspects of development. Cesare Brasca discusses development and fiscal policy with special attention to policies designed to aid backward areas which are part of larger territorial and political units. Capital formation and credit policy are studied by Franco Feroldi and the functions of the banking system with regard to these are explored. Siro Lombardini discusses advantages and

limitations of quantitative methods in analyses of the productivity of investments in relation to the formulation of development policy. Giancarlo Mazzocchi studies reinvestment of undistributed profits by business enterprises and its effects upon development with special reference to recent Italian experience. His work indicates a gradual growth in importance of this mode of financing, a relative decline in importance of the Italian private capital market and a concurrent growth of investment financing through public entities.

The remainder of the volume deals with topics which are less closely related. Economic development in Marxist thought with special reference to Italian contributions (which are found not to have been very significant) is discussed by Franca Duchini; Vito analyzes the problem of population and economic development and that of economic progress as an objective of economic policy. Italy, he states, cannot by itself alone achieve truly effective development but must be aided in its efforts by foreign capital and by more liberal immigration policies abroad. The volume closes with a paper by Giancarlo Mazzocchi in which the results of the International Congress on Backward Areas recently held in Milan are analyzed in the light of similar work currently done in other countries.

Limitations of space prevent a fuller discussion of the content of individual papers and of the place of this scholarly volume in the rapidly growing literature on economic development. It must suffice to say that, although varying in length and detail, all the papers which it contains give clear evidence of much careful thought on the part of their authors and do, collectively, represent a contribution clearly worthy of the attention of the student of economic development problems.

WILLIAM G. WELK

Washington, D.C.

India: The Awakening Giant. By W. S. WOYTINSKY. (New York: Harper & Brothers. 1957. Pp. xv, 201. $3.75.)

This readable and anecdotal version of the current Indian scene encompasses all aspects of life—political, economic, social and cultural. It reviews the colonial heritage, Indian spiritualism, the resource base, economic planning, education, village uplift, international relations and foreign policy, and world ideologies, as well as the patterns and strategies of economic development. The book has the grand sweep of the traveler who has not spent long enough in any one place for the complexities and confusions to assail him. The author feels he has the key to understanding India and it is this "simple fact that, in an environment and under conditions that are strange to us, she is shaping her future according to the pattern which is common to us and our friends and allies, indeed the common pattern of the free world." In interpreting India to the American public, the author's approach is on the whole very friendly and sympathetic to Indian sensibilities and leadership. Thus he relates India's "neutralism" to our own historic preoccupation with staying out of entangling alliances when we were a new nation. Likewise he tries to minimize the gulf between America's private enterprise system and India's recently proclaimed "socialist pattern" by pointing out that American capital-

ism has moved a long way from the rugged individualism of the nineteenth century and that conversely India's socialist pattern falls far short of all-out socialism. In fact under Mr. Woytinsky's facile pen this gulf dwindles to mere semantics.

At times, however, he lapses into the role of interpreting India and India's needs to the Indians—and here he is not so successful. He takes Indian intellectuals to task for their misreading of the Russian experience and their mistaken notion that India can learn something from Russian economic planning even though India proposes to conduct its planning within the framework of a political democracy. The author's admonitions on this count say in effect that development can only come table d'hôte and never cafeteria-style— either the capitalist model or the socialist one, and never the twain shall meet. Who can tell? Eclecticism is never so neat and tidy as ideological purity, but it may none the less prevail and confound the purists.

The author's point of view becomes clearer when he discusses the measurement of economic growth and the relation of heavy industry to development. Though he does not take exception to the specific capital allocation to the steel industry in India's second five-year plan, he implies that Indian intellectuals have uncritically accepted what he dubs the "Iron Calf" fetish of heavy industry, i.e. concentration on the building of a heavy industry core as the base for future industrialization. His advice that consumer-goods industries should come first and that they should import their machinery needs may fall rather flat in India where for the past hundred years there has been a large textile industry which has imported its equipment from abroad without any appreciable economic development ensuing. He sidesteps the whole question of the role of investment in basic industries as an instrument for increasing consumer purchasing power—and hence the demand for consumer goods, channeling an adequate proportion of the additional output into new investment, and providing a diversified economic structure. There is no serious discussion of the very real dilemma confronting countries desirous of becoming industrialized and raising their living standards—the dilemma of how to weigh present needs against future abundance, i.e., the advantages and the costs of rapid development of heavy industry as compared with proceeding at a more leisurely pace letting the consumer-goods industries take the lead. Instead he gives a lecture on the importance of the doctrine of comparative advantage which decrees the promoting of industries in line with the individual country's resource assets and limitations. These strictures may be pertinent to some countries which aspire to build up a heavy industry though they lack essential raw materials for it, but not to India since, quite aside from the dynamics of development, India is eminently suited to the production of steel for her own use and for export. She has a comparative advantage here due to her abundant raw materials of high quality in close proximity to each other.

It is the author's contention that institutional changes which would increase output without additional capital formation have not been thoroughly explored. He is no doubt right. Certainly agrarian reform, the education of the masses, and overhauling of the rural distributive and credit mechanisms

and of the caste structure have not gone very far. Genuine development re-
quires these institutional changes but not as an easy alternative to capital
formation—these reforms may in fact be even more difficult to achieve than
a stepped-up rate of savings and investment. Action is necessary on both
fronts and in all probability there is an interaction between them.

HELEN B. LAMB

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The Agrarian Prospect in India. By DANIEL THORNER. (Delhi, India: Delhi
University Press. 1956. Pp. 89.)

This is a significant book although a short one. It is important for a
variety of reasons. First, it deals with one of the major factors influencing
the future economic development of India; as well as a major problem in
many of the underdeveloped countries of the world. Second, Daniel Thorner
is easily one of the best informed economists, in India or outside of it, on
the agricultural sector of the Indian economy. He is also a provocative and
persuasive writer, and this book is a most stimulating one. Third, and pos-
sibly most important, it is among the pioneer works in the subject because of
its method, which relies heavily on actual field investigations and visits to
almost 100 villages in many Indian states, rather than on library sources. It
is inevitably a- preliminary work that can only scratch the surface of the
highly complex system of social and economic relationships in India. However,
if it stimulates further original research of a similar nature, and further
ideas, in this complex field it will have made a major contribution to social
science in India, as well as to our knowledge of Indian society.

The main points of this volume, which was initially a series of lectures
at the Delhi School of Economics, may be briefly summarized. Thorner pre-
sents the hypothesis that the existing system of legal, social and economic
relationships on the land acts as a "built-in depressor" to the economic
development of India. This system is characterized by a many-layered hier-
archy of rights to agricultural produce with numerous classes and subclasses
of proprietors (absentee or resident), tenants and landless laborers.[1] (The
exact relationships vary widely from state to state.) The effect of this system
is to deprive the actual "tiller" of the soil of the rewards of his efforts, and
to siphon those rewards to the land-owners who in many cases are not the
actual farmers. Thus the farmer has neither the incentive nor the resources
to invest in the land or improve his methods to raise output, nor the income
to purchase manufactured products from the industrial sector. At the same
time the landlord has everything to gain by maintaining the existing system
of relationships which preserves his high income as well as his social position.
This system of agricultural relationships is supported by, and in turn sup-
ports, other facets of the rural-agricultural society—the caste system, the
money-lending system, the fragmentation of holdings, etc.—and when com-

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[1] It would have been desirable to have presented some statistical support for this analysis.
The lecture character of this volume may not have made this possible. However, Thorner
has written several subsequent articles. See *Indian Economic Review* (August 1956),
and *Economic Weekly* (June 8, 1957).

bined with the continued pressure of the population on land results in stag-
nant agricultural output. Unless drastic change occurs to break this system
of rights on the land, the economic development of India will be thwarted.
The drastic change Thorner feels is necessary is a program to give the land
to the actual "tiller" of the soil.

Using this hypothesis as a touchstone for his analysis, Thorner then ex-
amines the land-reform legislation of the various Indian states, and finds
that in almost all cases the legislation is inadequate to deal with the problem.
It is inadequate either in its conception, which often accepts as a basis the
existing structure of rights, or in its enforcement. Both of these inadequacies
reflect the political and social power of the landlords. The reform Thorner
feels is necessary has not been achieved, except in Kashmir.

The argument is powerful: the hypothesis logical and well supported, and
the analysis of land-reform legislation in the various states persuasive al-
though preliminary (it has been criticized by other students of the subject).
However, the author has not, to this reader, made his economic case for the
reform he advocates. In order to do so he would have to show that the "land-
to-the-tiller" reform would raise agricultural output. In fact, he cites one
state—Andhra—where there has been a major improvement in the condition
of agriculture *without* land-reform legislation, but notwithstanding, shifts in
caste position and political power have had major effects. He is highly critical
of the Bombay land-reform law though rents have been drastically reduced;
and elsewhere he implies that limits on land-holdings of 15-20 acres are too
high. Certainly land reform is necessary—reduction of rents, protection of
the tenant in his rights, supply of adequate credit facilities at reasonable
interest rates to all farmers, reduction of the large *uneconomic* estates—but
whether a "land-to-the-tiller" program which for example, might result in
further fragmentation of land would achieve the desired economic goal is
doubtful. The author *hopes* so, largely on psychological grounds—he has not
proved it.

In conclusion, although Dr. Thorner has not proved the case for the type
of reform he advocates, he has written a powerful and persuasive book on the
need and importance of land reform in India. It is among the pioneering
efforts in method used in this highly important area of study. For both these
reasons it is must reading for anyone interested in the future of India's agri-
culture, and probably for anyone interested in the development problem in a
similar country.

<div align="right">GEORGE ROSEN</div>

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*The Economic Development of Jordan: Report of a Mission Organized by
    the International Bank for Reconstruction and Development.* (Balti-
    more: The Johns Hopkins Press. 1957. Pp. xvi, 488. $7.50.)

This book falls into two parts: a Summary Report setting out the main
background to the economy of Jordan and summarizing the sector programs,
and a Main Report which expounds the sector programs in more detail. The

aims are to increase Jordan's productive resources, reduce her dependence on foreign funds, and increase the standard of living of the population. To achieve these aims, the book offers and explains a ten-year program of public expenditures. History and geography combine to make Jordan's problems at once urgent and difficult.

The main Report deals with land use, mines and minerals, industry, transportation and communications, education and public health, housing and community services, money, banking and foreign trade, public finance, and the financing and execution of the development program. Many tables of statistical data are provided.

Agriculture accounts for nearly half of Jordan's total output in the private sector. The Report, therefore, attaches primary importance to the agricultural sector. Jordan's population of nearly 1½ million is growing at a rate of 3 per cent per annum (nearly one-third of the total population being refugees from the Arab-Israeli conflict); hence, the urgency of raising the output of agriculture. The Mission recommends that steps be taken to increase the productivity of rain-fed agricultural areas, and to increase the water resources of the irrigation-fed areas. For the former objective it recommends mechanized weeding and the use of tractors in the production of field crops. Wheat is the staple food of Jordan; the sketchy treatment given to wheat production in the Report suggests that the Mission felt that wheat could be imported relatively more cheaply than Jordan could produce it. It seems that this accounts for the Mission's emphasis on finding external markets for Jordan's other products like fruits and vegetables. Mechanized tillage and weeding may do much to improve the output of agriculture. However, improved storage and transportation facilities may be no less important if not more immediately urgent. Most countries like Jordan lose a substantial portion of the annual agricultural output through wastage, rot, and bad handling. These aspects of the agricultural problem are dealt with in the Report; however, in your reviewer's opinion, the emphasis placed on the use of tractors will give the reader (and more dangerously the policy-maker) the impression that the most immediate solution of Jordan's agricultural problem is to be found in mechanization.

The Report recommends the revival of the potash and the extension of the phosphate industry. In both activities the help of foreign technical experts would be required. A number of industrial projects are also recommended. The success with which these secondary industries will develop will depend ultimately on how fast the production of the staple foods can be accelerated or else imported cheaply from abroad.

Transportation receives the largest slice of public funds. This is justifiable in view of the impetus it will give to the distribution of products and the general push it will give to other projects. The Mission suggests that no new rail links be undertaken; existing lines are to be improved through reorganization and the adoption of Diesel engines. On the other hand, road development is to be pushed forward, particularly the Amman-Aquaba road. The development of rural roads will do much to bring additional produce to market.

The Report appropriately calls attention to the shortage of technical and administrative skills which may hold up the execution of the development program. Purely technical skill can be imported although it may be expensive. Ultimately, the shortage of higher administrative skill may prove to be a much more serious bottleneck than the shortage of purely engineering skills—a fact not readily appreciated by nationalist policy-makers in underdeveloped areas. Engineering skills can be acquired readily by intensive training; administrative skill requires years of experience, and underdeveloped areas are apparently in a hurry.

This Report maintains the standard of the best ones published so far by the World Bank. None of the reviewer's critical comments is meant to detract from the merits of such a sober and excellent work.

PIUS OKIGBO

Nuffield College, Oxford

Ocherk istorii promyshlennosti SSSR (1917-1940). By E. Y. LOKSHIN. (Moscow: Gosudarstvennoye Izdatel'stvo Politicheskoy Literatury. 1956. Pp. 320. 7 rbl.)

The *Outline of History of the Industry of the USSR, 1917-1940,* by Efraim Yudovich Lokshin, is one of the best studies of its kind that have appeared in the Soviet Union. This book may be accepted by the reader as comparatively objective and scientific.

In his foreword the author states that his intention was to write a history of the development of Soviet industry, and not to provide a detailed economic analysis of its growth. This distinction is sound enough, and, although the book is too sketchy to serve as a complete history, it fulfills its purpose nevertheless.

Lokshin's history covers the period between the Revolution of 1917 and the German attack on the Soviet Union during the second world war. Hence it embraces the most critical period in Soviet industrialization: the first two five-year plans. The author promises to bring his history up to modern times in his second volume.

Besides the usual chronological framework, Lokshin also adopted for his work the following system of standard measures of historical growth: (1) Structure of the national economy and of industry: (a) ratio of the contribution of industry and that of agriculture to the national income or product; (b) ratio of capital to consumer-goods output within the gross industrial output; (c) share of such leading industries as machinery, electrical, chemical, etc., in the gross industrial output. (2) Volume of industrial output: (a) share of national industry in world industrial output for the most important commodities; (b) ratio of national industrial output per capita of population to that of foreign nations. (3) Technical and economic indices of industrial production: (a) appearance of new commodities and industries; (b) indices of the electrification and mechanization of production and the introduction of new technologies; (c) indices of the concentration and specialization of production; (d) indices of labor productivity. (4) Measures of technical and economic independence of foreign countries: (a) structure of exports and

imports; (b) ratio of imports to domestic output for the most important commodities.

Lokshin used this system of measures to evaluate step by step the levels of development of Soviet industry in the course of its history. However, he did not always limit the scope of his inquiry to this framework. His description of various aspects of industrialization is even more complex.

Of course, a critical economist might hesitate to accept all the statistical data used by Lokshin at face value. Since he used only traditional data and since they include such series (discredited by Soviet statisticians themselves) as, *e.g.*, gross industrial output indices in terms of 1926-27 prices, the precision of his calculations is bound to be doubtful. But since, on the one hand, he did not use the data as a basis for economic analysis, and on the other hand, his use of the data was homogeneous and uniform throughout, his method can be relatively acceptable for the limited purposes of economic history. In fact, it is quite probable that few historical facts and events in Soviet economic history would change their meaning and significance if the average annual rate of growth of the gross output of industry were to be revised from, say, 20 to 18 or even 15 per cent.

Lokshin's work however invites criticism with respect to some other aspects. His treatment of the origin of the 199.5 billion rubles (in terms of 1955 prices) of capital invested by the Soviet government in industry during the first two and a half five-year plans is rather superficial and inadequate. In explaining the origin of this capital Lokshin uses the formula coined by Stalin in his *History of the CPSU/b/, Short Course:* that previously the peasants had had to pay rent to the landlords, Russia had had to pay huge interest on foreign debt, etc. These "internal sources" were saved by the Soviets after the revolution for the purpose of industrialization. Such an explanation of the origin of today's capital is not really an explanation at all.

In this connection Lokshin's treatment of the purposes of collectivization of agriculture is also inadequate. Although he recognizes that the old NEP sources and methods of capital formation were sufficient to provide enough capital only until 1930, and that by that time an acute need for more capital arose (p. 187), he misses the point when he concludes that heavy industry itself was going to provide this capital (p. 188). According to his argument the collectivization of agriculture provided industry only with labor (pp. 185-86). But Lokshin says nothing about the introduction of the new system of compulsory deliveries of collective-farm produce to the state at monopsonistically low prices, about the resale of this produce by the state to the consumers at monopolistically high prices, and about the appropriation of the resulting huge monopolistic profit into the state budget by the means of the turnover tax. This system came into being in 1930-1933, *i.e.*, exactly at the time of acute need of capital for the continuation of industrialization, and this is where the bulk of the capital comes from in the USSR even today.

There are other disputable but thought-provoking ideas in Lokshin's book. There are also many interesting new findings published for the first time. He finds, for example, that the rate of growth of the light industries in the USSR started to lag behind that of the heavy industries even during the

period of the NEP. He says that defense production started in the USSR on a mass scale in 1932 and immediately produced a slow-down in the rate of growth of the gross industrial output. His scrupulous description of all the zigzags of the continuous reform of the system of government management of industry also merits praise.

VSEVOLOD HOLUBNYCHY

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Business Fluctuations

Fluctuations, Growth, and Forecasting: The Principles of Dynamic Business Economics. By SHERMAN J. MAISEL. (New York: John Wiley & Sons. London: Chapman & Hall. 1957. Pp. xv, 552. $7.50.)

The appearance of another text in a field already so well supplied is unlikely to arouse undue enthusiasm. I will therefore say immediately that Maisel has written both a different and an extraordinarily good book. He has managed to assemble an astonishing array of analytical machinery, to describe it clearly and concisely, and to show its relevance to decision-making in a changing economy. The publishers have provided a legible type and well-drafted diagrams, and have produced what is, by contemporary standards, an attractive volume.

An adequate summary of the contents of this text could hardly be given in a reasonable amount of space, but the following statements may convey some notion of its coverage. The book consists of 25 chapters, arranged in five parts. Part I (five chapters) deals with the general principle of decision-making, economic growth and fluctuations, and the structure of the American economy. Part II contains only two chapters: one on the choice of data for decisions; the other on tools of macro- and micro-analysis. Part III is called "Studies in Spending Decisions." Here there are nine chapters on consumers, business, and government, with case studies applying the analytical concepts developed. Part IV is entitled "Forecasting," though the first of the four chapters is concerned with cycle theory, the next two with the history of domestic cycles since 1913, and only the last with forecasting as such. Part V, finally, consists of five chapters concerned with economic policy formulation by government, business, and private persons.

The modest size of the volume may be misleading. Maisel's treatment is brief but by no means superficial, and a typical chapter contains enough material to keep most students profitably occupied for some time. A chapter on long-run growth, for instance, covers in some 20 pages such topics as growth factors, secular trend, stagnation, long waves, and intermediate trends, followed by a substantial case study. Instructors who wish to supplement this text in one-semester courses will need to consider carefully how much additional matter their students can handle.

Although Maisel treats decision-making in all sectors of the economy, his emphasis is on the problems of business firms. This fact, together with the author's own academic connection, suggests that his text is specially suited

for use in schools of business, and the suggestion is confirmed by detailed examination of the book itself. I believe, nevertheless, that many instructors in liberal arts institutions would do well to give this text a trial. Students who can never understand what theory is all about should find Maisel's discussions illuminating.

In concluding what is meant to be a very favorable review, I must confess to some bias in favor of anyone who can write page after page of clear exposition in literate English.

RICHARD V. CLEMENCE

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Central Banking after Bagehot. By R. S. SAYERS. (New York and London: Oxford University Press. 1957. Pp. 149. $2.90.)

This volume brings together ten recent lectures and essays on central banking by Professor Sayers, about half of which are made available to the English-reading public for the first time. The discussion ranges from the historical development of English central banking to descriptions of the postwar London and New York money markets, from the theoretical basis of central banking to central banking in underdeveloped countries. Throughout there is evident not only the author's considerable command of his subject, but also his ability to render palatable—even, at times, lively—discussion that in less skillful hands might become tiresome.

While the range of subjects is wide, there is a persistent theme that recurs often. It concerns Sayers' conception of central banking, which is at once both broad and limited: broad in the sense that Sayers opposes fixed rules or conventions for central banking *techniques;* but limited in the sense that the *goals* of monetary policy are determined outside of the purview of the central banking authority. Sayers would oppose, that is to say, both those who prefer "rules" to "authority" in central banking and those who would have the central bank pursue its own goal—*e.g.,* price stability—independently of the fiscal authority.

Consequently, the emphasis throughout is on central banking techniques; or, more specifically, on the need for changing techniques as changes occur over time in both the goals and the institutional setting. This emphasis lends a realistic tone to the author's review of the history of English central banking (which comprises roughly half of the book), and leads him to question whether postwar developments imply a break with traditional central banking principles or simply the search for new means of implementing these principles.

> . . . first there was mercantile credit, then foreign lending, and now perhaps stock exchange speculation or consumer credit. If this is so, it is entirely in keeping with the basic tradition that the central bankers should seek out these newly important sensitive spots and find weapons

by which they can be touched. The central bank should . . . be ready to
use any device it can find to control the behaviour of the financial system
in the interest of the "employment policy" adopted by the government.

The essence of central banking, for Sayers, lies not simply in employing tradi-
tional bank-rate or open-market technique, but in ever seeking the (chang-
ing) sensitive points in the financial structure and devising new means of
imposing its influence in the direction higher social policy dictates.

From this Sayers develops a more fundamental point. The rationale of
central banking is simply the changing nature and composition of the supply
of money—or, better, liquidity. The reason that a set of rules cannot sup-
plant discretionary authority is that it is impossible to stratify financial insti-
tutions in such a way that liquidity is a simple and constant entity. Conse-
quently there can be no rules governing the supply of liquidity that can be
universally applicable. "We have central banks for the very reason that there
are no such rules."

Perhaps the most interesting application of Sayers' conception of central
banking is in his essay on "Central Banking in Underdeveloped Countries."
Considerations of space preclude an adequate review of his contributions to
an understanding of this subject. Suffice it to say that he disputes those who
would deny central banking a role in a country with "underdeveloped" capital
and commodity markets, contending rather that while central banking has
quite a different job to perform and must employ quite different techniques
in such a situation, there is much that it can do to facilitate economic
development.

Of particular interest to economists in this country is the essay entitled
"The New York Money Market Through London Eyes," wherein the author
questions the "bills-only doctrine" of open-market operations.

While one or two of the essays are already in a sense "period pieces" and
others may soon be, there is much in this volume of lasting interest to stu-
dents of central banking.

JOHN H. POWER

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Principles of Money and Banking. By HIRAM L. JOME. (Homewood, Ill.:
    Richard D. Irwin. 1957. Pp. xix, 622. $6.50.)

This text, the third in the field of money offered in the Irwin series in
economics, is doubtless intended to complement the more rigorous treatments
in Shaw and Halm. Professor Jome's book is less specifically oriented to the
economics major, and places much more emphasis on the mechanics and his-
tory of money and banking than on their relationship to the general level of
economic activity.

Section I provides a conventional introduction of the basic functions of
money. Sections II through V, dealing with the supply of money, regulation
of banking, and foreign exchange, are developed with a substantial admixture
of historical material. Part VI covers the area normally termed "monetary
theory," but only in terms of the causes and effects of price changes; and

only within the framework of the quantity theory and, surprisingly, the commodity theory. Part VII, "Controls and Influences on the Value of Money," includes a chapter on the basic problems of individual bank management as well as a discussion of general monetary and fiscal measures. The book concludes with a chapter describing the various types of savings institutions and loan contracts from the standpoint of an individual depositor-investor.

The chapter dealing with an individual's use of his bank is an interesting addition to the usual topics in a money and banking text, but two other innovations may create teaching problems. The deposit-creating activities of commercial banks are analyzed in Chapter 5, while discussion of the managerial decisions underlying loan policy is deferred to Chapter 25, where it is handled as an aspect of "Controls and Influences on the Value of Money." Similarly, the structure of the Federal Reserve System and its control powers are treated in entirely separate sections. While there is nothing illogical about this ordering of materials, it does leave the section on the sources of money in a state of suspension through most of the book.

Both style and content of the book will give it definite appeal to particular segments of the market. Jome's presentation is consciously simplified and at times colloquial to the point of slanginess. In the belief that historical background is essential to an understanding of present monetary and banking principles, he has interspersed the analysis with considerable illustrative detail. Those instructors who prefer this approach will find the historical material very well developed in most respects. Even the normally muddy going of American monetary and banking development becomes lively reading in Jome's hands. The treatment of some of the mechanics of the monetary and banking system—in particular, the clearing process, the handling of currency, and the legal development and present forms of negotiable instruments—is also unusually thorough.

The book will have less appeal for use in courses in which analysis of the relationship of money to the general level of economic activity is stressed. Also, in places the historical treatment takes the form of disconcertingly long digressions or of irrelevant asides. In Part II, for example, the analysis of the types and sources of money is almost lost in the details of illustrations from the past. The student of economic doctrine may also be a bit disconcerted by the discussion on page 6 in which the author undertakes to distinguish the concept of money as a medium of exchange from the Marxian concept of surplus value.

More significant is the general orientation of the book with respect to monetary theory. Despite the general tendency toward separate courses in money and banking and income theory at the undergraduate level, it is startling to find in a text on money only the commodity theory offered as an alternative to the quantity theory—and even stranger to find the latter described as having "the advantage of 'orthodoxy' and 'respectability'" at present (p. 470). The income-expenditure approach is discussed only briefly in a later chapter on fiscal policy.

Underlying this treatment are views of the role of money which are con-

sistent but hardly current. It is doubtful that many economists will share Jome's belief that "the commodity theory has been slighted to a great extent in current literature on the value of money, but seems to have enough points in its favor to merit careful consideration" (p. 470).

It is equally doubtful that a majority would share his evaluation of the gold standard and his nostalgic observation that "the effective operation of the gold standard assumes a rugged strength of national character that many countries probably do not possess" (p. 370). Those who do will find the whole matter of metallic standards and the contrasting monetary systems of the present very adequately presented. Those who do not will find the theoretical basis for discussion of monetary and fiscal policies somewhat unsatisfactory.

ROBERT A. CRUTCHFIELD

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Public Finance; Fiscal Policy

Die Steuern im Vereinigten Königreich unter Berücksichtigung der Entwicklung seit dem Ersten Weltkrieg. By HERBERT WEISE. Kieler Studien, no. 41. (Kiel: Institut für Weltwirtschaft, Universität Kiel. 1957. Pp xii, 360, and set of supplementary tables and charts. DM 40,00.)

Weise's book belongs to a series of studies published by the Institute for International Economics at the University of Kiel. In the same series in 1954 the author published an analysis of the British income tax emphasizing its economic aspects. The present study deals mainly with problems of tax law and does so in great detail. The many tables and charts attached to the book, as well as the many examples incorporated in the text showing how taxes are computed, have substantial value for a student of comparative taxation. Also the present volume is of interest to students of American public finance and of British economic institutions. The author's presentation, as the subtitle indicates, emphasizes developments since the first world war. There is also adequate discussion of the economic aspects of the various British taxes.

After a general discussion of the British tax system and of the relationship between the major taxes, the author devotes the main part of the book (278 pages) to a discussion of the taxes levied by the national government. In turn the income tax, corporation profits tax, the estate duty, the land tax, stamp duties, customs and excises, and finally the purchase tax are considered. There is a much briefer discussion of the rates, that is the taxes levied by counties and county boroughs. The book concludes with a legal as well as economic discussion of the problems of double taxation among nations, with emphasis on the British practice.

The American student of public finance will look to Weise's book mainly for items of interest from the standpoint of American experiences and problems. It is, first, interesting to note that the British income tax is paid by legal persons, *i.e.*, corporations, as well as individuals. The "earned income allowance" (presently £450 a year) coupled with an age allowance, making it possible to treat small capital income of aged persons as earned, is an interesting example of what the author calls "personification" of the income tax, by which

he simply means inclusion of social policy goals in the tax structure. The diversified deductible allowances are based on social goals too; for instance, a life insurance relief is designed to encourage savings. The tax rates have been frequently changed; the highest rate of the present structure is 92.5 per cent in the bracket above £100,000. Many other aspects of the income tax, *e.g.*, the withholding system (called in Britain the pay-as-you-earn system), parallel the American practice with minor deviations.

The corporation tax over and above the income tax is a newcomer to the British tax system having been added only in 1947. Each corporation, with the exception of those with profits of less than £2,000, pays it in addition to the standard 42.5 per cent income tax. The profits tax is 3 per cent for non-distributed and 30 per cent for distributed profits and seems to encourage self-financing although the author tries to prove by long statistical examples that the importance of such a development should not be overestimated. There is an interesting treatment of dividends. The individual stockholder counts them as part of his income; but in computing his tax he deducts the amount of income and profits taxes related to his dividend paid by the corporation and shown on a statement which he receives. While, for instance, partnerships do not pay the profits tax, all nationalized industries and "statutory undertakings" (*e.g.*, municipally owned utilities) have been paying this tax, now at 3 per cent, since 1952. There are some exceptions to this, such as the British Broadcasting Corporation.

The United Kingdom has a relatively unimportant system of property taxation. The land tax now has only minor importance, especially since the charges since 1948-49 have been fixed and the tax rate is rather low. The estate duty is really the only important property tax especially since its consolidation in 1949. At the time of death this duty is levied on the property, minus debts and funeral costs, but is paid by the heirs. The tax on the total estate shows a rate from 1 per cent for an estate over £2,000 value up to 80 per cent for an estate over £3,000,000 value. Redistribution of income is an obvious reason for such steep rates.

The stamp duties on documents, etc., and the excises follow general practice. It is only worth noting that an effort is made to avoid double taxation. Duty-paying imports belonging to categories subjected to excise taxes (such as tobacco, beer, alcohol, etc.) are exempt from excises, but their duty equals the excise tax. Since 1952 British law has consolidated customs and excises which, in view of the British dependence upon imports, is not surprising. The purchase tax has really no American equivalent. It is paid by the wholesaler (producer, importer) on goods sold to the retailer. Again there is an attempt not to duplicate excises and customs duties. The ad valorem rates differ from, *e.g.*, 5 per cent for shoes to 95 per cent for cosmetics. The author emphasizes especially the economic aspects of the purchase tax. On the one hand, it could, by diversifying rates, be used as a means of monetary policy; on the other hand, it inevitably creates a burden on the export trade, even though exporters do not pay it. Production for export is rarely exclusive in a firm and so the production for internal consumption, under existing purchase-tax rates, has to be taken into account by the producer when he makes up his export plans.

The role of rates levied by counties and county boroughs is definitely minor. They are paid by the "users" of land and homes (*e.g.*, by the people renting an apartment) on the basis of the annual rent. Not only are valuation assessments now centralized in a London agency, but the rates, varying according to county, are not sufficient to cover local expenses and thus create county dependence upon grants from the national government (general Exchequer contributions) and diminish the freedom of action of the counties.

The final section dealing with double taxation in international relations describes the current British practice, showing by examples how the two principles—territorial and personal—of taxation are interwoven in the British system to take into account the new position of the United Kingdom in international trade as a debtor nation.

. Discounting the rather heavily legal terminology, the book is a valuable contribution to tax literature. English terminology is used in parentheses, detailed tables and examples contribute to clarity. The American reader will be especially interested in the British treatment of corporations, in the purchase tax, and in the tax implications of British governmental centralization.

<div align="right">JERZY HAUPTMANN</div>

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International Economics

Europe and the Money Muddle. By ROBERT TRIFFIN. Yale Studies in Economics: 7. (New Haven: Yale University Press. 1957. Pp. xxvii, 351. $5.00.)

As introduction, Triffin begins with a review of the book written by himself. According to this, Triffin expects his book may appeal mainly to "government economists and other self-styled currency or banking experts directly concerned with the intricacies of European monetary and exchange arrangements." It should also be of interest to other people less directly involved in these matters who would like to get an over-all view of the development of these arrangements.

The book is primarily an expanded policy paper. It does not pretend to be —nor is it—a detached objective analysis. The background chapters are history as seen by Triffin from his position as a participant in its making. The book begins with a discussion of why Triffin does not believe in any theory of chronic world dollar shortage; the following five chapters cover Europe's postwar recovery, the "failure" of the international currency plans, and the various European payments agreements. Then comes a chapter on convertibility and one on conclusions and recommendations.

Triffin is one of those rare economists who writes English beautifully. He is always interesting and stimulating, even if one may not agree with what he is saying at some particular point. In this book, he has tried a little too hard to be amusing; for example, he prefaces the list of charts and tables: "The reader who glances at the charts and tables in the text may save himself the trouble of reading the rest of the book."

In his dedication and footnotes, Triffin pays compliments to a number of

former associates. The list is extensive enough to lead anyone knowing the people who were concerned with negotiations on European exchange arrangements to compile a little list of those *not* mentioned. This list is perhaps even more revealing of past policy conflicts and alliances than the former: it excludes, for example, all U.S. Treasury and Federal Reserve personnel of the time.

There is one dominant theme running through the book—that there is needed an international institutional framework for trade and payments and that only through such machinery can there be the necessary harmonization of national policies that will limit the spread of recessions and the instability resulting from trade and payments restrictions, exchange rate manipulations, etc. Within this general theme the dominant note is that regional arrangements, primarily within Europe, are necessary to achieve these ends.

In arguing for the latter point, Triffin relies heavily on the test of postwar European experience; that is, Western Europe created regional machinery and Western Europe recovered rapidly, dismantled its bilateral arrangements and has made great progress towards general convertibility. Almost inevitably, he overrates the contribution made by the regional machinery to the progress of the European countries. The billions of U.S. aid and the countries' own efforts in restoring their economies and the progress made in internal monetary stabilization were much more important. But, Triffin unquestionably has the advantage in the argument with those who maintain that the regional machinery slowed progress toward world-wide convertibility since these must argue in terms of what might have been while he can point to the fact that concrete progress did occur. However, it does not follow that, because in this case the International Monetary Fund abandoned the field to the European Payments Union, regional machinery is always and forever the better approach.

Why did the IMF fail in this case? I think the answer is missing in Triffin's analysis of the "over-all failure" of the Fund in the postwar period. The Fund failed because the United States, the source of the recovery aid, refused to use the Fund but instead stimulated the formation of and backed and financed the regional organization. The fundamental reason for this vote of no confidence in the Fund stems from the original mistake made (for what seemed like very good reasons at the time) of not giving the key position in the IMF, the post of managing director, to an American that the U.S. government would trust. The result was that the Fund as an organization did not receive that degree of confidence from the U.S. government that would have made possible the use of U.S. aid to Europe to help carry out Fund policies. Another consequence was that the directors of the Fund, where the U.S. influence is directly exerted, have taken and maintained control of the Fund, with all substantive decisions being made day-to-day by a full-time board of 17 members. This is the reason why the directors cannot be primarily the policy-makers in their own countries, as Triffin believes they should be for the Fund to perform its function as an effective forum of negotiation.

There are a number of historical points where one is tempted to take issue with Triffin's interpretation. One example: he presents 1947 as the year of

"crisis" in Europe, while the years from 1948 on are years of recovery. But 1947 was not really a year of "collapse." It fits into the same pattern of recovery as the last six months of 1945, 1946, 1948 or 1949. The peculiarity of 1947 did not lie in the $7 billion deficit that Europe ran that year. It lay in the fact that the initial postwar arrangements for making U.S. aid available to Western Europe (over $9 billion) were inadequate, and by the middle of 1947 the funds had been practically all committed and were fast running out. The "crisis" of 1947 was the sharp realization by the United States that postwar recovery in Europe could not be completed with what had been done so far and that a whole new system of aid had to be devised.

The analytical description of the development of the various European payment arrangements should be helpful to all people interested. The book contains a useful collection of analytical tables and charts on this period. I must say I share Triffin's prejudice for showing—contrary to the usual practice—increases in monetary assets by a plus rather than a minus sign.

<div align="right">ANDREW M. KAMARCK</div>

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The United Nations and Economic and Social Co-operation. By ROBERT E. ASHER, WALTER M. KOTSCHNIG, WILLIAM ADAMS BROWN, JR., and Associates. (Washington: The Brookings Institution. 1957. Pp. xi, 561. $2.50.)

The present volume constitutes the economic and social portion of a comprehensive study of the United Nations which was begun by the Brookings Institution in 1951. The organization of the material is functional rather than bureaucratic, in an effort to appraise the total U.N. contribution to the objectives of the Charter, rather than to evaluate the performance of each U.N. agency in its own designated area. It is a painstaking, thoughtful, and well-balanced examination of the one-world dream amid the realities of economic and social conflict, and it records in sober tones the modest fulfillments and melancholy frustrations of that dream.

The specific appraisals that emerge seem to fall into a general pattern which accords with this reviewer's own experience while a consultant to the U.N. Secretariat. The United Nations has been most effective on the level of specification of important problems, technical studies of these problems, collection and publication of current data, and operations with narrowly defined functions and widely accepted aims (such as the Children's Fund, the World Health Organization, Korean Reconstruction, and technical assistance for economic development). It has been least effective on the level of the big issues and the expensive, large-scale operations (such as international schemes for countercyclical stabilization, and international finance for economic development, especially for the less "credit-worthy" nations). As the authors put it succinctly, "a prerequisite for effective co-operation among sovereign governments is not a preponderance of votes but a meeting of minds."

The meetings and the votes, which constitute the substance of this volume, are well set forth in terms of the procedural record and the stated aims of the United Nations. The volume gives rather thin treatment, however, to the

overt' results and their. relation to actual conditions, and consequently the "appraisals" are limited in depth. But what economists will miss most of all is a full discussion of the ideas and conflicts of ideas which underlie the various U.N. activities and debates. The authors touch on some of the theoretical issues, such as fixed versus flexible exchange rates, and the interdependence of all phases of a developmental process; but on the whole they sidestep the theoretical considerations which, by providing the link between a "cause" and its "effects," are indispensable for determining the contribution of a given action to "the objectives of the U.N. Charter."

Are there in fact certain economic doctrines or doctrinal contrasts peculiar to the United Nations and its agencies, or implied in their reports? The affirmative was indicated at a session of the American Economic Association meeting in 1953, when R. F. Mikesell criticized the reports of the central and regional secretariats and "groups of experts" for neglecting resource allocation and the virtues of the free market, while from almost the opposite pole J. J. Spengler criticized the reports of the International Bank's missions for neglecting certain dynamic considerations and the virtues of suitable intervention in the free market! (See Papers and Proceedings, this journal, May 1954, pp. 570-99.) An ideologic cleavage of this kind, dividing the U.N. agencies into two or more conceptually armed camps, might be traced further in many respects: in the matter of investment criteria (such as the capital-intensity issue, and the debate over "balanced growth"), in the treatment of changes in the terms of trade, in the degree of emphasis on structural change (including programming, tariff protection, and the developmental use of fiscal policy), and in general a philosophy of adjustment contrasting with a philosophy of transformation.

An investigation of such differences in viewpoint would be an important contribution to the semantics of international discourse, and might help to shift U.N. debates from repetitive declarations of preferences to the fundamental questions of necessary and sufficient conditions for the ends in view. Since the present volume was already too large to include such an investigation, it is to be hoped that future studies will be devoted to this conceptual matter which is basic to rational policy.

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Eighth Annual Report on Exchange Restrictions. (Washington: International Monetary Fund. 1957. Pp. lx, 375.)

A complete survey of the exchange practices and payments restrictions employed in 72 countries (of which 60 are members of the International Monetary Fund) is contained in this report. For the most part, the report consists of a short summary of recent developments in exchange restrictions, and a country-by-country description of exchange and trade practices, as well as changes in these practices during 1956. In the introduction it is stated that "further progress has been made toward its [the IMF's] objective of establishing a fully multilateral system of payments," but that on the other hand "a large majority of the Fund members still retain exchange control ... and that

in many instances it is not possible to predict an early establishment of a truly multilateral payments regime."

Close examination of the Report reveals the validity of both statements. Taking the second statement first, it appears that 49 of the 60 members of the Fund still maintain trade and exchange restrictions under the postwar transitional period arrangements of article XIV of the Fund's agreement. These restrictive practices vary over a wide range which almost precludes any general statement. A great many countries apply restrictive regulations to import of goods and services, which in most cases relate both to the type of commodity and to the country of origin. Multiple exchange systems are widely practiced, and many countries maintain a freely fluctuating rate along with one or more fixed exchange rates.

With regard to the first statement, it is clearly evident that some progress toward convertibility was made during 1956, but that it was quite sporadic and did not encompass all countries (some nations even tightened their exchange restrictions). While the amount of progress made and the manner in which it was achieved varied greatly from one country to another, it is possible to discern two major patterns which the progress seems to have followed.

First, in attempting to simplify their exchange systems and facilitate improvement in their external position, some countries replaced their complicated multiple-rate systems with a single fluctuating rate. The fluctuations are then moderated by the intervention of the exchange authorities in the market (i.e., the exchange stabilization fund technique). Such a policy was adopted, for example, by Bolivia, Chile and Colombia. In most cases this change was accompanied by restrictive monetary and fiscal measures designed to improve the external position of the country. This trend demonstrates in part the unwillingness of countries to rely entirely on internal adjustments for the restoration of equilibrium in their balance of payments. It reflects a compromise between the need for exchange stability on the one hand, and the desire for domestic price and employment stability on the other. At least in some countries it is possible to distribute the burden of adjustment between exchange fluctuations and restrictive monetary and fiscal policies. While this system is contrary to the postwar decision in favor of exchange stability (with certain provisions for discontinuous alterations when permission is granted by the IMF), it may, for some countries, be the only way to achieve convertibility. It is quite possible that the successful experiment of Peru with such a system is responsible for its adoption by other countries (see S. C. Tsiang, "An Experiment with a Flexible Exchange System: The Case of Peru 1950-1954," *IMF Staff Papers*, Feb. 1957, V, 449-76).

The second trend which emerges from the Report is the relative success and widespread adoption of regional schemes. Regional economic arrangements were fostered during the postwar era not as an end in themselves, but as a stage in the progress toward a complete multilateral system of trade and payments. It appears now that the Organisation for European Economic Co-operation countries in Europe are gradually liberalizing their trade; that new regimes of trade and payments have been established between certain European and South American countries (the "Paris Club" including Argentina and 11

European countries, and the "Hague Club" including Brazil and 8 European countries); that progress has been made towards the European common market (including 6 European countries); and that discussions are under way about regional payment arrangements in South America and a custom union for the Scandinavian Countries. All these arrangements seem to have brought about a larger degree of currency transferability and more liberalized trade. Through strengthening the region's economic position, they subsequently enable further relaxation of restrictions on trade with countries outside the region.

The Report also discusses in detail the policies regarding capital movements, and can thereby shed light on the attitudes of underdeveloped countries toward foreign investments. While some of these countries still impede the inflow of foreign capital or at least are neutral toward it, many of them apparently do realize the importance of foreign investments for their economic development. At least 11 such countries grant preferential treatment to foreign capital. The preferential treatment ranges from pure laws of protection of the investments to their exemption from corporation taxes and import duties. Most laws include provisions for repatriation of capital and profit.

The Report is written in an informative manner. However, for the student of international finance who is interested in the general progress toward convertibility rather than in practices of individual countries, a more extensive summary would be helpful. On the other hand, the economist who is interested in exchange and trade developments in particular countries, would benefit greatly from some discussion of the economic conditions which gave rise to these developments.

MORDECHAI E. KREININ

Michigan State University

New Emphasis on Economic Development. (Washington: International Development Advisory Board. 1957. Pp. 18. Free.)

The Role of Foreign Aid in the Development of Other Countries. (Chicago: Research Center in Economic Development and Cultural Change, University of Chicago. 1957. Pp. 88. 30¢.)

The Objectives of United States Economic Assistance. (Cambridge: Center for International Studies, Mass. Institute of Technology. 1957. Pp. 73. 25¢.)

American Private Enterprise, Foreign Economic Development, and the Aid Programs. (Washington: American Enterprise Association. 1957. Pp. 68. 25¢.)

Economic Development Assistance. (New York: Committee for Economic Development. 1957. Pp. 37. 25¢.)

Report to the President by the President's Citizen Advisers on the Mutual Security Program. (Washington: Supt. Docs. 1957. Pp. 36. 50¢.)

These six recently published reports on the appropriate role of U. S. foreign aid in promoting economic development present a broad cross-section of views on several major policy issues. By and large, however, the reports are characterized by advocacy rather than analysis. In only a few instances do they provide useful discussions of the factors to be evaluated in reaching a decision on

a given problem, although in some cases one is able to get a reasonably clear idea of the nature of the problem by reading several reports together.

There appears to be general agreement that the ultimate objective of development assistance is the promotion of more mature, stable, and democratic societies. While most of the reports do not examine the means by which such aid may lead to these objectives, we are presented with two different hypotheses on the contribution of development aid: increased political stability may result from the process of development itself, or—a more questionable assumption, and at best one of long-run character—from higher levels of income. The first hypothesis appears in both the International Development Advisory Board (I.D.A.B.) and Chicago studies, though it is understandably developed most completely in the Massachusetts Institute of Technology (M.I.T.) study:

The existence of an aid program may affect the objectives of a country's leaders; even before substantial growth is achieved, such a program "can sharply alter . . . [the leaders'] conception and evaluation of the choices they confront. Specifically, it can reduce the relative attraction of aggressive foreign policies" (p. 22). Once a development program is in effect, it can alter both the structure of the society and the goals of many members of that society in a manner tending to promote the establishment of mature democratic institutions. It can, by channeling energies toward projects benefiting most groups in society, "provide a sense of community in which seeds of class struggle and violence tend to die"; it can provide "new and constructive options for advancement for the ambitious young who are not of the old ruling groups"; by increasing social and economic mobility it can reduce "the unilateral dependence on particular employers or on the approval of particular communities," and thus dilute "the monopoly position of traditional wielders of power and provide the basis for those 'checks and balances' at all levels of society which mark a mature democracy" (p. 21).

Not surprisingly, the reports that place the greatest emphasis on gains to be derived from the process of development are those advocating the largest programs. The recommendations for publicly financed aid range in size from a program limited to the current lending of the Export-Import Bank and International Bank, as proposed in the American Enterprise Association (A.E.A.) study, to one calling for an increase of perhaps $2 billion over present U. S. aid expenditures (M.I.T. and Chicago studies).

The Chicago report, and the M.I.T. report also if read in conjunction with the Millikan-Rostow study,[1] provide the estimates which form the basis of their recommendations. The failure of the others, except the A.E.A. report, to explain the process used in arriving at their recommendations renders them rather useless for purposes of analysis, and the reader is left to accept or reject the quantitative recommendations on the basis of the authors' reputations.

The relative role assigned to private investment is apparently also dependent on the size of the development program. The A.E.A. report, which necessarily relies almost exclusively on private investment, argues that such investment is

[1] M. F. Millikan and W. W. Rostow, *A Proposal: Key to an Effective Foreign Policy* (New York, 1957).

less likely than public aid to lead to disruption in a less developed country, since private capital will be employed only where there are complementary indigenous resources—in effect, the flow of U. S. capital to less developed countries is to be determined by the operation of the price mechanism.

The other reports recognize that private investment can assist in economic development, but none appears to believe that such investment can be expected to play a major role in contributing to development in several major areas—particularly in Asia and Africa. The Chicago study (p. 54) is the most pessimistic concerning the contribution to be made by private investment, primarily because it finds that investment in extractive industries "may provide little stimulus to the rest of the economy, normally, because these export commodities are produced in plantations or mines which technologically are completely different from traditional forms of production." This intriguing hypothesis is unfortunately not developed. The principal public policy recommendations in these reports for promoting investment are government guaranties against inconvertibility and expropriation, joint investment of public and private capital, and an expanded information program.

Virtually all of these reports advocate an increased emphasis on loans-in-aid programs; the Committee for Economic Development (C.E.D.) argues, for example, that grants "would tend to erode those very qualities that development assistance aims to build up (p. 29)." Grants are generally approved for such purposes as technical assistance, however, and in the M.I.T. and Chicago reports grants are considered appropriate in cases where the prospects for repayment of loans are small. In some reports (M.I.T., C.E.D., and I.D.A.B.), loans repayable in local currencies are advocated as an intermediate step between dollar loans and grants. The Citizen Advisers Report (p. 10) strongly argues against such arrangements, however, pointing out that "our relations with other countries will suffer from U.S. control of large amounts of their currencies." The one point on which virtually all of these foreign aid reports agree—that assistance should be provided on a sustained basis to permit long-range planning of development projects—has been accorded only limited Congressional acceptance.

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International Economics—Theory, Practice, Policy. By JACK N. BEHRMAN and WILSON E. SCHMIDT. (New York: Rinehart & Co. 1957. Pp. xxv, 561. $6.50.)

The authors of this refreshing new text are clearly competent craftsmen. It is evident that they understand the international economy and that they are not afraid to comment on it. They are skilled in the art of simplifying the theory of international trade. They possess that rarest of all skills—the ability to write a highly readable text for the student, yet one which does not offend the professional reader.

While the organization of materials is not new or unusual, it is mentioned here because it indicates something of the economy of space and the logical

structure of the work. Part I, The Theory of Trade and the Theory of Policy, contains 3 chapters (58 pp.). Part II, Development of Theory and Policy Through the Nineteenth Century, contains 4 chapters (86 pp.). Part III, External and Internal Disturbances and Their Adjustment, contains 8 chapters (198 pp.). Part IV, United States Policies on Aid, Trade, and Investment, contains the remaining 9 chapters (202 pp.). Pure theories of international trade really do not yield as much to practice and policy as the space allocations indicate.

In Part I, those seeking the sophistication of modern trade theory, or a careful development of the history of that portion of economic thought relevant to the international economy, will be disappointed. However, the undergraduate will not miss them, and the teacher may be grateful for a lucid development of a simple but adequate theory. Students and teacher may both tire of Mexican-made leather belts and U.S.-made baby buggies before the example completes its work. Yet Ricardo, with cloth and wine, did no better in making graphic simplicity of complex logic.

The contemporary task of illustration is more difficult than Ricardo's—because more is known; yet easier—for the final result is more satisfying. The student is not asked to accept a labor theory of value, international barter, nor a constant marginal productivity of factors. Thus only a few assumptions need be discarded to achieve "realism"; i.e., those of full employment, competition and constant returns to scale. A two-factor production function is employed for each country. The cost-price basis of trade is easily developed by then assuming dollar and peso prices, and an exchange rate. Trade equilibrium is achieved by movements of the exchange rate (or by flows of international money, if such is employed), by product-price changes, and by re-allocations of resources. Diminishing returns are considered when appropriate; not "later." Differing factor endowments seem obviously the bases both of different factor prices and of trade, in this context. It is thus easier to make the general case for freedom of trade. It is also easier, assuming the appropriate circumstances, to identify the economically sound cases for the restriction of trade.

Part II combines a history of doctrine with a history of selected Western nations' commercial policies. Neither, of course, can be adequately treated when a total of 86 pages is devoted to both. Mercantilism is sketched in lightly; yet, "neomercantilism" is recognizable and available for later reference. The treatments of the ideas of Smith, Ricardo and Mill cannot be called adequate. The situation is nearly saved, however, by successfully tracing the impact of these ideas to selected historical developments and policy changes. This reviewer favors such a method of teaching both doctrines and history, hence may be unduly forgiving of a light treatment of doctrine as such. Like Part I, this Part is concluded with a chapter on factor movements. It deals largely with nineteenth century history, but lays a good foundation for the discussion of twentieth century problems in Part IV. It also illustrates well the theoretical discussion, in Part I, of the role of factor mobility.

Part III is probably the strongest part of this text. Beginning with international payments, it proceeds to reserves of international currency, thence to monetary and fiscal problems and policy alternatives, and finally to ex-

change rate problems and systems. A "basic equation"—GNP + Imports
= Exports + Domestic Expenditures—is employed to its maximum advantage. The term "domestic expenditures" is a bit misleading, but otherwise the scheme should lead the student well into the mysteries of national and international financial problems. Product and income flows are often but imperfectly distinguished from money flows by the student—and to expect him to *relate* the two flows is usually asking too much. One of these chapters (Ch. 11) may succeed where others have failed. Chapter 12, Monetary and Fiscal Policies, is as good a textbook discussion of the open economy as can be found.

Part IV is entirely devoted to U.S. commercial policy, though this involves a good deal of analysis of European recovery and cooperation. There are two chapters on economic development, and there is one on trade controls for "cold war" strategic purposes. This entire section is well written—almost "breezy." Despite a tendency to glibly ascribe motives to the participants, this history of postwar developments is a remarkably good job.

The arrangements of topics, the use of subheads and the careful (and parsimonious) use of simple demand-and-supply diagrams should be helpful to the student. So also should the use, in the first three Parts, of appendices for certain technical materials. It might be wished that a similar practice were carried over into Part IV, providing more technical descriptions of some of the international institutions such as the International Monetary Fund.

This text is recommended for the many undergraduate students who are not (and do not wish to be) professional students of economics. Graduate students and other hardy souls might develop sharper teeth if fed a tougher diet.

DONALD A. MOORE

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Business Finance; Investment and Security Markets; Insurance

The Empire of High Finance. By VICTOR PERLO. (New York: International Publishers. 1957. Pp. 351. $5.50.)

The theme of Victor Perlo's book has been succinctly expressed by Lenin in the following passage (quoted by Perlo): "Finance capital, concentrated in a few hands and exercising a virtual monopoly, exacts enormous and ever-increasing profits from the floating of companies, issue of stock, state loans, etc., tightens the grip of financial oligarchies and levies tribute upon the whole of society for the benefit of monopolists" (p. 21). Or, in Perlo's own words: "Wall Street is the apex of monopoly power, and a symbol of the extreme concentration of that power under modern conditions. . . . The economic life of the country is largely controlled from this area no larger than a baseball field" (pp. 30-31). Perlo has written this book ". . . to increase public understanding of the structure and operations of American monopoly capitalism" (p. 17). He believes "We are exposed to distortions in newspapers and broadcasts, in the speeches of politicians and the books of professors" (p. 14).

The book is divided into three parts. Part I deals with the relations between finance and industry, describes the composition of the "spider web"—chief

institutions ·are the banks of various kinds, insurance companies, investment trusts, holding companies, foundations, and the great corporate law firms— ."through which the oligarchy runs the economic life of America" (p. 61), and describes some of the ways in which control is used to garner extra profits (pp. 55-57). The mechanism of control includes interlocking directorates, provision of banking services, bank trust funds, stockholdings, ties through legal firms, etc. Part II deals more specifically with the main financial empires (Morgan, Rockefeller, DuPont, Mellon, the Cleveland group, Chicago group, Boston interests, First National City Bank group, Bank of America group, and various lesser empires). The "duumvirate" (Morgan and Rockefeller), with control of $127 billion of assets between them, constitute the "giant empires of America" and are surrounded by "lesser kingdoms." Part III purports to show the merger between the control groups and the federal government in which ". . . the government has become in large measure an integrated part of the business structure. It is run by as well as for monopoly, and the key men of Wall Street are also the key men of Washington" (p. 270). In Part III, we also learn that although ". . . official publicity, backed by a whole school of academic economists, has emphasized the types of government regulation supposedly designed to stabilize the economy and avert depression . . . the main types of government intervention, with the maximum impact on the economy, are designed primarily for purposes quite remote from economic stabilization" (p. 255).

Perlo's solution for the problem of concentration which he has described is tucked away in a short paragraph at the end of Part I. "The moral is clear: anti-monopoly legislation cannot hope for decisive, lasting success so long as it stops with partial measures. In the long run, nothing less than the ending of private ownership and control of the key industrial and financial concentrates can prevail over the manifold devices of the financial oligarchy" (p. 123).

Perlo's contention that an oligarchy of bankers largely controls the economic life of the country runs directly counter to the prevailing view among economists who have investigated the matter. To substantiate his charge, Perlo has packed the pages of his book with names and figures, including fifteen appendices with a detailed breakdown of the concentration totals cited in the text. Despite the mass of names and figures, Perlo's indictment is unconvincing.

First, and most important, Perlo's conception of the criteria of the concentration of control is unacceptable. The sinews in Perlo's exposé of banker monopoly control and exploitation include interlocking directorates, provision of banking services, commercial bank trust funds, stockholdings, use of common legal firms, etc. These relationships are not inherently sinister. To be sure, some of these could be abused, but, *in and of themselves,* they are ordinary business relationships. They are not, as Perlo virtually treats them, prima facie evidence of concentration and banker control. Consider, for example, his discussion of the trust accounts held by commercial banks. Perlo tells us that the total sums held in trust accounts are huge, that these accounts are concentrated in some of the country's largest banks, and then rushes the

reader to the conclusion that "The main point is clear enough—through trust activities a few New York banks and the influential families behind them exercise a vital, and often decisive lever of control over all of the key points of the economy" (pp. 71-72). It is unlikely that many readers will think Perlo's conclusion is warranted by his facts. To translate the large trust accounts in some banks into "a decisive lever of control" in particular companies would require a case-by-case, detailed demonstration of the exact manner in which trust accounts *have actually been employed* for sinister banker control. Perlo has not done this for trust accounts nor has he conducted a similar investigation for his other criteria of control.

Second, for the overwhelming majority of companies cited in Perlo's book, the reader is told absolutely nothing about the links which are alleged to place a particular company under a particular "sphere of influence" and thereby to increase concentration. (*Cf.* the listing of almost 200 companies in Appendices 8-15). The reader is left to surmise that any given company was included because, in Perlo's private judgment and from his investigation of the facts, that company, in some unspecified way, and to some unspecified degree, fell under Perlo's general criteria for control.

Third, Perlo's interpretations and judgments about the significance of his control criteria are highly subjective, and Perlo does not reveal how he determines the weights for the various control elements in any particular case. Consider his discussion of the Ford Motor Company. Perlo relates on page 176 that the First National City Bank ". . . provided financial service to the Ford family over a considerable period. That relationship became more intimate with the public sale of Ford Motor Co. stock in 1956. First National City's law firm advised the Ford family, its investment banking affiliate, Blyth and Co., headed the selling syndicate, and the bank itself became Ford's stock transfer agent." The reader might be pardoned for suspecting that (by Perlo's criteria) the "spider web" had ensnared another giant company in the mesh of banker control. The reader would be mistaken, for Perlo concludes this formidable list of his own criteria for control relationships with the statement that "Despite the growth in relationships, . . . Ford . . . can [not] be considered as controlled by First National City Bank" (p. 176). The puzzled reader is left to wonder just how Perlo does decide when the control elements become decisive. In other words, just how does Perlo determine when a formerly independent company becomes a controlled satellite?

Fourth, Perlo's discussion of the consequences of control is unsatisfactory. Perlo's main emphasis is on the profits of control. There is virtually no analysis of how the alleged control affects the firm's operations in particular markets, *e.g.,* in terms of price policy; the effects on costs, entry, rate of innovation, etc., are not even mentioned.

Fifth, the limitations of some of the statistics are disturbing. For example, Appendix 1 lists the "Corporate Shareholdings of DuPont, Mellon, and Rockefeller Families, *April 1956*" (my italics), and thus purports to be *current* data. The value of the family holdings in April, 1956 for each of the listed companies was calculated by multiplying the number of shares held by each family times the market price per share on April 30, 1956. That much is un-

objectionable, but it is disturbing to learn that (with certain exceptions) the number of shares held by each family was derived by multiplying the shares outstanding on April 30, 1956 times the percentage of shares outstanding held by each family as listed in the prewar TNEC #29!

In the economic as in the political sphere, constant vigilance must be exercised to uncover abuses and undesirable developments. Perlo's book purports to expose the control of the economy by a financial oligarchy. In the opinion of this reviewer, however, Perlo's charge remains unproved.

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Business Organization; Managerial Economics; Marketing; Accounting

Automation in Business and Industry. Edited by EUGENE M. GRABBE. (New York: John Wiley. London: Chapman & Hall. 1957. Pp. xix, 611. $10.00.)

Automation: Its Purpose & Future. By MAGNUS PYKE. (New York: Philosophical Library. 1957. Pp. 191. $10.00.)

The Economic Consequences of Automation. By PAUL EINZIG. (New York: W. W. Norton. 1957. Pp. 255. $3.95.)

Automation is a technological development that promises to affect economic life and thought as profoundly as the factory system did. These three books are, in different ways, contributions to a burgeoning literature on automation that cannot safely be overlooked by economists.

Grabbe's book is a collection of lectures by prominent engineers and scientists who participated in an engineering extension course at the University of California in 1955. Only 4 of the 20 papers deal at all with any of the economic, management or social problems connected with automation, and even in these the coverage is meager. The book mostly explores the theories of feedback systems, instrumentation and control, analog and digital computers, input-output equipment, process control and so on. To be properly appreciated, a considerable part of the book requires some engineering background of the reader. This book is probably a valuable summary of technical developments which this reviewer cannot fully appreciate. However it also adds to the evidence that when physical scientists and engineers transcend their disciplines to ponder broader economic and social problems, as they are prone to do, they frequently either fall easily into oversimplifications, false analogies and clichés or else base their analysis on a rigid determinism that is long out of fashion in the social sciences.

Pyke covers much of the same territory as Grabbe (and at least a dozen other new books in the area) but the emphasis is on applications to industry. It contains many revealing illustrations, mostly from British experience since the writer is a British engineer. Applications of automation in the chemical, petroleum, transport, engineering and food-processing industries are discussed as well as interesting developments with accounting, guided missiles and lan-

guage translation. Only the 2 short final chapters, out of .13, touch on eco-
nomic and social effects, but these are written skillfully and with philosophical
insight and imagination. The possible consequences of changing job require-
ments and increasing leisure time are most provocative. Pyke seems to have
a better grasp of the social sciences and management than the American con-
tributors to the Grabbe volume. The book is written in the characteristically
lucid style of the British and the informed layman will find it a valuable hand-
book on the uses and potentialities of automation.

Einzig, a well-known British financial journalist, has come closer to writing
a book on automation which will be of interest to economists than anyone
else to date. He has consciously tried to cover all of the areas in which auto-
mation is likely to have significant economic consequences. He considers
effects on employment, production, inflation, capital investment, fiscal mone-
tary considerations, balance-of-payments problems, underdeveloped countries,
national defense and so on. Unfortunately his definition of automation is not
very precise, the result being that in many places automation is confused with
technology or even economic progress in general. The analysis is frequently
good, especially concerning production and inflation (Ch. 8 and 9) but since
the definition of automation is obscure the book does not achieve what it might
have. Because of its lack of clearly defined premises the book fails to make a
very good case for delineating a separate field of automation economics, much
as this reviewer would like to see such a case made.

In addition to this definitional failure several other objections can be raised.
First, the main thesis seems to be that automation necessarily causes unem-
ployment with what are considered to be desirable disinflationary results. This
premise is certainly more relevant to Great Britain (with its powerful unions,
public sympathy for labor and reliance on fiscal rather than monetary policies)
than to the United States if it is valid at all.

Second, and more serious, the analysis is frequently erroneous or oversim-
plified. For example, it is argued (p. 112) that if firms passed on automation
economies to consumers through lower prices this would lead to a "sweeping
wave of bankruptcies" which would, in turn, lead to unemployment and world-
wide depression. In another place (p. 171) he credits the great savings of the
wealthy in the nineteenth century for preventing a great inflation.

In still another example (p. 69) he argues that full employment causes
a decline of industrial discipline, an increase in irresponsible strikes and a lack
of inducement for workers to maintain high productivity. There are plenty
of facts to disprove all of these conclusions in spite of the reasoning on which
they are based. In the United States, for example, labor turnover in manufac-
turing in 1956 was below the 1 per cent minimum of 1933 in spite of full
employment. Productivity is at an all-time high, again in spite of full employ-
ment. British statistics of recent years also show that productivity can be
higher under full employment than unemployment, possibly because labor-
saving equipment and methods are more readily accepted by workers during
full employment when the "lump of labor" theory is more easily discredited
and because morale is an important determinant of productivity. As for in-
dustrial discipline, the weight of authority is that the threat of unemploy-

ment and destitution is among the poorest incentives for productivity. Unemployment merely separates those managements which can lead from those that can only drive. In general the book is characterized by long chains of reasoning, oversimplification and generalized conclusions sometimes based on few, if any, facts.

Third, the book appears to have been hurriedly written. It attempts to cover every conceivable angle of automation with reasons, advantages and disadvantages but without firm conclusions or factual proof. Hence although it is provocative it is occasionally unnecessarily wordy and there are contradictions, overlappings and even fallacious reasoning in some places. For example, it is asserted (p. 47) that "if there is an increase in demand for certain goods in relation to their supply, the result is a rise in their prices. This again tends to discourage demand and stimulate an increase in production. Conversely a fall in demand . . . tends to cause a fall in prices which again tends to encourage demand and to discourage production." The obvious conclusion here is that changes in demand are self-defeating. More sophisticated readers will immediately see the fallacy of defining demand in different ways depending on how it is used. In another place (Ch. 7) categories are established for further analysis and then they are not followed.

All of these criticisms, however, should not detract from the fact that this book is an important contribution to the popular understanding of automation and its effects. Although the book is not closely reasoned, the author demonstrates a wide grasp of economic subjects and an ability to write persuasively about economic problems for the general public. Furthermore, and most important, this is the first book on automation to be written by an economist and to be concerned primarily with *economic* consequences of automation. The market is glutted with books and articles by engineers and businessmen on economic consequences. Now, at last, is a book by an economist. The foregoing criticisms should not obscure the importance of the fact that this is a pioneering contribution.

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Marketing Management: Analysis and Decision. By JOHN A. HOWARD. (Illinois: Richard D. Irwin. 1957. Pp. xiii, 429. $6.50.)

Books on marketing and its problems differing in varying degrees from the traditional marketing textbooks have appeared in the recent past. These recent books tend to be more theoretical and oriented more to problems than to a descriptive approach.

Professor Howard's book, which is quite analytical, is oriented around decision-making. Considerable effort is made to separate out the variables in market situations and, after the analysis, to bring them together again as a basis for problem-solving.

The approach is eclectic, although basically economic in emphasis. The author quite intentionally uses conceptual tools from economics, psychology and sociology wherever one or the other or all three seem appropriate, with the rather acceptable viewpoint that man's behavior is revealed by each of the

several social sciences and that their contributions are important to each other.

The scheme of the book is straightforward. After a short two-chapter introduction orienting the book around profit-making considerations, the author goes into the forces which impinge on marketing decision-making—forces which have to be sorted out, analyzed and catalogued to see what variables the situation really contains and what weights must be assigned to the variables.

With this in view successive chapters take up competition and marketing, demand analysis, cost considerations, channel situations and questions of the law and marketing. These seven chapters comprise the section labeled "Typical Considerations in Marketing Decisions."

The final section—six chapters grouped under "Marketing Decisions"—sets forth the areas typically troublesome to executives and hence the phases of marketing activity most likely to require attention and decision-making. In this section are chapters setting forth an analysis of product decisions, channel, price, promotion and location-decision situations.

The character and tight workmanship of the text can be suggested by setting out in a little detail Howard's treatment of demand analysis. The chapter on the economic theory of demand sets forth in a clear and precise way the traditional apparatus of demand but adds analysis of dimensions of demand not typically given much attention in economic analysis—the impact of time and space on demand strength. After the treatment of the economic apparatus the author exerts considerable effort to "go behind the curves." This chapter is a very clear analysis of consumer behavior, approaching the problem of consumer choice from the point of view of individual motivation. Insight is added by treating the individual as part of a small group which in turn is but a part of the broader social stratification. This chapter is a good illustration of Howard's attempt to synthesize the offerings of several disciplines. The last of the three chapters on demand takes up demand forecasting and so rounds out the topic.

Other sections of the book exhibit the results of equally detailed workmanship and of the author's strong endeavor to isolate market forces for study and analysis. For those who favor a profit-maximizing orientation leaning rather strongly on marginalism and using a step-by-step analysis this book will afford a treatment to their liking.

PERRY BLISS

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Industrial Organization; Government and Business; Industry Studies

Congress and the Monopoly Problem: Fifty-six Years of Antitrust Development, 1900-1956. Prepared by the Legislative Reference Service, Library of Congress for the House Select Committee on Small Business, 85th Cong., 2nd sess. (Washington: Supt. Docs. 1957. Pp. x, 662.)

This publication by the Select Committee on Small Business of the House of Representatives represents a compendium of legislation, legislative propo-

sals and statistics dealing with the nation's antitrust laws for the first 56 years of the present century. In addition to summarizing antitrust laws, their jurisdiction, statutory exemptions from the laws, and proposals for legislative action from the turn of the century, this volume contains a digest of recommendations of the Temporary National Economic Committee (1942) and of the report on "Monopolistic and Unfair Trade Practices" of the 80th Congress (1948). Particularly helpful is a grouping of all laws and bills by subject-matter so that the reader can obtain a complete picture of laws dealing with a particular phase of antitrust activity.

A final section lists annual appropriations going to the Antitrust Division and to the Federal Trade Commission for its antimonopoly work. In 1956 the combined sums utilized in antimonopoly activity totaled less than $5.3 million, or .007 of 1 per cent of the federal budget for that year. As to the success of the two agencies in the work handled, out of approximately 1300 charges filed from 1900 to 1956, the Antitrust Division won 834, lost 322, with 148 still pending, for a respectable legal batting average of .641. Between 1917 and 1956 the Federal Trade Commission completed 1,307 antimonopoly charges, which resulted in orders to cease and desist in 756 cases, with charges dismissed in 551 cases, for a legal batting average of .578. Possibly no other expenditure by the federal government has over the years for so little in funds produced so much in valuable economic results—the maintenance of vigorous competition in many, if not most, areas of the private economy.

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Trucking Mergers, Concentration, and Small Business: An Analysis of Inter-state Commerce Commission Policy, 1950-1956. By WALTER ADAMS and JAMES B. HENDRY. (Washington: Supt. Docs. 1957. Pp. vi, 154.)

This report, prepared for the Senate's Select Committee on Small Business, presents an intensive and thoroughly documented analysis of recent concentration trends and the 1950-56 record of Interstate Commerce Commission action involving the combination of firms in parts of the motor trucking industry.

In fulfilling the Committee's assignment to study the ICC's administration of Section 5 of the Interstate Commerce Act, the authors submitted a three-part study. Part I is a quantitative analysis of mergers and increases in concentration during the 1950-56 period. Three segments of the industry, general freight, automobile, and petroleum carriers, are closely scrutinized. The second part reviews and criticizes ICC decisions on merger applications. The final portion, an exhaustive study of household movers, seemingly integrates and substantiates the findings of the first two parts.

Among the conclusions reached are these: Concentration in the trucking industry has increased through mergers. The increase cannot be attributed to or justified by economies of scale. The extent of concentration is not necessarily alarming and there is no broad charge that oligopolistic practices prevail. In some segments of the industry and possibly in some regions, smaller carriers have found their markets absorbed by larger firms. The ICC has not been consistent in its consideration of merger proposals. "Giant" carriers

(defined as those with annual operating revenues of $2.5 million or more) fared better before the ICC than did smaller carriers.

Of particular importance is the duality exhibited in ICC decisions. For example, the merger of large trucking firms is approved because the combination is not shown to be contrary to the public interest while a similar proposal from smaller firms is denied because the merger has not been demonstrated to be in the public interest. Further examples of "schizoid" ICC actions are found in the matters of unlawful operation and dormant rights. Of very little or no significance to the ICC is the single ownership of several firms (in some cases through holding companies) and the integration, often in a conglomerate sense, of many transportation activities.

The extensive documentation of this report is outstanding. Other analysts will undoubtedly wish they had such an amount of information available. Students of the problems of small business, mergers and concentration, and government regulation and control of business will find this a valuable study.

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American Commodity Flow. By EDWARD L. ULLMAN. (Seattle: University of Washington Press. 1957. Pp. xxii, 215. $4.00.)

The nature of this study is indicated by its subtitle: A Geographical Interpretation of Rail and Water Traffic Based on Principles of Spatial Interchange. Accordingly the volume is devoted to a description of rail and water traffic flows in American domestic and foreign trade, primarily through the medium of 164 maps, with some accompanying textual interpretation. The result is said to be "a new representation of the geography of linkages and connections to be set alongside the well-known static geography of distinctive production and consumption regions and climate and terrain patterns" (p. 1).

The features of American rail traffic flows which the author finds worthy of note are the predominance of east-west movements, often involving heavy grades, despite the sensitivity of railroads to grades; the concentration of traffic in the industrial northeast and adjacent areas of food and fuel production; the large volume and relatively long hauls of Appalachian coal traffic; and the long-haul movements of lumber and fruits and vegetables from distant producing areas to the northeast. An 8-page chapter entitled "The Bases for Transportation and Interaction" finds the explanation of this traffic pattern in three factors: complementarity, or areal differentiation; intervening complementarity or opportunities, *i.e.*, between two more widely separated areas; and transferability or distance, measured in real terms of transfer and time costs.

According to Ullman, "The most important feature of the pattern of state-to-state rail flows . . . is the concentration of flows in the region of state of origin and destination" (p. 28). This localization means "a modification of the meaning attached by some to the traditional concept of the United States as the largest national market in the world. . . . It definitely is not one market for most producers, and yet it provides the opportunity for one market for a significant, critical number of volume producers of finished products or of naturally localized primary products like certain minerals and fruits" (p. 30).

The foregoing findings will not be new or surprising to most economists, particularly to those working in the fields of transportation, interregional trade, and location theory.

An unavoidable limitation of studies of this sort is that they cannot give a complete picture of traffic flows. This arises from the fact that data comparable to the Interstate Commerce Commission's waybill sample used to depict rail traffic flows are not available for other agencies of transportation. Complete data probably would not materially change the picture of traffic flow so far as direction is concerned but might well show material differences in the relative volume of short- and long-haul movements. Certain limitations peculiar to the present study may be noted. First, rail traffic flow data are presented for 21 states; these are said to be representative states but the basis of selection is not stated. Second, the rail traffic flow data presented are for 1950. The author states that sample studies of individual states for later years showed no important change in traffic pattern; nevertheless use of the latest available data would seem to be desirable. Possibly one reason for the use of 1950 data was the availability of I.C.C. studies which tabulated state-to-state movements for that year in usable form, thereby sparing the author the necessity of a great deal of tabulating. Finally, the author's rail traffic flow maps (3 by 4½ inches in size and two on a page) are inconveniently small for readers desiring an accurate measure of the relative volumes of originating and terminating tonnage in the various states. Probably considerations of economy in publishing dictated this arrangement, but considering the central importance of these maps for this study the scale might well have been increased. However, these comments should not obscure the fact that Ullman has performed a useful and ambitious task in a field which can profitably be further developed.

ROBERT W. HARBESON

University of Illinois

The Economics of the Fur Industry. By VICTOR R. FUCHS. Columbia Studies in the Social Sciences, no. 593. (New York: Columbia University Press. 1957. Pp. xi, 168. $5.00.)

Written for both economists and members of the trade, this volume concentrates on the fur industry in the United States since the first world war. Including in the industry the many protagonists from producer to retailer, Mr. Fuchs examines two of its most important problems: incessant fluctuations in business activity and absence of growth in an expanding economy. Generally speaking, he believes that the large number of firms, ease of entry, operation by owners, unchanging technology, and absence of price leadership produce an industry whose classically competitive structure adversely influences it in its quest for consumer preference.

In explanation of this situation the author describes the complex stages through which the raw pelts pass from fur farmer and trapper to final consumer. Special attention is given to an approximation of cost curves in establishments manufacturing fur garments, an analysis of the demand for furs by consumers, nonseasonal fluctuations in sales, and the competitive situation in

the fur industry in comparison with other industries in the United States today. Finally he makes a series of recommendations concerning the way in which a reform program aimed at achieving more concerted economic action by the industry could be financed and effected.

This book shares with industrial studies in general the problem that an immense amount of descriptive material must be presented to readers who are chiefly interested in seeing an analysis of the situation carried as far as possible. Not that the author has not used the analytical tools of the economist in dissecting his problem, in fact he has done so quite creditably. The fault is rather with the subject itself. Real markets are almost always clouded with institutional paraphernalia and this is particularly true here, for several levels and types of markets are included in one inquiry. Thus sales of pelts by farmers and trappers to or through cooperatives, auction companies, trading monopolies, brokers, and county collectors may be viewed as a single transactions level in which many alternative market forms exist. But this is only the first of two or more distinct levels through which the furs must pass before they are in the hands of the consumer. In a context such as this, each description opens so many vistas for exploration that the economist is continually frustrated by the necessity of moving on before the depths are plumbed. By electing to study in detail the industry's major problems of competition and fluctuations in activity, Fuchs has utilized his space effectively and achieved depth in at least two areas. This short book is a welcome addition to the growing list of industry studies.

LLOYD SAVILLE

Duke University

Industry and the State. By P. SARGANT FLORENCE. (London: Hutchinson's University Library. New York: Rinehart & Co., distributor. 1957. Pp. 196. $1.50.)

This small and stimulating account of the economic policies of the British government should prove useful to American economists as recommended supplementary reading in courses in economic history, labor, and government-business relations.

The claim of the author (emeritus professor and senior fellow in the Faculty of Commerce and Social Science at the University of Birmingham) that: "This book . . . is not just an analytical catalogue of State-industry contacts . . . [that] certain guiding ideas are introduced to connect and explain the facts described" is an accurate description of the book itself. The state is assumed to act "to bring industry into line with public opinion" and industry is defined to include mining, public utilities, construction and manufacturing, and exclude agriculture and the service industries.

The author begins with a very brief chapter on "Ethics, Science and Art of State Policy in Industry" in which he points out that any industrial policy adopted by the state has three fundamental aspects: (1) some ethical purpose, (2) some fact or knowledge of cause and effect, and (3) a law or order of the state. Thus a public policy prohibiting the exposure of meat in shops follows from the knowledge that flies may contaminate exposed food and the aim

of keeping disease to a minimum. The industrial aims of twentieth century British opinion are held to be "an aggregate increase, more equality and more stability of income." Specific public policies are later examined in the light of these objectives.

In Chapter 2, the author attempts to cover "Fourteen Decades of State Policy in Industry." In an interesting "Chronology of State Action" (pp. 20-22) he lists in parallel columns the main industrial legislation and the leading "contributory events" which usually consist of war, a Royal Commission study, or a treatise by an economist or philosopher. John Stuart Mill's five reasons for a "presumption" in favor of a minimum of government interference are reviewed along with his six "exceptions." The author concludes that *"laissez faire"* still appears the presumptive rule of British policy, exceptions and deviations from which must be justified for some specific reason," but that new values have gained strength "particularly democracy and greater equality," and that the new policy is best illustrated by the phrase *"laissez collective faire."* The remainder of the book develops this thesis by examining specific public policies.

Chapter 3 discusses the role of the state in protecting workers from employers, stockholders from management, and consumers from producers. Chapter 4 analyzes the ways in which the state has participated with these groups. In both chapters, the emphasis is on public policy in the field of labor. The main conclusion is that state interference in labor relations has been greatest in industries in which both employers and workers are least well organized. Thus industries employing a high percentage of women and organized predominantly in small plants have considerable state participation, and vice versa. With respect to industrial policies, the author notes without exploration, that "the plan of industrial 'self-government' by the people may end in a medieval stereotype of just prices, fair relativities and fixed relationships, and even a hierarchy of classes."

Chapter 5 is devoted to industrial services rendered by the state, such as statistical information and employment agencies, and to the various forms of social security.

The final two chapters tackle the problems of state operation and state control, of industry. Under the public-corporation form of nationalization, the author finds that industrial relations have become "less personal and human," that working for the benefit of the community rather than for another man's profit has neither improved the workers' attitude nor reduced the number of strikes, that consumers have experienced repeated price increases with little chance of knowing whether the increases are justified on economic grounds, that the industrial officials are "quite as unwilling to have their work independently investigated and ventilated as most private businesses," and that a "further interest has to be represented in nationalized undertakings—the nation as a whole and as a continuing entity."

These statements should not be interpreted, however, to mean that the author is opposed to state interference. Indeed, most of his analysis and proposals suggest the wisdom of more rather than less state control. The philosophy throughout the book seems to be one of protecting each group's interests,

through equalizing and improving the processes of bargaining rather than strengthening competition.

The author is very much at home throughout the book. Yet he who delights in locating weak spots will not be disappointed; the book offers something for everyone. If, for example, one wishes to be critical, he might turn to page 141, where on a single page, coal is held to be an "increasing cost" industry because costs increase as the more fertile veins are exhausted, electricity and gas are held to be in "joint demand," and most significant of all, an increased consumption in response to lower prices which reflect lower costs is held to be "wasteful" because "of the very cheapness of the product," and "increased production might be developed at the expense of other products." Clearly, putting resources to uses in which price covers cost—cost which reflects all competing demands for the resources in question—is not using them uneconomically. Perhaps the fault is one of expression. Compression can introduce a lack of clarity.

For a compact book crammed full of facts, however, the author has done a remarkably lucid bit of writing. It is stimulating and informative, and requires little by way of background for easy understanding of its contents.

FLOYD A. BOND

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Land Economics; Agricultural Economics; Economic Geography; Housing

Farm Trouble. By LAUREN SOTH. (Princeton: Princeton University Press. 1957. Pp. vii, 221. $3.75.)

Seldom is a major national problem presented so competently, lucidly and honestly as in this small book on the farm problem. This does not mean that the book is noncontroversial nor, for that matter, that it is free of biases. Few of its readers will agree with all of it, but still fewer will fail to profit from reading it. Its purpose obviously is not that of presenting a meticulously accurate picture. It is rather to suggest and stimulate discussion of new and possibly more realistic approaches to the problems under discussion. However, these are not advanced without solid foundations of first-hand study and observation. The author, a former member of the agricultural economics staff of Iowa State College, has also served as editor of the U. S. Department of Agriculture's *Agricultural Situation* and is now an editor of the *Des Moines Register and Tribune*. He has recently been made chairman of the Agriculture Committee of the National Planning Association and was a Pulitzer Prize winner in 1955.

The opening chapters, entitled "The Continuing Debate" and "The Shibboleths," give an excellent and very readable statement of the situation. The chapter on shibboleths is especially refreshing in its frank dismissal of widely accepted and politically popular slogans, concepts and whipping boys. So also are Chapters 4 and 5, entitled "All Kinds of Farms and Farmers" and "Farmers in an Industrial Economy." Chapter 3, "A Sick Industry," is less convincing. One is hard put to accept easily the designation "sick" for an in-

dustry that has just been passing through the period of greatest prosperity in all its history, one in which output has risen by 40 per cent in the past 20 years, where output per man-hour is up by more than 100 per cent and in which land prices have risen by well over 100 per cent since 1940. Perhaps a better title would have been "An Ill-Adjusted Industry."

The author concedes (p. 43) that "In the last quarter century or so, American farmers have vastly improved their relative income status. American farmers are 'better off' in relation to urban dwellers, perhaps, than farmers of any other country at any time in history." He also states (p. 60) that ". . . the top 2 million farm families have incomes that look fairly good by urban standards," $5,600 in net money income in 1953 as compared with an average net money income of about $5,000 for nonfarm families. Clearly, the two ways of life are not strictly comparable and as yet we have no way of measuring the intangible forms of income involved except through observation of the behavior of people in making their choices between them.

This leads into the chapter on "The Neglected One Third," that is, the low-income farm people. Here the discussion leaves this reviewer less than satisfied. There are inconsistencies in it and the case seems overstated. Granted that this is a large and important problem, is there not a danger of assuming too easily that all or most of these people could become successful farm entrepreneurs or urban wage workers or would do so if they could? Does everyone want to adjust to the driving pace of present-day urban life and does the national interest require that we strive unremittingly for maximum physical production? Despite these reservations, it must be recognized that this group constitutes one of the important problems in agriculture.

Later chapters deal with "The Surplus Problem," "Price Supports," "Keeping Farm Income Up," "The Soil" and various other matters, always in a stimulating and thought-provoking way but certainly not in ways that all readers will agree with. With much of it this writer is in agreement, but it is difficult to escape a feeling that both the difficulties of administration and the costs of the programs suggested are minimized. These later sections warrant fuller discussion than is feasible in the space here available. One can only say that, as a whole, here is a book that many people should read and think about; It doesn't matter too much whether they agree with all of the conclusions that are stated or implied.

MURRAY R. BENEDICT

University of California

Agricultural Policy and Trade Liberalization in the United States, 1934-1956. By ALLAN RAU. Études d'Histoire Économique, Politique et Sociale No. XXI. (Geneva: E. Droz. Paris: Minard. 1957. Pp. 160.)

It is no news that U.S. agricultural policy conflicts with our ostensibly liberal trade policy. However, Rau believes (p. 20) there is scope and need for an up-to-date work on this conflict, as much has happened since D. G. Johnson's *Trade and Agriculture* appeared in 1950. It is questionable whether this book meets the need. The first two-thirds of it contains mainly a narrative of the often-told history of agricultural legislation down to 1950. Rau has crowded

into the remaining fifty pages a review of farm legislation subsequent to 1950, illustrations of the agriculture-trade conflict taken from the debates over the ITO Charter and in various sessions of GATT, and a fourteen-page summary of alternative farm policies (direct income payments, forward prices, stimulants to mobility, etc.). Finally, the author concludes that (a) to resolve the conflict, agriculture must give way to a liberal commercial policy, and (b) the only realistic attack is to convince Congressmen and their farm constituents that expanded foreign trade is in both the nation's and the farmers' own general interest.

Rau's book contains none of the economic analysis which distinguished Johnson's work. It presents no new empirical material; the only statistical table (p. 149) is taken from the Randall Commission *Staff Papers*, and the only chart (p. 150) is from the C.E.D.'s *Economic Policy for American Agriculture*. Throughout the text, references are almost entirely confined to secondary sources. These, however, Rau has sifted thoroughly. He drags out the narrative of farm legislation to tedious and unnecessary length in view of the wealth of available material on the topic. He does not suggest any new alternative policies, and gives the alternatives surveyed a very summary treatment. Apparently Rau favors direct income payments, but feels political pressure will prevent this; he is noncommittal on forward pricing, but approves of measures to expedite the movement of surplus labor, to reduce marginal acreage, and to increase domestic demand.

The trade policy to which agricultural policy is to be adapted is the simple one of no quotas, export subsidies, or protective tariffs. The book contains no analysis—indeed, no mention—of the vices and virtues of international commodity agreements, buffer stocks, commodity reserves, or other devices designed to stabilize international agricultural markets. Nor does it examine Gunnar Myrdal's provocative thesis in *An International Economy* that farm policy is an example of national integration triumphant over international integration, with the ultimate solution to be found only in gaining international solidarity.

Agricultural Policy and Trade Liberalization is then a summary of the literature and legislation on the topic, but not a contribution to the former. For the nonspecialist it would serve as an introduction, but even in this role it suffers from excessive emphasis on the details of legislation. The bibliography is helpful.

<div style="text-align: right">CHARLES E. STALEY</div>

University of Kansas

A Concept of Agribusiness. By JOHN H. DAVIS and RAY A. GOLDBERG. (Boston: Division of Research, Graduate School of Business Administration, Harvard University. 1957. Pp. xiv, 136. $6.00.)

It is so common to think of agriculture and business as distinct segments of the national economy that an effort is required to bring these so-called segments together in a conceptual framework which emphasizes their mutually supporting relationships. Believing that misguided action results from the segmented way of thinking, the authors of this study suggest a new word to

describe the interrelated functions of agriculture and business—the term *agribusiness*. By agribusiness they mean "the sum total of all operations involved in the manufacture and distribution of farm supplies; production operations on the farm; and the storage, processing, and distribution of farm commodities and items made from them."

Agribusiness owes its origin chiefly to the technological revolution of the eighteenth century. The application of labor-saving machinery and scientific knowledge raised productivity levels to such a degree that a declining proportion of the population was able to supply food and fiber to a growing population. Moreover, a companion revolution occurred off the farm as functions hitherto considered part of normal farm routine were assumed by outside agencies.

Though changes of this order have many benefits to recommend them, it can hardly be said that the transition from agriculture to agribusiness has been one of uninterrupted progress. On the contrary, the authors say that our food and fiber economy presents difficult problems which are "rooted in imbalances brought about by spotty progress on an uneven front."

In a pioneer study of this nature it is important to determine the over-all magnitude, dimensions, and composition of agribusiness and its component parts. This the authors accomplish by means of a variety of statistical measurements. They find that agribusiness is a major component of our economy, one that comprises between 35 and 50 per cent of the national total, depending on the type of yardstick one employs.

The largest section of the study (48 pages) is devoted to an analysis of the inner working of the food and fiber sector and its relation to other sectors of the economy. Here the authors employ the Leontief input-output method in terms of 1947 data. By means of tables, charts, and accompanying textual analysis, the reader may trace the flow of goods and services among and within industries, particularly those comprising the agribusiness complex.

In a final chapter the authors consider some of the policy implications of agribusiness. They call for a reappraisal of existing policy and the formulation of new policy in terms of a comprehensive frame of reference. For agribusiness this means both a vertical and a horizontal approach—vertical in terms of production, processing, and distribution on a commodity basis; and horizontal with respect to policies concerning other commodities, the national economy, and our international responsibilities. Probably the most controversial policy recommendation is that responsibility for agribusiness be shifted from government to the private sector of the economy "as rapidly as possible and to the degree possible." Instead of centering in government, the policy-making function would be assumed by spokesmen of the respective segments of agribusiness, assisted by researchers and technicians. Some conflict is anticipated with respect to existing antitrust laws and regulations, and the authors suggest modification of the legal system so that agribusiness may be placed on an economic basis comparable to that of business generally.

Since the Harvard study is largely exploratory in nature, it is difficult to assess its merits and demerits. Certainly, research that adds to understanding of complex agribusiness relationships will be welcomed by scholars and policy-

makers. In this respect the use of input-output analysis would appear to be a definite contribution. On the other hand, some readers may be disturbed by certain policy recommendations. While the authors call for the use of democratic procedures and a consideration of national economic goals in formulating agribusiness policy, little attention is given to the existing power structure and the subtle ways of exerting influence for particularistic ends. What, for example, is to prevent one or more private groups from dominating agribusiness in perhaps a more fundamental way than government agricultural policy is now influenced by certain pressure groups? The experience of the National Recovery Administration, which to the reviewer had features quite similar to the proposals under consideration, is a case in point. Grant McConnell's *The Decline of Agrarian Democracy* (Berkeley and Los Angeles, 1953) is also instructive concerning the *realpolitik* of business and agricultural collaboration. Instead of finding the cause of imbalance in the agricultural sector, it might be pertinent to inquire if much of the difficulty is not traceable to the highly organized, integrated, and more or less monopolized urban industries.

RICHARD B. SHERIDAN

University of Kansas

Labor Economics

The Demand and Supply of Scientific Personnel. By DAVID M. BLANK and GEORGE J. STIGLER. (New York: National Bureau of Economic Research. 1957. Pp. xix, 200. $4.00.)

At the time of its publication in the summer of 1957, one might have sympathized with the authors of this book for having missed by a few months a peak in the interest of a large nonprofessional audience which had so recently been preoccupied with the "shortage" of scientists and engineers. It would now seem, however, that they need no sympathy on this score since their subject has since experienced a major "sputnik effect" and promises to continue to be the subject of much public discussion.

The study reported on in this book was undertaken by the National Bureau of Economic Research for the National Science Foundation. This is one case where the authors' disavowal of any responsibility on the part of the sponsor for views expressed in the book has more than formal meaning, since many of the authors' conclusions seem at variance with public statements of those high in the councils of the Foundation.

Although engineers, natural scientists, and mathematicians represent today only a little more than one per cent of the labor force in the United States, no one will question the special—some would say crucial—importance of this group of professions. In the eight decades since 1870, the number of engineers and chemists grew 17 times as fast as the labor force; moreover, there has been no significant retardation in this rate of growth in recent years. The central purpose of this book is to explain, as far as is possible on the basis of available data, the forces which have shaped the long-run demand and supply for the services of this technically trained manpower—particularly engineers.

On the demand side, Blank and Stigler call particular attention to the spec-

tacular growth of expenditures on research and development since 1940. By. 1930, expenditures from all sources are estimated to have risen to only $166 million; by 1952 they were about $3,750 million; and today they are probably in the neighborhood of $5 billion. Despite the growing importance of government-financed research, the authors find that only a small part of the total increase in demand since 1940 can be accounted for by this factor. Their statistical analyses give somewhat conflicting results as to the extent to which government research funds have had a substitution effect and to what extent a "pump-priming" effect on private research outlays. It is quite possible that in the short run the substitution effect has been important, while over time government research and development expenditures have had a significant pump-priming effect.

In the authors' view, the single most important factor explaining the secular increase in demand has been the shifting pattern of industrial composition. About 40 per cent of the increase in demand can be explained by the fact that industries which are relatively heavy consumers of the services of engineers have grown more rapidly than the others. They suggest two other factors which would explain most of the rest of the increase, though they are unable to estimate the precise importance of either. First, there has for years been a secular decline in the relative salaries of engineers, and this has led to a substitution of technically trained manpower for other inputs. The authors are much less certain with regard to the other factor, the effect on demand from technological progress which, in many industries at least, has resulted in increasingly complex productive techniques. As they rightly point out, this trend does not necessarily lead to an increased demand for technical manpower; among other things, the associated increases in output per unit of engineering input are often very large. With regard to industrial research, it is difficult as yet to say very much. Certainly, the forces determing demand for manpower in this activity are complex, and our experience has beeen so short with this as a major activity that we have little data available permitting of systematic analysis.

On the supply side, Blank and Stigler do hazard some forecasts; but, as they recognize, there are serious difficulties. For one thing, although there seems no question that college enrollments will undergo a dramatic increase in the next decade or two, it is very difficult to estimate the proportion of those in college who will specialize in the sciences and engineering. This proportion has been slightly less than 10 per cent for most of the period since 1930, save for the five years immediately following the second world war when it rose above 11 per cent. Although Blank and Stigler show that engineering salaries have fallen throughout this period relative to the general level of incomes in the labor force and to incomes in the independent professions (*e.g.,* law, medicine), they argue that this is not directly significant. Instead, what matters is the relative attractiveness of the occupations most entering college students consider as alternatives to engineering (*e.g.,* business administration). In the period since 1950, for which data are available, starting salaries for engineers have been higher than those in these other "close substitute" fields, and "such a premium is likely to exercise a continuing incentive for students

to shift towards engineering" (p. 76). The authors present a projection of the number of college graduates who will receive engineering degrees up to 1970 based on several alternative assumptions. Even their most conservative estimate shows a substantial increase: engineering degrees averaged under 28,000 per year in the years from 1950-55; Blank and Stigler estimate a possible 35,000 by 1960 and 60,000 by 1970; on less conservative assumptions, the last figure jumps to 90,000 a year.

In their discussion of supply, the authors surprisingly fail to consider the possibility that there may be any limitation (even in the short run) on the capacity of colleges and universities in such fields as engineering and the sciences. They assume implicitly that student choice will be completely free of any such restraint. Moreover, they ignore the possibility, so much emphasized by others in recent years, that many high school programs are so deficient that many students entering college do not have the background, in such fields as mathematics, which is required in the sciences and engineering, so that in effect they do not have the option of selecting these as their major subject. Nor is consideration given to the possibility that some students may be unable to pursue their education as far as they otherwise might because of inability to get financial support.

Despite the fact that engineering salaries have declined secularly relative to those received in many other occupations, an engineering education continues to be a worthwhile investment (*i.e.*, the present value of the average engineer's lifetime income exceeds that of a high school graduate going directly into the labor force). Blank and Stigler make no attempt to explain the persistence of this relatively high rate of return.

The book concludes with a brief chapter on the supply and demand for mathematicians and physicists. Attention in this chapter is directed solely to the university demand for these two groups. It seems a mistake today to ignore, as the authors do, the increasing importance of industrial demand, particularly in research, for these talents.

Although the chief focus is on the long-run determinants of supply and demand, the section of their work which will be most avidly read and discussed is that dealing with the "shortage" of recent years. And there are already indications (in reviews in scientific and engineering journals) that the noneconomist is missing one of the most useful points made in the few pages devoted to the subject. Blank and Stigler conclude that, while there was a minor break in the relatively declining trend of engineering salaries (compared to other segments of the labor force) during the Korean episode, there is no evidence that all the hue and cry over the shortage has been justified. The point, of course, is that the authors are applying an economist's —a market—definition of shortage: "A shortage exists when the number of workers available (the supply) increases less rapidly than the number demanded *at salaries paid in the recent past*" (p. 24, italics theirs). To test the existence of shortage so defined, the authors then look to the movement of relative salaries. But many of those who have been most concerned about a "shortage" have used the term in a very different sense: what they are really saying is that, in their opinion, the demand (in a market sense) and

therefore the supply, should be greater than they are; for example, because of our technological race with the Soviet Union. In other words, they are asserting a value judgment with regard to the number of scientists and engineers we *ought* to want and to have.

While Blank and Stigler should be applauded for attempting to introduce an objective definition of "shortage," there may be some question regarding the particular definition they adopt and the interpretation of the available evidence used in its application. The definition, as given, is not complete since no definition of "recent past" is presented. Also, the usefulness of this definition may be questioned, since a market will experience a "shortage" whenever price rises (presumably relative to other prices) thus making a shortage an almost ubiquitous occurrence. In fact, the authors seem to be saying the following: except where there is price control (official, or established through private monopoly power) there can never really be a shortage (in a *strictly* economic sense) since in a free market supply and demand will always be equalized by price; however, since everyone is so excited about shortages of scientists and engineers, we'll call an increase in relative prices a shortage, but on this definition we can show there really hasn't been one worth speaking of, anyway. Now Blank and Stigler may very well be right that the rise in relative price is the explanation of the complaints that a shortage existed: none of us likes to be suddenly confronted with a higher price for something we purchase regularly. But if, in their words, a market is "technically a good market," which responds without any restraints to changes in supply and/or demand, then I don't see why the term "shortage" (which does have a well-understood meaning in the price-control case) need be used at all, when we can simply describe the phenomenon as "increase in relative price."

Blank and Stigler base their conclusion that there has not been a significant shortage on the fact that salaries have not increased much more for this type of personnel than for many other categories of employees. But, in the case of all the occupations (*i.e.*, accountants, salesmen, etc.) for which data are presented, starting salaries have risen in the period from 1950 to 1956 by nearly 50 per cent, an increase considerably larger than the rise in wage earners' incomes (see Table 14, p. 28). Thus, on their definition, should not the authors have concluded that there has been a shortage—and probably of significant proportions compared to other periods in our history—in *all* of these "white collar" occupations? Moreover, on their own definition it is not clear how they are able to assert that the shortage "was hardly more than a cross-current in a tide" (p. 28). How much of a price rise in any given market is necessary to establish a "serious" shortage of the Blank-Stigler variety? The authors do not tell us.

They recognize that their test of "shortage" requires a free market; *i.e.*, prices must be free to respond to a change in supply or demand. Finding no obvious impediment to the freedom of the market for scientific and engineering talent, they conclude that there is no problem on this score. While one may agree with their conclusion that there are no obvious and important imperfections in the markets in which these services are exchanged, they do not consider the possibility that the character of the "product," the manner

in which contracts are made (and broken), the "customs and habits of the market"—in short, the total institutional framework—produce a market which is very sluggish in its response to changed conditions such as the large and rapid increase in demand of the past few years, thus creating a temporary situation analogous in many respects to the price-control type of shortage.

Some readers will be disappointed in this book because they will come to it hoping for a specific prediction of supply and demand for scientific personnel or at least a complete model useful in making such predictions. But, given the very meager data available, and the inherent difficulty in any market of segregating demand and supply factors when the analysis is based primarily on historical time series, I doubt that anyone could have drawn more definitive conclusions with regard to the issues and factors the authors have selected to consider. We may be disappointed that the National Science Foundation did not provide the National Bureau with the funds necessary to fill some of the important data gaps—certainly, as the authors repeatedly remind us, much more should be done along these lines.

This is a particularly welcome book at this time—despite any minor reservations and disappointments we may have—because of the amount of confusion and nonsense in the current discussion by engineers and natural scientists of the economics of their own professions. At a minimum, it does not seem unreasonable for the economist to ask the noneconomist to use our most honored terms "supply" and "demand" in their traditional meanings. Since Blank and Stigler have written with their noneconomist audience in mind, we now have a reference which shows how to apply these terms to the market for scientific personnel.

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The Theory of Wage Determination. Proceedings of a Conference held by the International Economic Association. Edited by JOHN T. DUNLOP. (New York: St. Martin's Press. London: Macmillan. 1957. Pp. xv, 437. $7.50.)

This highly interesting collection of 22 essays is the outcome of a ten-day conference of 35 economists brought together by the International Economic Association at Seelisberg, Switzerland, in September 1954. The purpose was to consider "the" theory of wage determination. As with the Association's parent, the United Nations itself, the result if not the intent reveals more diversity than unity. Since science advances with frank interchanges of views, this is not an objection to the effort. Indeed, if any legitimate protest can be made against ventures of this kind it would be the quite frivolous one that it is left to the critic to find the unity and to evaluate the diversity, a task that bears some similarity to an assignment to review a new edition of *Britannica*.

John Dunlop's introductory essay provides a sagacious and recondite review of the history of wage theory, followed by reference to leading unsettled questions. He rightly contends that the weak points of the marginal productivity analysis involve the supply side, in particular its inability to get beyond

a single rate and to explain the determination of particular wage structures, specially for administrative units embraced by collective bargaining. He wisely avoids the false dichotomy between "political" and "economic" wage theories, yet fully recognizes the causal role of decision-making for particular wage structures. By means of concepts of key rates, job clusters, and wage contours, he breaks new ground, bringing together the joint influence of market forces on the one side and matters of equity, bargaining, and administration on the other.

The next group of six essays deals with the general level of wages. They add little that is new to the existing literature, and their main concern is with the stability of full employment. Harry Johnson finds that neither the classical nor the Keynesian approach explicitly explains determination of the money wage level. Leaning towards a cost-push theory of inflation, he notes correctly that money supply now depends upon government policy, although money only becomes residually determined under an unlimited commitment to full employment. I. Gasparini follows with a purely formal analysis of alternative wage-level theories. Phelps Brown attempts to separate distributional from productivity influences upon the course of real wages in the United Kingdom, reiterating his earlier finding that unions have only cut into profits when sellers' price "conventions" collapse from inability to pass on higher costs to consumers—an exceptional rather than typical situation. Bent Hansen develops a valuable but purely formal dynamic analysis of wage-level changes, based upon the weighted over-all impact of gaps in labor supply or demand in submarkets. This leads him to the highly interesting conclusion that stability of the wage level depends in no simple or direct way upon the rate or volume of general unemployment. V. F. Wagner also considers the stability question, arguing unconvincingly that a deliberate policy of full employment must include direct wage and price controls to meet an inevitable inflation problem. W. Krelle closes with a model showing how the levels of real and money wages are determined.

Part III takes up the impact of the union, indicating perhaps inadvertently that the answer depends upon the theoretical framework and national environment of the particular author. Reviewing the British experience since 1938, B. C. Roberts concludes that while union policies have exerted some influence on wage behavior, the economic state of each industry and of the whole economy have been primary, even helping to shape union policies themselves. By contrast, H. A. Turner reaffirms his primarily "political" theory: British wage behavior since 1940 can be explained by the attempt to maintain or restore "established relativities" and by the preference of expanding unions for flat-rate general increases. Hubert Brochier shifts attention to the question of a theoretical model of the union, rejecting both the political and purely economic types now extant and urging an approach that would unite a genetic with a functional analysis. Jean Marchal moves in a similar direction, contending from the French experience that wage-setting now reflects the acts and ambitions of organized national groups. Wage theory can no longer be autonomous but must be consolidated with distributive analysis conducted on a group level.

Part IV concerns six essays on wage structure, where again market forces and bargaining forces are linked together. According to Clark Kerr, unions have their greatest impact upon plant differentials and interplant relationships within common factor and product markets. Beyond these categories, union incentive for uniformity declines while the power requirements rapidly increase. So far as occupational, interindustry, and interregional differentials are concerned, both Kerr and Reynolds strongly emphasize economic forces while crediting unionism with a selective impact upon interregional wage relationships. Viewing the Swedish experience, Gostä Rehn finds that the employers formally advocate wage flexibility while the unions urge rigidity on equity grounds, although in full employment flexibility would raise labor's share at the expense of profits while rigidity would work the other way around. Accordingly, neither group follows its doctrines in practice. The spread of unionism, Rehn thinks, has checked inflation and has not increased wage distortion relative to the productivity standard. Interpreting the French case in quite contrary terms, François Perroux stresses the damaging effects of wage rigidity. France suffers from structural inflation because it is a directed economy in which strategic groups have successfully divorced their incomes from close correspondence with relative productive contributions, by immuring themselves in a haphazard system of protected positions whose joint effects are chronic inflation and built-in economic inefficiency. F. Sellier and G. Rottier complete the section with studies of the evolution of wage differentials in France and England respectively.

The remaining parts of the book include two essays on bargaining theory, in which K. W. Rothschild contributes a competent review of wage theory and calls for a broad sociological approach, and G. L. S. Shackle evaluates recent work in this field. C. A. Myers then considers the American labor mobility studies, finding that in the main for 1940-1950 the labor market showed remarkable flexibility, effecting broad shifts that correspond well with the processes encompassed by the neoclassical model. Giovanni Demaria contributes a generalized analysis of the determinants of labor supply, contending that the aggregate supply is zero-elastic relative to money wages, while Douglas' reverse curve describes particular markets. The book closes with a highly interesting transcript of the discussions, edited by Douglas Hague. The reader can gain much by comparing the papers seriatim with this latter very informative record.

At this point the reviewer must attempt the impossible task of indicating the extent of unity and of diversity in wage theory suggested by these 22 contributions. On the side of unity, (1) the monetary theorists indicate that the money wage level still functions as an exogenous variable, not yet successfully incorporated in a more general aggregative analysis. (2) Full employment continues to pose a problem of price-wage stability, and there is no practical criterion to guide full employment policy with surety along non-inflationary lines. (3) The real meaning of an inflationary cost-push is a decision in favor of an elastic supply of money, to prevent massive unemployment. (4) Unions make a difference for wage level and wage structure, and their influence is greatest at the plant and locality levels. (5) Neither the

unions nor managements operate in a discretionary vacuum, although they do have some discretion: supply and demand forces do provide incentives and constraints, which increase in influence as the zone of the analysis widens. (6) Neither a purely "political" nor purely "economic" model of the union or of wage determination is adequate as an explanatory device. (7) For gross differences in wages—intertemporal, interregional, interindustrial, and international—neoclassical wage theory is still a very powerful tool, indeed the only one we have.

On the side of diversity, (1) there is conflict over the respective roles of economic and of institutional forces in wage behavior. (2) There is a deep methodological division between those who formulate purely analytical theories, leaving the question there, and those who insist that the hypotheses suggested by such theories be put to empirical test. (3) Regarding the implications of union power and performance for economic welfare, there is a clash between those who see rigidity, distortion, inflation, and economic inefficiency as the dolorous outcome, and those who do not.

These suggestions do not exhaust the list and they can be disputed. However, one conclusion can hardly be assailed: this book affords a valuable inventory of the present state of labor economics. As such it deserves a careful reading.

GEORGE H. HILDEBRAND

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Theoretische Grundlagen der Lohnpolitik. By ERICH ARNDT. Veröffentlichungen der Akademie für Gemeinwirtschaft, Hamburg. (Tübingen: J. C. B. Mohr [Paul Siebeck]. 1957. Pp. xi, 295. DM 24,60; paper, DM 21,00.)

The author, *Privatdozent* at the University of Hamburg, has set himself a difficult assignment, namely that of presenting a systematic discussion of wage policy. In order to do so he was of course compelled to state the theoretical foundations of wage policy, and to discuss current wage theory as well. Since he proceeds from wage policy to theory, the discussion must seem at times quite unnecessarily heavy and occasionally repetitious to a student. At an advanced level, these shortcomings are minor and the considerable merits of the book as a competent statement of modern wage theory will appear more significant.

The introductory chapter deals with "the social conflict in the wage relationship and its elimination." Its thesis is that the wage conflict is only one of many tensions in the union-management or worker-employer relationship. By solving or reducing these other tensions we could free the inevitable wage conflict from these additional sources of strain. In this context the author makes many intelligent observations about the place of unions in a modern industrial system, particularly in his criticisms of the position of Röpke and his associates, but on the whole the American reader will find this chapter too brief to be more than an outline of some of the most intricate problems of industrial sociology and psychology.

The main body of the book consists of a chapter on the "need, objectives

and methods of wage policy" followed by three chapters devoted to the
micro- and macroeconomic effects of wage-setting—which turn out to coincide
with the effects of wage increases on the firm and on the economy. The need
for a wage policy is said to emerge from the fact that the market mechanism
does not function on the labor market in the same way as on the product
market. It does not always tend to produce a short- or long-run equilibrium.
Nor will the market prevent threats to the existence of workers or ensure the
proper sharing of increases in productivity by the labor force. Wage-setting
is therefore necessary. By this the author apparently means any wage de-
termination leading to a wage rate different from the one the market would
have arrived at in the absence of intervention. Such intervention may be by
the state, associations of workers and employers, or by the plant. Collective
bargaining thus appears as one of the forms of wage policy as do incentive
wages, minimum wage laws, etc. In other words, a good deal of the area
of labor economics as taught in this country comes under the heading of
wage policy as understood by the author. It is not quite clear what the
theoretical model is with which he contrasts his concept of wage policy,
except by implication from the fact that wage policy seems to lead consistently
to wage rates above the (competitive or monopsonistic?) equilibrium wage.

The main elements of wage theory are then presented in the guise of an
elaborate discussion of the consequences of "wage policy," *i.e.*, of wage rates
above equilibrium. The three chapters of this part of the volume deal suc-
cessively with the wage as an element in the costs of the individual firm, the
wage as income in the individual household and, finally, the macroeconomic
effects of wage increases upon employment and price levels. In doing this,
the author presents a detailed picture of conventional modern wage theory,
i.e., of marginal productivity theory combined with the theory of imperfect
competition, Keynesian employment theory, multiplier, acceleration principle,
etc. These tools of analysis are handled with admirable thoroughness. Indeed,
one has the impression that the author has squeezed these theoretical con-
structs to the last drop and perhaps pushed on even into the stage of negative
marginal returns. He betrays considerable familiarity with American litera-
ture and a high degree of sophistication in his handling of theoretical tools.

However, this reviewer would have greatly preferred to see the author's
considerable analytical gifts applied to more sophisticated questions as well
as to the choice of more realistic assumptions. Thus, though the author is
familiar with the Lester-Machlup controversy on the validity of the marginal
productivity theory in the field of wages and with most of the work of the
leading American authors in the area, and expresses some doubts himself,
he handles the theory as if no uncertainty existed as to the meaning of the
concept of "marginal value product." The book would have greatly profited
from a thorough exploration of the issue of estimated or expected value product
in the functioning of the theory, the danger of its turning into a tautology,
the limits of the application of marginal productivity theory to the economy
as a whole, etc. The discussion of the possible effects of wage increases upon
productivity would gain considerably from references to empirical material
in that area. The problems of price stability in an era of full employment

are stated very well indeed, but within the narrow framework of his own assumptions the author has little to offer in the way of a new approach.

Generally speaking, the author seems to overestimate the insights that accepted theory offers and to rely on the results of deductive reasoning without much reference to empirical material. One has the sense in reading the book that there are few open issues left in an area of economics which most authors regard as particularly unsettled. Yet, there is considerable merit in the author's concern with a full presentation of prevailing theory and great interest in some of his more practical conclusions. He has some very interesting comments on cost-of-living escalator arrangements for wage rates, the decline of the role of the strike in (German) labor relations and—characteristically—the need to substitute "scientific" guidance for our antiquated methods of settling wage conflicts.

All in all, the book is useful and in parts excellent. Its main disadvantage is that by creating the appearance that a well-established body of knowledge exists in the field of wage theory it may discourage rather than call forth further research.

ADOLF STURMTHAL

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New Concepts in Wage Determination. Edited by GEORGE W. TAYLOR and FRANK G. PIERSON. (New York: McGraw-Hill. 1957. Pp. xiii, 336. $6.50.)

Students of labor economics will find this symposium on wage theory and structure a valuable addition to the literature on the subject. Twelve recognized experts in the field, including four past presidents of the Industrial and Labor Research Association, have collaborated to present this lucid and frequently brilliant summary of current wage theory and wage structures.

The contributors do not attempt to construct a comprehensive wage theory. Indeed the authors seem to question the feasibility and desirability of formulating such a theory. As a result the volume concentrates on the explanation of a variety of subjects dealing with wage structure and wage movement. But the authors have avoided the pitfall of presenting disjointed discussions on related topics which is so common to this type of symposium. The twelve authors met over a period of three years while the volume was being written. The result is a well-integrated and excellent volume on the various aspects of wage determinations—collective bargaining, interplant and intraplant wage structures, wage differentials and wage movements.

In their search for principles of wage determination the authors have placed the greatest emphasis on empirical evidence including day-to-day experience and have avoided excessive reliance upon deductive reasoning, the tried and true handmaiden of traditional wage theory. The latter approach was tried recently in a similar symposium (*Impact of the Union,* [New York, 1951], edited by D. M. Wright) by several prominent general economists covering much of the same ground as the present volume. These economists applied the tools of their trade without the benefit of empirical evidence to the examination of wage theory and the economic impact of unions. The

result was an arid discussion, related little to actual conditions. In contrast the present volume is realistic, meaningful and establishes a sound framework for the analysis of wage structure.

The 11 essays in the volume are divided into 3 sections. The volume opens with an historical review of wage theory by Frank G. Pierson. He finds that received wage theory offers little help for analyzing wage relations, nor does it add much to the understanding of contemporary wage issues, though he finds marginal analysis helpful to explain long-run wage movements. He appears to imply that the tools available to the economist are insufficient for the understanding of wage determination, but he expresses faith that recent studies will lead to a better understanding of wage behavior and urges further empirical work to achieve this goal. A safe conclusion, for one cannot lose caste by suggesting further study. It would have been helpful if Pierson had pin-pointed the areas where additional research might be more fruitful.

The next three chapters constitute a tripartite discussion of the role collective bargaining plays in wage determination. Leland Hazard concentrates his discussion on collective bargaining in large business units. While he grudgingly admits that the purpose of business is to make profits, yet he asserts that management in large corporations is more concerned with maintaining production and is, therefore, ready to make concessions to unions to win uninterrupted production. Apparently, according to Hazard, large corporations are little concerned about profits. He would have us believe that as long as production continues profits come naturally to corporations. The union spokesmen, Nathaniel Goldfinger and Everett Kassalow, appear to be as apologetic about unions asking for more as Hazard is reluctant to talk about profits. The burden of their arguments is to prove that unions have a long-run impact upon wage determination and that unions have had a salutary effect upon total economic development. They state their case skillfully and persuasively. George W. Taylor presents the public view on the role of collective bargaining. He stresses the need for consent in achieving wage determination and concludes that a theory of collective bargaining is essential for a realistic theory of wages under present American conditions.

The second part of the symposium consists of 4 papers dealing with structural characteristics of wages, and their interrelationship within the firm, industry and nation. John T. Dunlop suggests that a wage theory must take account of intraplant as well as interplant wage relationships and also of the role of key rates in determining wage structures. The Dunlop paper, though brilliant as far as it goes, presents a static analysis of the anatomy of wage structure. He stops short of explaining the impact of total economic activity on wage structure. The other three contributors to this section—E. Robert Livernash, Arthur M. Ross and Richard A. Lester—deal respectively with intraplant, interplant and regional wage relationships and differentials. Their discussion is largely descriptive of present-day wage structure in the American economy.

The final part of the book deals with general wage movements at the national level. Lloyd G. Reynolds questions the desirability or feasibility of a general theory of wages in our economy where wage determination is highly

fragmentized. Of timely interest is his unverified assertion that unions have only a minor influence in raising prices and money wage levels. Clark Kerr asserts that unions have not had a significant effect on income distribution. Though his conclusion may be correct, the voluminous statistics that he uses to buttress his thesis are of questionable pertinence and significance. But Kerr is apparently aware of the shortcomings of his evidence since he tempers his conclusions by a reminder of Samuel Butler's observation that "life is the art of drawing sufficient conclusions from insufficient premises." The volume closes with a keen and discerning analysis by Melvin Rothbaum of wage movements in the United States, France and Italy between 1938 and 1952 and the major factors that have accounted for the general wage movements in these countries during the 14-year period.

The volume offers little that is new in wage concepts. This is largely due to the prominence of the contributors, who have themselves pioneered in the respective areas which they summarize in the symposium, and to the general acceptance of their original contributions. The real value of *New Concepts in Wage Determination* lies in its presentation of a comprehensive framework for the analysis of wage structure in the United States. The study fails, however, to throw much light on the relation of wage levels and wage movements to macroeconomic analysis.

Throughout the volume several of the contributors comment on the fact that economic theory is a product of the economic developments of a given time and place. *New Concepts in Wage Determination* seems to support this proposition. It was written in a period of relatively high employment, stable prices and peaceful industrial relations. The volume reflects these conditions. Apparently satisfied with existing conditions, the authors show little concern for public policy implications of their findings. Some readers may find the study wanting due to this cavalier disregard of the broader implications of wage movements and structure. But in a period of prosperity and contentment there seems to be little inclination to worry about these disconcerting and disturbing problems.

SAR A. LEVITAN

Legislative Reference Service
Library of Congress

History of Employers' Associations in the United States. By CLARENCE E. BONNETT. (New York: Vantage Press. 1956. Pp. ix, 573. $5.00.)

Bonnett's earlier work, *Employers' Associations in the United States,* published in 1922, dealt with the structure and function of these organizations for purposes of labor problems. The present work is designed to trace the development of employers' associations in various industries through the year 1900.

. The 500-odd pages in this book are literally crammed with facts and figures, arranged chronologically with industry breakdowns in each period. It is a painstaking description of the activities of employers' associations in such matters as resistance to union organization, strike tactics, negotiation practices, etc. But one looks in vain for an analysis of that behavior or for the basic

trends. Nor is there even a concluding chapter which might provide the reader with an incisive over-all view of the socio-economic forces responsible for shaping the policies of American employers' associations in labor matters during the period studied. To be sure, there is a final chapter entitled Summary and Conclusions; but it offers no such orientation. In brief, the book can best be characterized as a detailed chronology—nothing more, nothing less.

To aver that this is not an analytic piece of historical research is hardly synonymous with saying that the author's efforts have been wasted. Quite the contrary. The "raw material" in this volume can prove quite valuable for scholars with a more analytic bent of mind than Bonnett. They can utilize his findings to "test" and—where necessary—reformulate the relevant components of the various models now available to explain the collective bargaining institution in this country.

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Labor Economics and Industrial Relations. By A. L. GITLOW. (Homewood, Ill.: Richard D. Irwin. 1957. Pp. xvii, 724. $6.75.)

Professor Gitlow's book is one of several new textbooks on labor economics published in recent years and should be considered among the better ones. The author has covered the major topics necessary to an introductory course in labor economics without permitting the book to become too voluminous.

Part I deals primarily with population, migration, and labor-force materials. Trade unionism and industrial relations is the subject of Part II. It begins with chapters on the theory of the labor movement and union development and includes a discussion of a variety of issues, such as dualism and union rivalry, monopolistic effects of unions, collective bargaining objectives and policies, and the legal framework of the union and its bargaining policies. Part III is concerned with wages and employment. In addition to a concise presentation of the marginal productivity theory, there are chapters on wages and mobility, wages and employment, wage criteria, and wage supplements. Part IV is devoted to such problems as unemployment, technology, and insecurity.

In general, the book is well written. Its straightforward writing is not meant to entertain, but the student should find it clear and concise. Several of the topics could easily be rearranged should this be desired. The book is short enough (724 pages, but large print) to permit the instructor to add supplementary materials to chapters considered to be inadequately developed. This reviewer, for instance, would supplement the material on the labor force (Part I), union development (Part II), and trade union wage policy (Part III), if he were using the book in an introductory course in labor economics.

The heart of the book is Parts II and III, and there are several chapters in these sections worthy of particular mention. Gitlow is one of very few authors who make any attempt to consider the goals or purposes of trade unionism by presenting any theories of the labor movement. For this he is to be commended, although the theories may be presented too briefly for proper appreciation by the student. Had he been more selective in the number dis-

cussed, the space could have been devoted to the more important concepts.

Rather than including consideration of such issues as communism, dualism, leadership rivalry, and union corruption in his chapter on "Trade Union Government," Gitlow devotes an entire chapter to these as "Trade Union Problems." This is an interesting arrangement and certainly emphasizes some major problems, but this chapter is disappointing. It is superficial and choppy and fails to get beyond the reporting stages and down to an analytical treatment. Biographical sketches of a few labor leaders are presented rather than an examination of the status of leadership in the American union movement. The section dealing with union corruption is a newspaper account of some of the unions involved in racketeering and does not adequately meet the issues of corruption, ways unions become corrupted, where controls are lacking, and possible remedies.

The chapters on collective bargaining are closely integrated and well developed. He moves smoothly from the aims and scope of collective bargaining to the issues involved in the development of a trade agreement. A most adequate presentation of wage theory is made in Part III. Gitlow gives a fairly rigorous treatment of the marginal productivity analysis, recognizing the criticism of this theory in recent years.

After a general discussion of mobility, Gitlow gives a synopsis of thirteen case studies. They illustrate many of the problems involved in the determination of mobility and take the place of the more traditional listing of factors that make for and that work against mobility. The implications of these studies would have been clearer to the student if Gitlow had provided a more adequate appraisal of their results.

Gitlow's very good treatment of wage differentials might well have been preceded by a thorough treatment of trade-union wage policy. This topic which has received rather intensive research in recent years is mentioned only briefly in the theory section; but an entire chapter is devoted to wage criteria.

It is easy to point out omissions and to suggest additions, but any adverse comments should not overshadow the fact that a good book on labor economics has been written.

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NOTES

Simon S. Kuznets, of the Johns Hopkins University, has been appointed chairman of the American Economic Association nominating committee. He would appreciate suggestions for officers for next year at as early a date as possible.

ANNOUNCEMENT REGARDING THE PROGRAM FOR THE ANNUAL MEETING AT CHICAGO, DECEMBER 1958

The 1958 annual meeting of the Association will be held at the Palmer House, Chicago, Illinois, December 27-29. To provide the officers of the Association with more adequate information on papers in preparation that might merit a place on the program, last year President Copeland inaugurated an open competition. On the basis of this experiment an open competition will be repeated this year. Those wishing to enter the competition should write Professor James Washington Bell, Secretary, American Economic Association, Evanston, Illinois, indicating in not more than 100 words the nature of the proposed paper and requesting an instruction sheet and entry form. Papers entered in the competition must be in the hands of the committee of judges by September 1, 1958. Each paper will be identified by code number, and judges will make their selection without knowing the names of the authors. The authors of the winning papers will be invited to present them in one or more sessions at the Chicago meeting, and the papers will be published in the *Proceedings*. Preference will be given to papers that report on empirical research in progress or completed, or those of a theoretical nature that make a contribution towards integrating developments in different branches of economics or developments in economics and some other social science.

GEORGE W. STOCKING, *President*

AWARD OF THE FRANCIS A. WALKER AND JOHN BATES CLARK MEDALS

At the December 1957 meeting of the Association in Philadelphia, the Francis A. Walker medal was awarded to Frank H. Knight, and the John Bates Clark medal was awarded to Kenneth J. Arrow. In presenting the medals, George W. Stocking, chairman of the Committee on Honors and Awards in 1957 and now president of the Association, made the following remarks:

"The Francis A. Walker award is made 'not more frequently than once every five years to the living [American] economist who in the judgment of the awarding body has during his career made the greatest contribution to economics.' The medal this year is being awarded to a social philosopher the depth and breadth of whose thinking has few parallels in the history of the social sciences. All economists have lasting indebtedness to him for his classic study of the role of the entrepreneur in economic activity, *Risk, Uncertainty and Profit*. Social philosophers everywhere are indebted to him for his continuing and penetrating analysis of the relationship between ethics and economics. Distinguished scholar, beloved teacher, and profound student, Frank H. Knight has won an enduring place in the economists' Pantheon. On behalf of the American Economic Association, it is a privilege and a pleasure to confer on him the Francis A. Walker award.

"The John Bates Clark award is to be made 'every two years to that American economist under the age of 40 who is adjudged to have made a significant contribution to economic thought and knowledge.' Selecting a recipient for the Clark award has been difficult. Under the terms of the award several American economists could qualify. It was necessary, however, for the electoral college, *i.e.*, the executive and awards committees, to make a choice among the several strong candidates. Kenneth Arrow's work has been of central importance in the field of welfare economics. In his *Social Choice and Individual Values* he put a widely accepted approach to welfare economics into a broader context,

demonstrating that this approach was internally inconsistent on the level of generality at which it had been used, and he has thereby forced a rethinking of the logical foundations of the subject. The rigor of his analysis, the techniques used, and the conclusions reached have had what may well prove a lasting impact not alone in economics but in other fields dealing with values. On behalf of the Association I take pleasure in awarding the John Bates Clark medal to Kenneth J. Arrow."

STYLE INSTRUCTIONS

A revised version of the *Review's Style Instructions* for guidance in the preparation of manuscripts is now available upon request to the managing editor. In addition to making some minor improvements in the form of citations, an alternative method is provided for citing works to which reference is made: this is the method, frequently employed in scientific publications, of assembling a numbered list of references at the end of the paper and of referring to these works in the text by number. Although the older method of footnote citation will still be employed when authors prefer it, the newer method has important advantages—for example, it provides a bibliographical list, it saves space and type, and it saves the reader from searching for references identified by *op. cit.*

SUMMER INSTITUTES FOR FOREIGN STUDENTS PREPARATORY TO GRADUATE WORK IN ECONOMICS

The Ford Foundation has made possible for a three-year period summer institutes to provide training in basic economic analysis and in oral and written English for students who are entering the United States for graduate work in economics including agricultural economics. All expenses of the students, including additional travel, will be paid if necessary, but the students and their sponsoring institutions will be asked to contribute in so far as possible. Colleges, universities and government and international agencies interested in this program may recommend candidates for admission to the institutes. Application forms for recommending students are being provided by the Institute of International Education, 1 East 67th Street, New York 21, N.Y.

PUBLICATIONS

In October 1958 the first issue of a new international quarterly, *Comparative Studies in Society and History*, will be published by Mouton of The Hague with several American universities as sponsors. The purpose of the journal is to serve as a clearing house for substantive work on problems that are common to any two or more of the numerous branches of study dealing with man's life in society. A board of consulting editors is representative of comparative politics, comparative literature, economics, sociology, anthropology and various fields of history. Economics and economic history are represented by C. Cipolla (Venice), J. J. Spengler (Duke University), and C. Verlinden (Ghent). The editor is Sylvia L. Thrupp. Correspondence regarding contributions should be directed to the editor, Box 222, Faculty Exchange, University of Chicago, Chicago, Illinois.

Deaths

Renzo Bianchi, chairman of the department of economics, Carleton College, died November 30, 1957.

Benjamin F. Catherwood, retired instructor in economics at Purdue University, died January, 1957.

Herbert Dorn, professor emeritus of economics at the University of Delaware, died August 11, 1957.

Joseph B. Hubbard died December 10, 1957.

Visiting Foreign Scholars

P. Sargant Florence, of the University of Birmingham, England, has been appointed

visiting professor in political economy at the Johns Hopkins University for the February 1959 term.

H. J. Habakkuk, of All Souls College, Oxford University, has been appointed visiting Seager Lecturer at Columbia University for the winter semester of 1958-59.

Alan T. Peacock, of the University of Edinburgh, Scotland, has been appointed visiting professor in political economy at the Johns Hopkins University for the October 1958 term.

Alan R. Prest, lecturer in economics at Christ's College, Cambridge University, is in this country this spring on a Rockefeller travel grant.

George B. Richardson, of Nuffield College, Oxford University, is studying at Harvard University on a Rockefeller Foundation fellowship. He will visit Stanford University and other academic centers during the current academic year.

Francis Seton, of Nuffield College, Oxford University, has been appointed visiting associate professor in Russian economics at Columbia University for the winter semester of 1958-59.

Thomas Wilson, of University College, Oxford, is visiting professor of economics at the University of Oregon in the current spring term.

Appointments and Resignations

William H. Anderson, who has returned from a year of service with the International Cooperation Administration in Iran, has assumed the chairmanship of the department of economics at the University of Southern California.

Gary S. Becker has been appointed associate professor of economics in the Graduate Faculties of Political Science at Columbia University.

Myrtle Beinhauer, formerly of Drake University and Kansas State College, has been appointed assistant professor of economics at Western Michigan University.

Lewis Bell, formerly research associate in the Bureau of Business Research, University of Kentucky, is now director of the Division of Purchases, Kentucky Department of Finance.

Roy G. Blakey has accepted an appointment as visiting professor of economics at the University of New Mexico in the current semester.

Andrew F. Brimmer, of the Federal Reserve Bank of New York, has also been appointed lecturer at The City College, New York City.

Henry D. Brohm has resigned as professor of marketing at the University of Florida.

Samuel L. Brown has been appointed to the senior staff of the President's Council of Economic Advisers.

Robert W. Campbell has been promoted to assistant professor of economics at the University of Southern California.

Joseph Carrabino has been appointed associate professor of production management in the School of Business Administration, University of California, Los Angeles.

Allan M. Cartter is on leave from Duke University to be program associate, Economic Development and Administration, the Ford Foundation.

Abraham Charnes, formerly of Purdue University, has been appointed professor of applied mathematics and economics at Northwestern University.

Irwin Cochran is director of the Bureau of Business Management at the University of Illinois.

John A. Cochran, formerly of the University of Illinois, has been appointed associate professor of economics at Southern Illinois University.

John T. Conlon has been appointed assistant professor of industrial administration in the School of Business Administration, University of Massachusetts.

Leonard T. Conway has been promoted to assistant professor of economics at Hunter College.

Joseph D. Coppock, of Earlham College, is visiting professor at Indiana University in the current semester.

Ralph K. Davidson has been promoted to associate professor of economics at Purdue University.

Alain Enthoven, of the RAND Corporation, is visiting associate professor at the University of Washington during the spring quarter.

G. Heberton Evans, Jr., after a year's leave of absence, has resumed chairmanship of the department of political economy at the Johns Hopkins University.

Leon S. Felde has been appointed commercial director at the Consulate General of Belgium in New Orleans, La.

Thomas W. Gavett, formerly of the University of Wisconsin, has been appointed assistant professor of economics at West, Virginia University.

Alexander Gerschenkron, of Harvard University, is visiting research professor at the University of California during the current semester.

William D. Grampp, of the University of Illinois in Chicago, is visiting professor of economics at Indiana University during the current semester.

Philip B. Hartley has been appointed assistant professor of business administration at the University of Kansas.

Tyler F. Haygood has been appointed professor of economics at the School of Business, University of Louisville.

Rolf Hayn, on a year's leave of absence from, the University of Oklahoma, is working for the Treasury Department of the Puerto Rico government, San Juan, Puerto Rico.

Albert O. Hirschman, of Yale University, has been appointed visiting professor of economics in international trade at Columbia University for 1958-59.

Richard H. Holton, formerly of Harvard University, is an associate professor in the School of Business Administration, University of California, Berkeley.

C. Hayden Jamison, formerly of Beloit College, has assumed the position of economist at the Beloit State Bank, Beloit, Wisconsin.

George Jaszi has resigned from the U. S. Department of Commerce and has accepted a senior staff appointment at the Brookings Institution.

Keith W. Johnson, of the Pacific Gas and Electric Company, San Francisco, has been named technical consultant to the Committee on Economic Policy of the Business Advisory Council for the Department of Commerce.

Ronald Jones, formerly of Swarthmore College, has been appointed assistant professor at the University of Rochester.

Robert Judd, formerly of Bradley University, is assistant professor of economics at Beloit College.

William B. Kelly, Jr., formerly with U.S. Department of State, has been appointed William L. Clayton instructor and research associate in commercial policy at the Fletcher School, Tufts University.

Milo Kimball has retired from the University of Massachusetts where he was professor of business finance in the School of Business Administration.

John J. Klein has been appointed assistant professor of economics, Oklahoma State University.

Allen Kneese, of the University of New Mexico, has accepted a position as financial economist with the Federal Reserve Bank of Kansas City.

Alfred Kressel has accepted a position on the faculty of the University of Buffalo.

Kenneth Kurihara, of Rutgers University, has been appointed visiting professor at the University of Washington in the spring quarter.

George E. Lent, formerly with the U.S. Treasury Department and Department of Defense, is visiting professor of business economics and director of research at the Amos. Tuck School, Dartmouth University.

Dudley G. Luckett has been appointed instructor in the department of economics and sociology at Iowa State College.

Edward Marcus has been promoted to associate professor of economics at Brooklyn College.

James W. Martin has been transferred from Commissioner of Finance to Commissioner or Highways in the state of Kentucky. He is conducting a second-semester seminar on government finance administration at the University of Kentucky, where he is director of the Bureau of Business Research.

O. J. McDiarmid is on leave from the International Bank for Reconstruction and Development to serve as economic advisor to the Development Board of Iraq.

George L. Melville, of Knox College, is serving as visiting assistant professor at Indiana University during the current semester.

Aurelius Morgner, of Texas A & M College, is serving as visiting professor of economics at the Escola de Sociologia e Politica, University of Sao Paulo, Brazil, from February through August, 1958.

William G. Murray, of the division of agriculture, Iowa State College, has been awarded the 1957 Gamma Sigma Delta Award for Distinguished Service to Agriculture.

Wladimir Naleszkiewicz has been appointed instructor in economics at the Henderson State Teachers College, Arkadelphia, Arkansas.

Ronald R. Olsen, formerly of Ohio University, has been appointed assistant professor of labor economics at the University of Kansas.

Oscar A. Ornati, formerly of Cornell University, has been appointed associate professor of economics in the Graduate Faculty of Political and Social Science of the New School for Social Research.

Richard Peirce has been appointed assistant professor of accounting in the School of Business Administration, University of California, Los Angeles.

Jacob Perlman, formerly with the United Nations, has been appointed head of the Office of Special Studies, National Science Foundation.

Kirk R. Petshek has been promoted from assistant director of commerce for economic development to urban development and economic coordinator, City of Philadelphia.

Jack A. Pontney has been appointed instructor in economics at the University of Southern California.

Philip K. Rahbany has been appointed assistant professor at Western Michigan University.

Stanley Reiter has been promoted to associate professor of economics at Purdue University.

Philip T. Ries, formerly of the Jenkintown Bank and Trust Company, has accepted a position as assistant title officer and manager of the Jenkintown branch of the Title Insurance Corporation of Pennsylvania.

Richard Rosett, formerly of Yale University, has been appointed assistant professor at the University of Rochester.

David J. Saposs is visiting professor in the Institute of Labor and Industrial Relations, University of Illinois, during the second semester of 1957-58.

Alfred L. Seelye has been appointed dean of the College of Business and Public Service, Michigan State University.

Satya S. Sengupta has been appointed research associate in the department of economics and sociology, Iowa State College.

Robert G. Seymour has been named associate dean of the College of Commerce and Business Administration, University of Illinois.

William K. Sharkey has been promoted to associate professor of economics at Purdue University.

Ansel M. Sharp has been appointed assistant professor of economics at Oklahoma State University.

Philip Sheinwold has been promoted to assistant professor of economics at Brooklyn College.

George J. Stigler, of Columbia University, has been appointed Walgreen Professor of American Institutions and director of the Walgreen Foundation in the School of Business, University of Chicago, effective October 1, 1958.

Frank T. Stockton, retired from the University of Kansas, is now connected with the Kansas Civil Defense Survival Project.

Curwen Stoddart has been promoted to assistant professor of economics at Brooklyn College.

Paul Studensky has resigned as director of the Albany Graduate Program in Public Administration to accept a position as senior fiscal consultant with the New York State Temporary Commission on the Constitutional Convention.

Irving Swerdlow has been appointed professor of economics at Syracuse University.

Raymond D. Thomas, formerly dean of the College of Business, Oklahoma State University, has been appointed director of the Oklahoma State Department of Commerce and Industry.

Robert W. Travis has been promoted to associate professor of real estate at the University of Florida.

Paul M. Van Arsdell is head of the new department of finance, University of Illinois.

Desider Vikor has been appointed assistant professor of economics at Memphis State University.

William Voris, of Los Angeles State College, is on an International Cooperation Administration assignment in Teheran, Iran.

Arnold R. Weber has been appointed assistant professor of industrial relations in the School of Business, University of Chicago, effective July 1, 1958.

Frank J. Welch, of the University of Kentucky, is on leave of absence to serve on the Board of Directors of the Tennessee Valley Authority.

Jay W. Wiley has been promoted to professor of economics at Purdue University.

Robert E. Will has been appointed acting chairman of the department of economics, Carleton College.

Herman J. Wyngarden retired on July 1, 1957 from his post as dean, College of Business and Public Service, Michigan State University.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Economics: Instructor or assistant professor, to teach principles of economics and one or two advanced courses in finance. Permanent position with opportunity for promotion. Teaching experience preferred. Desire man with Ph.D. or with requirements for Ph.D. practically completed. Salary and rank to be determined by education and experience.

Management: Assistant professor to teach courses in management. Permanent position with opportunity for promotion. Ph.D. and teaching experience preferred. Good opportunity for consulting work in a large industrial center. Salary depends upon training and experience.

Accounting: Instructor or assistant professor to teach courses in accounting. Position permanent with opportunity for promotion. Prefer one with C.P.A. and previous teaching experience. Salary and rank based upon training and experience.

Marketing: Assistant professor to teach courses in marketing. Position permanent with opportunity for promotion. Prefer one with Ph.D. and previous teaching experience. Salary and rank based upon training and experience. Good opportunity for consultation work in large industrial center.

Anyone interested in any of the four positions above is asked to write: Dr. Herman P. Thomas, School of Business Administration, University of Richmond, Virginia.

Accounting and statistics: A leading Southern college will have an opening in September, 1958, for a young man with or close to his Ph.D. Courses will include elementary accounting and statistics. Salary and rank depend upon training and experience. P198

International economics, money and banking: A medium sized, Midwestern university will have a vacancy in September, 1958, for a young man with or very close to his Ph.D. Salary approximately $5,600 for academic year, depending upon training and experience. P200

Social studies: Associate professor or professor of social studies (depending on experience and academic background), beginning September 1, 1958; salary $5,976 or $7,206. Master's degree with major training in economics required. Preparation should include several of the following areas: economic theory, consumer economics, labor problems, international trade, industrial organization, and international economic problems. At least thirty credit hours beyond the master's level are expected and promotion in rank would require considerable organized study at the doctoral level. Write: State University Teachers College, Department of Social Studies, Plattsburgh, N.Y.

Economics: Professor of economics and head of the department in a small-sized liberal arts college located in a metropolitan area in the Middle West. Must be an excellent teacher and have completed work for Ph.D. Salary depends upon qualifications and experience. P201

ARTICLES

OTHER DEPARTMENTS

THE COST OF CAPITAL, CORPORATION FINANCE
AND THE THEORY OF INVESTMENT

By Franco Modigliani and Merton H. Miller[*]

What is the "cost of capital" to a firm in a world in which funds are used to acquire assets whose yields are uncertain; and in which capital can be obtained by many different media, ranging from pure debt instruments, representing money-fixed claims, to pure equity issues, giving holders only the right to a pro-rata share in the uncertain venture? This question has vexed at least three classes of economists: (1) the corporation finance specialist concerned with the techniques of financing firms so as to ensure their survival and growth; (2) the managerial economist concerned with capital budgeting; and (3) the economic theorist concerned with explaining investment behavior at both the micro and macro levels.[1]

In much of his formal analysis, the economic theorist at least has tended to side-step the essence of this cost-of-capital problem by proceeding as though physical assets—like bonds—could be regarded as yielding known, sure streams. Given this assumption, the theorist has concluded that the cost of capital to the owners of a firm is simply the rate of interest on bonds; and has derived the familiar proposition that the firm, acting rationally, will tend to push investment to the point

[1] The literature bearing on the cost-of-capital problem is far too extensive for listing here. Numerous references to it will be found throughout the paper though we make no claim to completeness. One phase of the problem which we do not consider explicitly, but which has a considerable literature of its own is the relation between the cost of capital and public utility rates. For a recent summary of the "cost-of-capital theory" of rate regulation and a brief discussion of some of its implications, the reader may refer to H. M. Somers [20].

where the marginal yield on physical assets is equal to the market rate
of interest.[2] This proposition can be shown to follow from either of two
criteria of rational decision-making which are equivalent under certain-
ty, namely (1) the maximization of profits and (2) the maximization of
market value.

According to the first criterion, a physical asset is worth acquiring if
it will increase the net profit of the owners of the firm. But net profit
will increase only if the expected rate of return, or yield, of the asset
exceeds the rate of interest. According to the second criterion, an asset
is worth acquiring if it increases the value of the owners' equity, *i.e.*, if
it adds more to the market value of the firm than the costs of acquisi-
tion. But what the asset adds is given by capitalizing the stream it gen-
erates at the market rate of interest, and this capitalized value will
exceed its cost if and only if the yield of the asset exceeds the rate of
interest. Note that, under either formulation, the cost of capital is equal
to the rate of interest on bonds, regardless of whether the funds are
acquired through debt instruments or through new issues of common
stock. Indeed, in a world of sure returns, the distinction between debt
and equity funds reduces largely to one of terminology.

It must be acknowledged that some attempt is usually made in this
type of analysis to allow for the existence of uncertainty. This attempt
typically takes the form of superimposing on the results of the certainty
analysis the notion of a "risk discount" to be subtracted from the ex-
pected yield (or a "risk premium" to be added to the market rate of
interest). Investment decisions are then supposed to be based on a com-
parison of this "risk adjusted" or "certainty equivalent" yield with the
market rate of interest.[3] No satisfactory explanation has yet been pro-
vided, however, as to what determines the size of the risk discount and
how it varies in response to changes in other variables.

Considered as a convenient approximation, the model of the firm
constructed via this certainty—or certainty-equivalent—approach has
admittedly been useful in dealing with some of the grosser aspects of
the processes of capital accumulation and economic fluctuations. Such
a model underlies, for example, the familiar Keynesian aggregate invest-
ment function in which aggregate investment is written as a function of
the rate of interest—the same riskless rate of interest which appears
later in the system in the liquidity-preference equation. Yet few would
maintain that this approximation is adequate. At the macroeconomic
level there are ample grounds for doubting that the rate of interest has

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[2] Or, more accurately, to the marginal cost of borrowed funds since it is customary, at least
in advanced analysis, to draw the supply curve of borrowed funds to the firm as a rising one.
For an advanced treatment of the certainty case, see F. and V. Lutz [13].

[3] The classic examples of the certainty-equivalent approach are found in J. R. Hicks [8] and
O. Lange [11].

as large and as direct an influence on the rate of investment as this analysis would lead us to believe. At the microeconomic level the certainty model has little descriptive value and provides no real guidance to the finance specialist or managerial economist whose main problems cannot be treated in a framework which deals so cavalierly with uncertainty and ignores all forms of financing other than debt issues.[4]

Only recently have economists begun to face up seriously to the problem of the cost of capital *cum* risk. In the process they have found their interests and endeavors merging with those of the finance specialist and the managerial economist who have lived with the problem longer and more intimately. In this joint search to establish the principles which govern rational investment and financial policy in a world of uncertainty two main lines of attack can be discerned. These lines represent, in effect, attempts to extrapolate to the world of uncertainty each of the two criteria—profit maximization and market value maximization—which were seen to have equivalent implications in the special case of certainty. With the recognition of uncertainty this equivalence vanishes. In fact, the profit maximization criterion is no longer even well defined. Under uncertainty there corresponds to each decision of the firm not a unique profit outcome, but a plurality of mutually exclusive outcomes which can at best be described by a subjective probability distribution. The profit outcome, in short, has become a random variable and as such its maximization no longer has an operational meaning. Nor can this difficulty generally be disposed of by using the mathematical expectation of profits as the variable to be maximized. For decisions which affect the expected value will also tend to affect the dispersion and other characteristics of the distribution of outcomes. In particular, the use of debt rather than equity funds to finance a given venture may well increase the expected return to the owners, but only at the cost of increased dispersion of the outcomes.

Under these conditions the profit outcomes of alternative investment and financing decisions can be compared and ranked only in terms of a *subjective* "utility function" of the owners which weighs the expected yield against other characteristics of the distribution. Accordingly, the extrapolation of the profit maximization criterion of the certainty model has tended to evolve into utility maximization, sometimes explicitly, more frequently in a qualitative and heuristic form.[5]

The utility approach undoubtedly represents an advance over the certainty or certainty-equivalent approach. It does at least permit us

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[4] Those who have taken a "case-method" course in finance in recent years will recall in this connection the famous Liquigas case of Hunt and Williams, |9, pp. 193–96] a case which is often used to introduce the student to the cost-of-capital problem and to poke a bit of fun at the economist's certainty-model.

[5] For an attempt at a rigorous explicit development of this line of attack, see F. Modigliani and M. Zeman [14].

to explore (within limits) some of the implications of different financing arrangements, and it does give some meaning to the "cost" of different types of funds. However, because the cost of capital has become an essentially subjective concept, the utility approach has serious drawbacks for normative as well as analytical purposes. How, for example, is management to ascertain the risk preferences of its stockholders and to compromise among their tastes? And how can the economist build a meaningful investment function in the face of the fact that any given investment opportunity might or might not be worth exploiting depending on precisely who happen to be the owners of the firm at the moment?

Fortunately, these questions do not have to be answered; for the alternative approach, based on market value maximization, can provide the basis for an operational definition of the cost of capital and a workable theory of investment. Under this approach any investment project and its concomitant financing plan must pass only the following test: Will the project, as financed, raise the market value of the firm's shares? If so, it is worth undertaking; if not, its return is less than the marginal cost of capital to the firm. Note that such a test is entirely independent of the tastes of the current owners, since market prices will reflect not only their preferences but those of all potential owners as well. If any current stockholder disagrees with management and the market over the valuation of the project, he is free to sell out and reinvest elsewhere, but will still benefit from the capital appreciation resulting from management's decision.

The potential advantages of the market-value approach have long been appreciated; yet analytical results have been meager. What appears to be keeping this line of development from achieving its promise is largely the lack of an adequate theory of the effect of financial structure on market valuations, and of how these effects can be inferred from objective market data. It is with the development of such a theory and of its implications for the cost-of-capital problem that we shall be concerned in this paper.

Our procedure will be to develop in Section I the basic theory itself and to give some brief account of its empirical relevance. In Section II, we show how the theory can be used to answer the cost-of-capital question and how it permits us to develop a theory of investment of the firm under conditions of uncertainty. Throughout these sections the approach is essentially a partial-equilibrium one focusing on the firm and "industry." Accordingly, the "prices" of certain income streams will be treated as constant and given from outside the model, just as in the standard Marshallian analysis of the firm and industry the prices of all inputs and of all other products are taken as given. We have chosen to focus at this level rather than on the economy as a whole because it

is at the level of the firm and the industry that the interests of the various specialists concerned with the cost-of-capital problem come most closely together. Although the emphasis has thus been placed on partial-equilibrium analysis, the results obtained also provide the essential building blocks for a general equilibrium model which shows how those prices which are here taken as given, are themselves determined. For reasons of space, however, and because the material is of interest in its own right, the presentation of the general equilibrium model which rounds out the analysis must be deferred to a subsequent paper.

I. *The Valuation of Securities, Leverage, and the Cost of Capital*

A. *The Capitalization Rate for Uncertain Streams*

As a starting point, consider an economy in which all physical assets are owned by corporations. For the moment, assume that these corporations can finance their assets by issuing common stock only; the introduction of bond issues, or their equivalent, as a source of corporate funds is postponed until the next part of this section.

The physical assets held by each firm will yield to the owners of the firm—its stockholders—a stream of "profits" over time; but the elements of this series need not be constant and in any event are uncertain. This stream of income, and hence the stream accruing to any share of common stock, will be regarded as extending indefinitely into the future. We assume, however, that the mean value of the stream over time, or average profit per unit of time, is finite and represents a random variable subject to a (subjective) probability distribution. We shall refer to the average value over time of the stream accruing to a given share as the return of that share; and to the mathematical expectation of this average as the expected return of the share.[6] Although individual investors may have different views as to the shape of the probability distri-

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[6] These propositions can be restated analytically as follows: The assets of the ith firm generate a stream:

$$X_i(1), X_i(2) \cdots X_i(T)$$

whose elements are random variables subject to the joint probability distribution:

$$\chi_i[X_i(1), X_i(2) \cdots X_i(t)].$$

The return to the ith firm is defined as:

$$X_i = \lim_{T \to \infty} \frac{1}{T} \sum_{t=1}^{T} X_i(t).$$

X_i is itself a random variable with a probability distribution $\Phi_i(X_i)$ whose form is determined uniquely by χ_i. The expected return $\bar{X}_i$ is defined as $\bar{X}_i = E(X_i) = \int_{X_i} X_i \Phi_i(X_i) dX_i$. If N_i is the number of shares outstanding, the return of the ith share is $x_i = (1/N)X_i$ with probability distribution $\phi_i(x_i)dx_i = \Phi_i(Nx_i)d(Nx_i)$ and expected value $\bar{x}_i = (1/N)\bar{X}_i$.

bution of the return of any share, we shall assume for simplicity that they are at least in agreement as to the expected return.[7]

This way of characterizing uncertain streams merits brief comment. Notice first that the stream is a stream of profits, not dividends. As will become clear later, as long as management is presumed to be acting in the best interests of the stockholders, retained earnings can be regarded as equivalent to a fully subscribed, pre-emptive issue of common stock. Hence, for present purposes, the division of the stream between cash dividends and retained earnings in any period is a mere detail. Notice also that the uncertainty attaches to the mean value over time of the stream of profits and should not be confused with variability over time of the successive elements of the stream. That variability and uncertainty are two totally different concepts should be clear from the fact that the elements of a stream can be variable even though known with certainty. It can be shown, furthermore, that whether the elements of a stream are sure or uncertain, the effect of variability per se on the valuation of the stream is at best a second-order one which can safely be neglected for our purposes (and indeed most others too).[8]

The next assumption plays a strategic role in the rest of the analysis. We shall assume that firms can be divided into "equivalent return" classes such that the return on the shares issued by any firm in any given class is proportional to (and hence perfectly correlated with) the return on the shares issued by any other firm in the same class. This assumption implies that the various shares within the same class differ, at most, by a "scale factor." Accordingly, if we adjust for the difference in scale, by taking the *ratio* of the return to the expected return, the probability distribution of that ratio is identical for all shares in the class. It follows that all relevant properties of a share are uniquely characterized by specifying (1) the class to which it belongs and (2) its expected return.

The significance of this assumption is that it permits us to classify firms into groups within which the shares of different firms are "homogeneous," that is, perfect substitutes for one another. We have, thus, an analogue to the familiar concept of the industry in which it is the commodity produced by the firms that is taken as homogeneous. To complete this analogy with Marshallian price theory, we shall assume in the

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[7] To deal adequately with refinements such as differences among investors in estimates of expected returns would require extensive discussion of the theory of portfolio selection. Brief references to these and related topics will be made in the succeeding article on the general equilibrium model.

[8] The reader may convince himself of this by asking how much he would be willing to rebate to his employer for the privilege of receiving his annual salary in equal monthly installments rather than in irregular amounts over the year. See also J. M. Keynes [10, esp. pp. 53–54].

analysis to follow that the shares concerned are traded in perfect markets under conditions of atomistic competition.[9]

From our definition of homogeneous classes of stock it follows that in equilibrium in a perfect capital market the price per dollar's worth of expected return must be the same for all shares of any given class. Or, equivalently, in any given class the price of every share must be proportional to its expected return. Let us denote this factor of proportionality for any class, say the kth class, by $1/\rho_k$. Then if p_j denotes the price and $\bar{x}_j$ is the expected return per share of the jth firm in class k, we must have:

(1)
$$p_j = \frac{1}{\rho_k} \bar{x}_j;$$

or, equivalently,

(2)
$$\frac{\bar{x}_j}{p_j} = \rho_k \text{ a constant for all firms } j \text{ in class } k.$$

The constants ρ_k (one for each of the k classes) can be given several economic interpretations: (a) From (2) we see that each ρ_k is the expected rate of return of any share in class k. (b) From (1) $1/\rho_k$ is the price which an investor has to pay for a dollar's worth of expected return in the class k. (c) Again from (1), by analogy with the terminology for perpetual bonds, ρ_k can be regarded as the market rate of capitalization for the expected value of the uncertain streams of the kind generated by the kth class of firms.[10]

B. *Debt Financing and Its Effects on Security Prices*

Having developed an apparatus for dealing with uncertain streams we can now approach the heart of the cost-of-capital problem by dropping the assumption that firms cannot issue bonds. The introduction of debt-financing changes the market for shares in a very fundamental way. Because firms may have different proportions of debt in their capi-

[9] Just what our classes of stocks contain and how the different classes can be identified by outside observers are empirical questions to which we shall return later. For the present, it is sufficient to observe: (1) Our concept of a class, while not identical to that of the industry is at least closely related to it. Certainly the basic characteristics of the probability distributions of the returns on assets will depend to a significant extent on the product sold and the technology used. (2) What are the appropriate class boundaries will depend on the particular problem being studied. An economist concerned with general tendencies in the market, for example, might well be prepared to work with far wider classes than would be appropriate for an investor planning his portfolio, or a firm planning its financial strategy.

[10] We cannot, on the basis of the assumptions so far, make any statements about the relationship or spread between the various ρ's or capitalization rates. Before we could do so we would have to make further specific assumptions about the way investors believe the probability distributions vary from class to class, as well as assumptions about investors' preferences as between the characteristics of different distributions.

tal structure, shares of different companies, even in the same class, can give rise to different probability distributions of returns. In the language of finance, the shares will be subject to different degrees of financial risk or "leverage" and hence they will no longer be perfect substitutes for one another.

To exhibit the mechanism determining the relative prices of shares under these conditions, we make the following two assumptions about the nature of bonds and the bond market, though they are actually stronger than is necessary and will be relaxed later: (1) All bonds (including any debts issued by households for the purpose of carrying shares) are assumed to yield a constant income per unit of time, and this income is regarded as certain by all traders regardless of the issuer. (2) Bonds, like stocks, are traded in a perfect market, where the term perfect is to be taken in its usual sense as implying that any two commodities which are perfect substitutes for each other must sell, in equilibrium, at the same price. It follows from assumption (1) that all bonds are in fact perfect substitutes up to a scale factor. It follows from assumption (2) that they must all sell at the same price per dollar's worth of return, or what amounts to the same thing must yield the same rate of return. This rate of return will be denoted by r and referred to as the rate of interest or, equivalently, as the capitalization rate for sure streams. We now can derive the following two basic propositions with respect to the valuation of securities in companies with different capital structures:

Proposition I. Consider any company j and let $\overline{X}_j$ stand as before for the expected return on the assets owned by the company (that is, its expected profit before deduction of interest). Denote by D_j the market value of the debts of the company; by S_j the market value of its common shares; and by $V_j \equiv S_j + D_j$ the market value of all its securities or, as we shall say, the market value of the firm. Then, our Proposition I asserts that we must have in equilibrium:

(3) $V_j \equiv (S_j + D_j) = \overline{X}_j/\rho_k$, for any firm j in class k.

That is, the *market value of any firm is independent of its capital structure and is given by capitalizing its expected return at the rate ρ_k appropriate to its class.*

This proposition can be stated in an equivalent way in terms of the firm's "average cost of capital," $\overline{X}_j/V_j$, which is the ratio of its expected return to the market value of all its securities. Our proposition then is:

(4) $\dfrac{\overline{X}_j}{(S_j + D_j)} \equiv \dfrac{\overline{X}_j}{V_j} = \rho_k$, for any firm j, in class k.

That is, *the average cost of capital to any firm is completely independent of*

its capital structure and is equal to the capitalization rate of a pure equity stream of its class.

To establish Proposition I we will show that as long as the relations (3) or (4) do not hold between any pair of firms in a class, arbitrage will take place and restore the stated equalities. We use the term arbitrage advisedly. For if Proposition I did not hold, an investor could buy and sell stocks and bonds in such a way as to exchange one income stream for another stream, identical in all relevant respects but selling at a lower price. The exchange would therefore be advantageous to the investor quite independently of his attitudes toward risk.[11] As investors exploit these arbitrage opportunities, the value of the overpriced shares will fall and that of the underpriced shares will rise, thereby tending to eliminate the discrepancy between the market values of the firms.

By way of proof, consider two firms in the same class and assume for simplicity only, that the expected return, $\overline{X}$, is the same for both firms. Let company 1 be financed entirely with common stock while company 2 has some debt in its capital structure. Suppose first the value of the levered firm, V_2, to be larger than that of the unlevered one, V_1. Consider an investor holding s_2 dollars' worth of the shares of company 2, representing a fraction α of the total outstanding stock, S_2. The return from this portfolio, denoted by Y_2, will be a fraction α of the income available for the stockholders of company 2, which is equal to the total return X_2 less the interest charge, rD_2. Since under our assumption of homogeneity, the anticipated total return of company 2, X_2, is, under all circumstances, the same as the anticipated total return to company 1, X_1, we can hereafter replace X_2 and X_1 by a common symbol X. Hence, the return from the initial portfolio can be written as:

$$(5) \qquad\qquad Y_2 = \alpha(X - rD_2).$$

Now suppose the investor sold his αS_2 worth of company 2 shares and acquired instead an amount $s_1 = \alpha(S_2 + D_2)$ of the shares of company 1. He could do so by utilizing the amount αS_2 realized from the sale of his initial holding and borrowing an additional amount αD_2 on his own credit, pledging his new holdings in company 1 as a collateral. He would thus secure for himself a fraction $s_1/S_1 = \alpha(S_2 + D_2)/S_1$ of the shares and earnings of company 1. Making proper allowance for the interest payments on his personal debt αD_2, the return from the new portfolio, Y_1, is given by:

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[11] In the language of the theory of choice, the exchanges are movements from inefficient points in the interior to efficient points on the boundary of the investor's opportunity set; and not movements between efficient points along the boundary. Hence for this part of the analysis nothing is involved in the way of specific assumptions about investor attitudes or behavior other than that investors behave consistently and prefer more income to less income, *ceteris paribus*.

$$(6) \qquad Y_1 = \frac{\alpha(S_2 + D_2)}{S_1} X - r\alpha D_2 = \alpha \frac{V_2}{V_1} X - r\alpha D_2.$$

Comparing (5) with (6) we see that as long as $V_2 > V_1$ we must have $Y_1 > Y_2$, so that it pays owners of company 2's shares to sell their holdings, thereby depressing S_2 and hence V_2; and to acquire shares of company 1, thereby raising S_1 and thus V_1. We conclude therefore that levered companies cannot command a premium over unlevered companies because investors have the opportunity of putting the equivalent leverage into their portfolio directly by borrowing on personal account.

Consider now the other possibility, namely that the market value of the levered company V_2 is less than V_1. Suppose an investor holds initially an amount s_1 of shares of company 1, representing a fraction α of the total outstanding stock, S_1. His return from this holding is:

$$Y_1 = \frac{s_1}{S_1} X = \alpha X.$$

Suppose he were to exchange this initial holding for another portfolio, also worth s_1, but consisting of s_2 dollars of stock of company 2 and of d dollars of bonds, where s_2 and d are given by:

$$(7) \qquad s_2 = \frac{S_2}{V_2} s_1, \qquad d = \frac{D_2}{V_2} s_1.$$

In other words the new portfolio is to consist of stock of company 2 and of bonds in the proportions S_2/V_2 and D_2/V_2, respectively. The return from the stock in the new portfolio will be a fraction s_2/S_2 of the total return to stockholders of company 2, which is $(X - rD_2)$, and the return from the bonds will be rd. Making use of (7), the total return from the portfolio, Y_2, can be expressed as follows:

$$Y_2 = \frac{s_2}{S_2} (X - rD_2) + rd = \frac{s_1}{V_2} (X - rD_2) + r \frac{D_2}{V_2} s_1 = \frac{s_1}{V_2} X = \alpha \frac{S_1}{V_2} X$$

(since $s_1 = \alpha S_1$). Comparing Y_2 with Y_1 we see that, if $V_2 < S_1 \equiv V_1$, then Y_2 will exceed Y_1. Hence it pays the holders of company 1's shares to sell these holdings and replace them with a mixed portfolio containing an appropriate fraction of the shares of company 2.

The acquisition of a mixed portfolio of stock of a levered company j and of bonds in the proportion S_j/V_j and D_j/V_j respectively, may be regarded as an operation which "undoes" the leverage, giving access to an appropriate fraction of the unlevered return X_j. It is this possibility of undoing leverage which prevents the value of levered firms from being consistently less than those of unlevered firms, or more generally prevents the average cost of capital $\overline{X}_j/V_j$ from being systematically higher for levered than for nonlevered companies in the same class.

Since we have already shown that arbitrage will also prevent V_2 from being larger than V_1, we can conclude that in equilibrium we must have $V_2 = V_1$, as stated in Proposition I.

Proposition II. From Proposition I we can derive the following proposition concerning the rate of return on common stock in companies whose capital structure includes some debt: the expected rate of return or yield, i, on the stock of any company j belonging to the kth class is a linear function of leverage as follows:

$$(8) \qquad\qquad i_j = \rho_k + (\rho_k - r) D_j/S_j.$$

That is, *the expected yield of a share of stock is equal to the appropriate capitalization rate ρ_k for a pure equity stream in the class, plus a premium related to financial risk equal to the debt-to-equity ratio times the spread between ρ_k and r.* Or equivalently, the market price of any share of stock is given by capitalizing its expected return at the continuously variable rate i_j of (8).[12]

A number of writers have stated close equivalents of our Proposition I although by appealing to intuition rather than by attempting a proof and only to insist immediately that the results were not applicable to the actual capital markets.[13] Proposition II, however, so far as we have been able to discover is new.[14] To establish it we first note that, by definition, the expected rate of return, i, is given by:

$$(9) \qquad\qquad i_j \equiv \frac{\overline{X}_j - rD_j}{S_j}.$$

From Proposition I, equation (3), we know that:

$$\overline{X}_j = \rho_k(S_j + D_j).$$

Substituting in (9) and simplifying, we obtain equation (8).

[12] To illustrate, suppose $\overline{X} = 1000$, $D = 4000$, $r = 5$ per cent and $\rho_k = 10$ per cent. These values imply that $V = 10,000$ and $S = 6000$ by virtue of Proposition I. The expected yield or rate of return per share is then:

$$i = \frac{1000 - 200}{6000} = .1 + (.1 - .05)\frac{4000}{6000} = 13\tfrac{1}{3} \text{ per cent.}$$

[13] See, for example, J. B. Williams [21], esp. pp. 72–73]; David Durand [3]; and W. A. Morton [15]. None of these writers describe in any detail the mechanism which is supposed to keep the average cost of capital constant under changes in capital structure. They seem, however, to be visualizing the equilibrating mechanism in terms of switches by investors between stocks and bonds as the yields of each get out of line with their "riskiness." This is an argument quite different from the pure arbitrage mechanism underlying our proof, and the difference is crucial. Regarding Proposition I as resting on investors' attitudes toward risk leads inevitably to a misunderstanding of many factors influencing relative yields such as, for example, limitations on the portfolio composition of financial institutions. See below, esp. Section I.D.

[14] Morton does make reference to a linear yield function but only " . . . for the sake of simplicity and because the particular function used makes no essential difference in my conclusions" [15, p. 443, note 2].

C. *Some Qualifications and Extensions of the Basic Propositions*

The methods and results developed so far can be extended in a number of useful directions, of which we shall consider here only three: (1) allowing for a corporate profits tax under which interest payments are deductible; (2) recognizing the existence of a multiplicity of bonds and interest rates; and (3) acknowledging the presence of market imperfections which might interfere with the process of arbitrage. The first two will be examined briefly in this section with some further attention given to the tax problem in Section II. Market imperfections will be discussed in Part D of this section in the course of a comparison of our results with those of received doctrines in the field of finance.

Effects of the Present Method of Taxing Corporations. The deduction of interest in computing taxable corporate profits will prevent the arbitrage process from making the value of all firms in a given class proportional to the expected returns generated by their physical assets. Instead, it can be shown (by the same type of proof used for the original version of Proposition I) that the market values of firms in each class must be proportional in equilibrium to their expected return net of taxes (that is, to the sum of the interest paid and expected net stockholder income). This means we must replace each $\overline{X}_j$ in the original versions of Propositions I and II with a new variable $\overline{X}_j^\tau$ representing the total income net of taxes generated by the firm:

$$(10) \qquad \overline{X}_j^\tau \equiv (\overline{X}_j - rD_j)(1 - \tau) + rD_j \equiv \bar{\pi}_j^\tau + rD_j,$$

where $\bar{\pi}_j^\tau$ represents the expected net income accruing to the common stockholders and τ stands for the average rate of corporate income tax.[15]

After making these substitutions, the propositions, when adjusted for taxes, continue to have the same form as their originals. That is, Proposition I becomes:

$$(11) \qquad \frac{\overline{X}_j^\tau}{V_j} = \rho_k^\tau, \text{ for any firm in class } k,$$

and Proposition II becomes

$$(12) \qquad i_j \equiv \frac{\bar{\pi}_j^\tau}{S_j} = \rho_j^\tau + (\rho_k^\tau - r)D_j/S_j$$

where ρ_k^τ is the capitalization rate for income net of taxes in class k.

Although the form of the propositions is unaffected, certain interpretations must be changed. In particular, the after-tax capitalization rate

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[15] For simplicity, we shall ignore throughout the tiny element of progression in our present corporate tax and treat τ as a constant independent of $(X_j - rD_j)$.

$\rho_k{}^r$ can no longer be identified with the "average cost of capital" which is $\rho_k = \overline{X}_j/V_j$. The difference between $\rho_k{}^r$ and the "true" average cost of capital, as we shall see, is a matter of some relevance in connection with investment planning within the firm (Section II). For the description of market behavior, however, which is our immediate concern here, the distinction is not essential. To simplify presentation, therefore, and to preserve continuity with the terminology in the standard literature we shall continue in this section to refer to $\rho_k{}^r$ as the average cost of capital, though strictly speaking this identification is correct only in the absence of taxes.

Effects of a Plurality of Bonds and Interest Rates. In existing capital markets we find not one, but a whole family of interest rates varying with maturity, with the technical provisions of the loan and, what is most relevant for present purposes, with the financial condition of the borrower.[16] Economic theory and market experience both suggest that the yields demanded by lenders tend to increase with the debt-equity ratio of the borrowing firm (or individual). If so, and if we can assume as a first approximation that this yield curve, $r = r (D/S)$, whatever its precise form, is the same for all borrowers, then we can readily extend our propositions to the case of a rising supply curve for borrowed funds.[17]

Proposition I is actually unaffected in form and interpretation by the fact that the rate of interest may rise with leverage; while the average cost of *borrowed* funds will tend to increase as debt rises, the average cost of funds from *all* sources will still be independent of leverage (apart from the tax effect). This conclusion follows directly from the ability of those who engage in arbitrage to undo the leverage in any financial structure by acquiring an appropriately mixed portfolio of bonds and stocks. Because of this ability, the ratio of earnings (*before* interest charges) to market value—*i.e.*, the average cost of capital from all

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[16] We shall not consider here the extension of the analysis to encompass the time structure of interest rates. Although some of the problems posed by the time structure can be handled within our comparative statics framework, an adequate discussion would require a separate paper.

[17] We can also develop a theory of bond valuation along lines essentially parallel to those followed for the case of shares. We conjecture that the curve of bond yields as a function of leverage will turn out to be a nonlinear one in contrast to the linear function of leverage developed for common shares. However, we would also expect that the rate of increase in the yield on new issues would not be substantial in practice. This relatively slow rise would reflect the fact that interest rate increases by themselves can never be completely satisfactory to creditors as compensation for their increased risk. Such increases may simply serve to raise r so high relative to ρ that they become self-defeating by giving rise to a situation in which even normal fluctuations in earnings may force the company into bankruptcy. The difficulty of borrowing more, therefore, tends to show up in the usual case not so much in higher rates as in the form of increasingly stringent restrictions imposed on the company's management and finances by the creditors; and ultimately in a complete inability to obtain new borrowed funds, at least from the institutional investors who normally set the standards in the market for bonds.

sources—must be the same for all firms in a given class.[18] In other words, the increased cost of borrowed funds as leverage increases will tend to be offset by a corresponding reduction in the yield of common stock. This seemingly paradoxical result will be examined more closely below in connection with Proposition II.

A significant modification of Proposition I would be required only if the yield curve $r=r(D/S)$ were different for different borrowers, as might happen if creditors had marked preferences for the securities of a particular class of debtors. If, for example, corporations as a class were able to borrow at lower rates than individuals having equivalent personal leverage, then the average cost of capital to corporations might fall slightly, as leverage increased over some range, in reflection of this differential. In evaluating this possibility, however, remember that the relevant interest rate for our arbitrage operators is the rate on brokers' loans and, historically, that rate has not been noticeably higher than representative corporate rates.[19] The operations of holding companies and investment trusts which can borrow on terms comparable to operating companies represent still another force which could be expected to wipe out any marked or prolonged advantages from holding levered stocks.[20]

Although Proposition I remains unaffected as long as the yield curve is the same for all borrowers, the relation between common stock yields and leverage will no longer be the strictly linear one given by the original Proposition II. If r increases with leverage, the yield i will still tend to

[18] One normally minor qualification might be noted. Once we relax the assumption that all bonds have certain yields, our arbitrage operator faces the danger of something comparable to "gambler's ruin." That is, there is always the possibility that an otherwise sound concern— one whose long-run expected income is greater than its interest liability—might be forced into liquidation as a result of a run of temporary losses. Since reorganization generally involves costs, and because the operation of the firm may be hampered during the period of reorganization with lasting unfavorable effects on earnings prospects, we might perhaps expect heavily levered companies to sell at a slight discount relative to less heavily indebted companies of the same class.

[19] Under normal conditions, moreover, a substantial part of the arbitrage process could be expected to take the form, not of having the arbitrage operators go into debt on personal account to put the required leverage into their portfolios, but simply of having them reduce the amount of corporate bonds they already hold when they acquire underpriced unlevered stock. Margin requirements are also somewhat less of an obstacle to maintaining any desired degree of leverage in a portfolio than might be thought at first glance. Leverage could be largely restored in the face of higher margin requirements by switching to stocks having more leverage at the corporate level.

[20] An extreme form of inequality between borrowing and lending rates occurs, of course, in the case of preferred stocks, which can not be directly issued by individuals on personal account. Here again, however, we would expect that the operations of investment corporations plus the ability of arbitrage operators to sell off their holdings of preferred stocks would act to prevent the emergence of any substantial premiums (for this reason) on capital structures containing preferred stocks. Nor are preferred stocks so far removed from bonds as to make it impossible for arbitrage operators to approximate closely the risk and leverage of a corporate preferred stock by incurring a somewhat smaller debt on personal account.

rise as D/S increases, but at a decreasing rather than a constant rate. Beyond some high level of leverage, depending on the exact form of the interest function, the yield may even start to fall.[21] The relation between i and D/S could conceivably take the form indicated by the curve MD
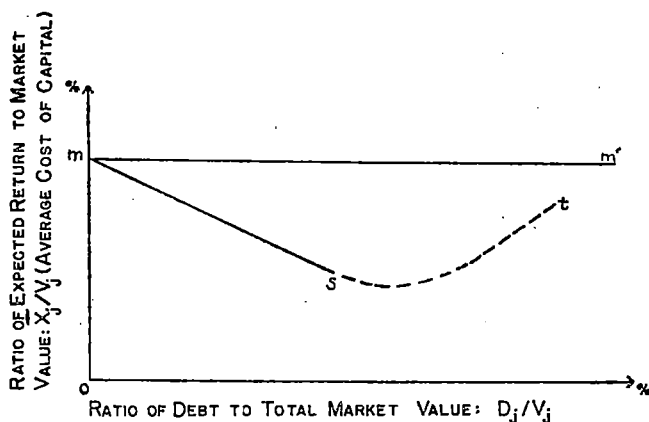
FIGURE 1
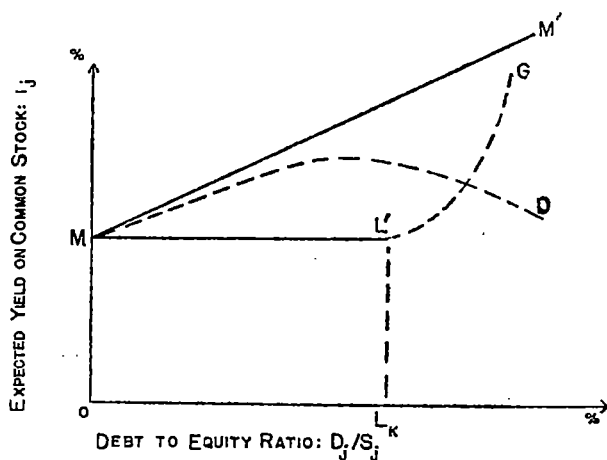
FIGURE 2

in Figure 2, although in practice the curvature would be much less pronounced. By contrast, with a constant rate of interest, the relation would be linear throughout as shown by line MM', Figure 2.

The downward sloping part of the curve MD perhaps requires some

[21] Since new lenders are unlikely to permit this much leverage (cf. note 17), this range of the curve is likely to be occupied by companies whose earnings prospects have fallen substantially since the time when their debts were issued.

comment since it may be hard to imagine why investors, other than those who like lotteries, would purchase stocks in this range. Remember, however, that the yield curve of Proposition II is a consequence of the more fundamental Proposition I. Should the demand by the risk-lovers prove insufficient to keep the market to the peculiar yield-curve MD, this demand would be reinforced by the action of arbitrage operators. The latter would find it profitable to own a pro-rata share of the firm as a whole by holding its stock *and* bonds, the lower yield of the shares being thus offset by the higher return on bonds.

D. *The Relation of Propositions I and II to Current Doctrines*

The propositions we have developed with respect to the valuation of firms and shares appear to be substantially at variance with current doctrines in the field of finance. The main differences between our view and the current view are summarized graphically in Figures 1 and 2. Our Proposition I [equation (4)] asserts that the average cost of capital, $\overline{X}_j{}^r/V_j$, is a constant for all firms j in class k, independently of their financial structure. This implies that, if we were to take a sample of firms in a given class, and if for each firm we were to plot the ratio of expected return to market value against some measure of leverage or financial structure, the points would tend to fall on a horizontal straight line with intercept $\rho_k{}^r$, like the solid line mm' in Figure 1.[22] From Proposition I we derived Proposition II [equation (8)] which, taking the simplest version with r constant, asserts that, for all firms in a class, the relation between the yield on common stock and financial structure, measured by D_j/S_j, will approximate a straight line with slope $(\rho_k{}^r-r)$ and intercept $\rho_k{}^r$. This relationship is shown as the solid line MM' in Figure 2, to which reference has been made earlier.[23]

By contrast, the conventional view among finance specialists appears to start from the proposition that, other things equal, the earnings-price ratio (or its reciprocal, the times-earnings multiplier) of a firm's common stock will normally be only slightly affected by "moderate" amounts of debt in the firm's capital structure.[24] Translated into our no-

[22] In Figure 1 the measure of leverage used is D_j/V_j (the ratio of debt to market value) rather than D_j/S_j (the ratio of debt to equity), the concept used in the analytical development. The D_j/V_j measure is introduced at this point because it simplifies comparison and contrast of our view with the traditional position.

[23] The line MM' in Figure 2 has been drawn with a positive slope on the assumption that $\rho_k{}^r>r$, a condition which will normally obtain. Our Proposition II as given in equation (8) would continue to be valid, of course, even in the unlikely event that $\rho_k{}^r<r$, but the slope of MM' would be negative.

[24] See, *e.g.*, Graham and Dodd [6, pp. 464–66]. Without doing violence to this position, we can bring out its implications more sharply by ignoring the qualification and treating the yield as a virtual constant over the relevant range. See in this connection the discussion in Durand [3, esp. pp. 225–37] of what he calls the "net income method" of valuation.

tation, it asserts that for any firm j in the class k,

(13) $$\frac{\overline{X}_j{}^\tau - rD_j}{S_j} \equiv \frac{\bar{\pi}_j{}^\tau}{S_j} = i_k{}^*, \text{ a constant for } \frac{D_j}{S_j} \leq L_k$$

or, equivalently,

(14) $$S_j = \bar{\pi}_j{}^\tau / i_k{}^*.$$

Here $i_k{}^*$ represents the capitalization rate or earnings-price ratio on the common stock and L_k denotes some amount of leverage regarded as the maximum "reasonable" amount for firms of the class k. This assumed relationship between yield and leverage is the horizontal solid line ML' of Figure 2. Beyond L', the yield will presumably rise sharply as the market discounts "excessive" trading on the equity. This possibility of a rising range for high leverages is indicated by the broken-line segment $L'G$ in the figure.[25]

If the value of shares were really given by (14) then the over-all market value of the firm must be:

(16) $$V_j \equiv S_j + D_j = \frac{\overline{X}_j{}^\tau - rD_j}{i_k{}^*} + D_j = \frac{\overline{X}_j{}^\tau}{i_k{}^*} + \frac{(i_k{}^* - r)D_j}{i_k{}^*}.$$

That is, for any given level of expected total returns after taxes $(\overline{X}_j{}^\tau)$ and assuming, as seems natural, that $i_k{}^* > r$, the value of the firm must tend to *rise* with debt;[26] whereas our Proposition I asserts that the value of the firm is completely independent of the capital structure. Another way of contrasting our position with the traditional one is in terms of the cost of capital. Solving (16) for $\overline{X}_j{}^\tau / V_j$ yields:

(17) $$\overline{X}_j{}^\tau / V_j = i_k{}^* - (i_k{}^* - r)D_j / V_j.$$

According to this equation, the average cost of capital is not independent of capital structure as we have argued, but should tend to *fall* with increasing leverage, at least within the relevant range of moderate debt ratios, as shown by the line ms in Figure 1. Or to put it in more familiar terms, debt-financing should be "cheaper" than equity-financing if not carried too far.

When we also allow for the possibility of a rising range of stock yields for large values of leverage, we obtain a U-shaped curve like mst in

[25] To make it easier to see some of the implications of this hypothesis as well as to prepare the ground for later statistical testing, it will be helpful to assume that the notion of a critical limit on leverage beyond which yields rise rapidly, can be epitomized by a quadratic relation of the form:

(15) $$\bar{\pi}_j{}^\tau / S_j = i_k{}^* + \beta(D_j / S_j) + \alpha(D_j / S_j)^2, \quad \alpha > 0.$$

[26] For a typical discussion of how a promoter can, supposedly, increase the market value of a firm by recourse to debt issues, see W. J. Eiteman [4, esp. pp. 11–13].

Figure 1.[27] That a yield-curve for stocks of the form $ML'G$ in Figure 2 implies a U-shaped cost-of-capital curve has, of course, been recognized by many writers. A natural further step has been to suggest that the capital structure corresponding to the trough of the U is an "optimal capital structure" towards which management ought to strive in the best interests of the stockholders.[28] According to our model, by contrast, no such optimal structure exists—all structures being equivalent from the point of view of the cost of capital.

Although the falling, or at least U-shaped, cost-of-capital function is in one form or another the dominant view in the literature, the ultimate rationale of that view is by no means clear. The crucial element in the position—that the expected earnings-price ratio of the stock is largely unaffected by leverage up to some conventional limit—is rarely even regarded as something which requires explanation. It is usually simply taken for granted or it is merely asserted that this is the way the market behaves.[29] To the extent that the constant earnings-price ratio has a rationale at all we suspect that it reflects in most cases the feeling that moderate amounts of debt in "sound" corporations do not really add very much to the "riskiness" of the stock. Since the extra risk is slight, it seems natural to suppose that firms will not have to pay noticeably higher yields in order to induce investors to hold the stock.[30]

A more sophisticated line of argument has been advanced by David Durand [3, pp. 231–33]. He suggests that because insurance companies and certain other important institutional investors are restricted to debt securities, nonfinancial corporations are able to borrow from them at interest rates which are lower than would be required to compensate

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[27] The U-shaped nature of the cost-of-capital curve can be exhibited explicitly if the yield curve for shares as a function of leverage can be approximated by equation (15) of footnote 25. From that equation, multiplying both sides by S_j we obtain: $\bar{\pi}_j{}^r = \bar{X}_j{}^r - r D_j = i_k{}^* S_j + \beta D_j + \alpha D_j{}^2 / S_j$ or, adding and subtracting $i_k{}^* D_k$ from the right-hand side and collecting terms,

$$(18) \qquad \bar{X}_j{}^r = i_k{}^*(S_j + D_j) + (\beta + r - i_k{}^*)D_j + \alpha D^2{}_j / S_j.$$

Dividing (18) by V_j gives an expression for the cost of capital:

$$(19) \quad \bar{X}_j{}^r / V_j = i_k{}^* - (i_k{}^* - r - \beta)D_j / V_j + \alpha D_j{}^2 / S_j V_j = i_k{}^* - (i_k{}^* - r - \beta)D_j / V_j + \alpha(D_j / V_j)^2 / (1 - D_j / V_j)$$

which is clearly U-shaped since α is supposed to be positive.

[28] For a typical statement see S. M. Robbins [16, p. 307]. See also Graham and Dodd [6, pp. 468–74].

[29] See e.g., Graham and Dodd [6, p. 466].

[30] A typical statement is the following by Guthmann and Dougall [7, p. 245]: "Theoretically it might be argued that the increased hazard from using bonds and preferred stocks would counterbalance this additional income and so prevent the common stock from being more attractive than when it had a lower return but fewer prior obligations. In practice, the extra earnings from 'trading on the equity' are often regarded by investors as more than sufficient to serve as a 'premium for risk' when the proportions of the several securities are judiciously mixed."

creditors in a free market. Thus, while he would presumably agree with
our conclusions that stockholders could not gain from leverage in an un-
constrained market, he concludes that they can gain under present insti-
tutional arrangements. This gain would arise by virtue of the "safety
superpremium" which lenders are willing to pay corporations for the
privilege of lending.[31]

The defective link in both the traditional and the Durand version of
the argument lies in the confusion between investors' subjective risk
preferences and their objective market opportunities. Our Propositions
I and II, as noted earlier, do not depend for their validity on any as-
sumption about individual risk preferences. Nor do they involve any as-
sertion as to what is an adequate compensation to investors for assum-
ing a given degree of risk. They rely merely on the fact that a given
commodity cannot consistently sell at more than one price in the mar-
ket; or more precisely that the price of a commodity representing a
"bundle" of two other commodities cannot be consistently different
from the weighted average of the prices of the two components (the
weights being equal to the proportion of the two commodities in the
bundle).

An analogy may he helpful at this point. The relations between $1/\rho_k$,
the price per dollar of an unlevered stream in class k; $1/r$, the price per
dollar of a sure stream, and $1/i_j$, the price per dollar of a levered stream
j, in the kth class, are essentially the same as those between, respective-
ly, the price of whole milk, the price of butter fat, and the price of milk
which has been thinned out by skimming off some of the butter fat. Our
Proposition I states that a firm cannot reduce the cost of capital—*i.e.*,
increase the market value of the stream it generates—by securing part
of its capital through the sale of bonds, even though debt money ap-
pears to be cheaper. This assertion is equivalent to the proposition that,
under perfect markets, a dairy farmer cannot in general earn more for
the milk he produces by skimming some of the butter fat and selling
it separately, even though butter fat per unit weight, sells for more
than whole milk. The advantage from skimming the milk rather than
selling whole milk would be purely illusory; for what would be gained
from selling the high-priced butter fat would be lost in selling the low-
priced residue of thinned milk. Similarly our Proposition II—that the
price per dollar of a levered stream falls as leverage increases—is an ex-

[31] Like Durand, Morton [15] contends "that the actual market deviates from [Proposition
I] by giving a changing over-all cost of money at different points of the [leverage] scale" (p.
443, note 2, inserts ours), but the basis for this contention is nowhere clearly stated. Judging
by the great emphasis given to the lack of mobility of investment funds between stocks and
bonds and to the psychological and institutional pressures toward debt portfolios (see pp. 444–
51 and especially his discussion of the optimal capital structure on p. 453) he would seem to be
taking a position very similar to that of Durand above.

act analogue of the statement that the price per gallon of thinned milk falls continuously as more butter fat is skimmed off.[32]

It is clear that this last assertion is true as long as butter fat is worth more per unit weight than whole milk, and it holds even if, for many consumers, taking a little cream out of the milk (adding a little leverage to the stock) does not detract noticeably from the taste (does not add noticeably to the risk). Furthermore the argument remains valid even in the face of instituional limitations of the type envisaged by Durand. For suppose that a large fraction of the population habitually dines in restaurants which are required by law to serve only cream in lieu of milk (entrust their savings to institutional investors who can only buy bonds). To be sure the price of butter fat will then tend to be higher in relation to that of skimmed milk than in the absence such restrictions (the rate of interest will tend to be lower), and this will benefit people who eat at home and who like skim milk (who manage their own portfolio and are able and willing to take risk). But it will still be the case that a farmer cannot gain by skimming some of the butter fat and selling it separately (firm cannot reduce the cost of capital by recourse to borrowed funds).[33]

Our propositions can be regarded as the extension of the classical theory of markets to the particular case of the capital markets. Those who hold the current view—whether they realize it or not—must as-

[32] Let M denote the quantity of whole milk, B/M the proportion of butter fat in the whole milk, and let p_M, p_B and p_α denote, respectively, the price per unit weight of whole milk, butter fat and thinned milk from which a fraction α of the butter fat has been skimmed off. We then have the fundamental perfect market relation:

(a) $$p_\alpha(M - \alpha B) + p_B \alpha B = p_M M, \quad 0 \leq \alpha \leq 1,$$

stating that total receipts will be the same amount $p_M M$, independently of the amount αB of butter fat that may have been sold separately. Since p_M corresponds to $1/\rho$, p_B to $1/r$, p_α to $1/i$, M to $\bar{X}$ and αB to rD, (a) is equivalent to Proposition I, $S + D = \bar{X}/\rho$. From (a) we derive:

(b) $$p_\alpha = p_M \frac{M}{M - \alpha B} - p_B \frac{\alpha B}{M - \alpha B}$$

which gives the price of thinned milk as an explicit function of the proportion of butter fat skimmed off; the function decreasing as long as $p_B > p_M$. From (a) also follows:

(c) $$1/p_\alpha = 1/p_M + (1/p_M - 1/p_B) \frac{p_B \alpha B}{p_\alpha(M - \alpha B)}$$

which is the exact analogue of Proposition II, as given by (8).

[33] The reader who likes parables will find that the analogy with interrelated commodity markets can be pushed a good deal farther than we have done in the text. For instance, the effect of changes in the market rate of interest on the over-all cost of capital is the same as the effect of a change in the price of butter on the price of whole milk. Similarly, just as the relation between the prices of skim milk and butter fat influences the kind of cows that will be reared, so the relation between i and r influences the kind of ventures that will be undertaken. If people like butter we shall have Guernseys; if they are willing to pay a high price for safety, this will encourage ventures which promise smaller but less uncertain streams per dollar of physical assets.

sume not merely that there are lags and frictions in the equilibrating .process—a feeling we certainly share,[34] claiming for our propositions only that they describe the central tendency around which observations will scatter—but also that there are large and *systematic* imperfections in the market which permanently bias the outcome. This is an assumption that economists, at any rate, will instinctively eye with some skepticism.

In any event, whether such prolonged, systematic departures from equilibrium really exist or whether our propositions are better descriptions of long-run market behavior can be settled only by empirical research. Before going on to the theory of investment it may be helpful, therefore, to look at the evidence.

E. *Some Preliminary Evidence on the Basic Propositions*

Unfortunately the evidence which has been assembled so far is amazingly skimpy. Indeed, we have been able to locate only two recent studies—and these of rather limited scope—which were designed to throw light on the issue. Pending the results of more comprehensive tests which we hope will soon be available, we shall review briefly such evidence as is provided by the two studies in question: (1) an analysis of the relation between security yields and financial structure for some 43 large electric utilities by F. B. Allen [1], and (2) a parallel (unpublished) study by Robert Smith [19], for 42 oil companies designed to test whether Allen's rather striking results would be found in an industry with very different characteristics.[35] The Allen study is based on average figures for the years 1947 and 1948, while the Smith study relates to the single year 1953.

The Effect of Leverage on the Cost of Capital. According to the received view, as shown in equation (17) the average cost of capital, $\bar{X}^\tau/V$, should decline linearly with leverage as measured by the ratio D/V, at least through most of the relevant range.[36] According to Proposition I, the average cost of capital within a given class k should tend to have the same value ρ_k^τ independently of the degree of leverage. A simple test

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[34] Several specific examples of the failure of the arbitrage mechanism can be found in Graham and Dodd [6, *e.g.*, pp. 646–48]. The price discrepancy described on pp. 646–47 is particularly curious since it persists even today despite the fact that a whole generation of security analysts has been brought up on this book!

[35] We wish to express our thanks to both writers for making available to us some of their original worksheets. In addition to these recent studies there is a frequently cited (but apparently seldom read) study by the Federal Communications Commission in 1938 [22] which purports to show the existence of an optimal capital structure or range of structures (in the sense defined above) for public utilities in the 1930's. By current standards for statistical investigations, however, this study cannot be regarded as having any real evidential value for the problem at hand.

[36] We shall simplify our notation in this section by dropping the subscript j used to denote a particular firm wherever this will not lead to confusion.

of the merits of the two alternative hypotheses can thus be carried out by correlating $\overline{X}^r/V$ with D/V. If the traditional view is correct, the correlation should be significantly negative; if our view represents a better approximation to reality, then the correlation should not be significantly different from zero.

Both studies provide information about the average value of D—the market value of bonds and preferred stock—and of V—the market value of all securities.[37] From these data we can readily compute the ratio D/V and this ratio (expressed as a percentage) is represented by the symbol d in the regression equations below. The measurement of the variable $\overline{X}^r/V$, however, presents serious difficulties. Strictly speaking, the numerator should measure the expected returns net of taxes, but this is a variable on which no direct information is available. As an approximation, we have followed both authors and used (1) the average value of actual net returns in 1947 and 1948 for Allen's utilities; and (2) actual net returns in 1953 for Smith's oil companies. Net return is defined in both cases as the sum of interest, preferred dividends and stockholders' income net of corporate income taxes. Although this approximation to expected returns is undoubtedly very crude, there is no reason to believe that it will systematically bias the test in so far as the sign of the regression coefficient is concerned. The roughness of the approximation, however, will tend to make for a wide scatter. Also contributing to the scatter is the crudeness of the industrial classification, since especially within the sample of oil companies, the assumption that all the firms belong to the same class in our sense, is at best only approximately valid.

Denoting by x our approximation to $\overline{X}^r/V$ (expressed, like d, as a percentage), the results of the tests are as follows:

$$\text{Electric Utilities} \quad x = 5.3 + .006d \quad r = .12$$
$$(\pm .008)$$

$$\text{Oil Companies} \quad x = 8.5 + .006d \quad r = .04.$$
$$(\pm .024)$$

The data underlying these equations are also shown in scatter diagram form in Figures 3 and 4.

The results of these tests are clearly favorable to our hypothesis.

[37] Note that for purposes of this test preferred stocks, since they represent an *expected* fixed obligation, are properly classified with bonds even though the tax status of preferred dividends is different from that of interest payments and even though preferred dividends are really fixed only as to their maximum in any year. Some difficulty of classification does arise in the case of convertible preferred stocks (and convertible bonds) selling at a substantial premium, but fortunately very few such issues were involved for the companies included in the two studies. Smith included bank loans and certain other short-term obligations (at book values) in his data on oil company debts and this treatment is perhaps open to some question. However, the amounts involved were relatively small and check computations showed that their elimination would lead to only minor differences in the test results.
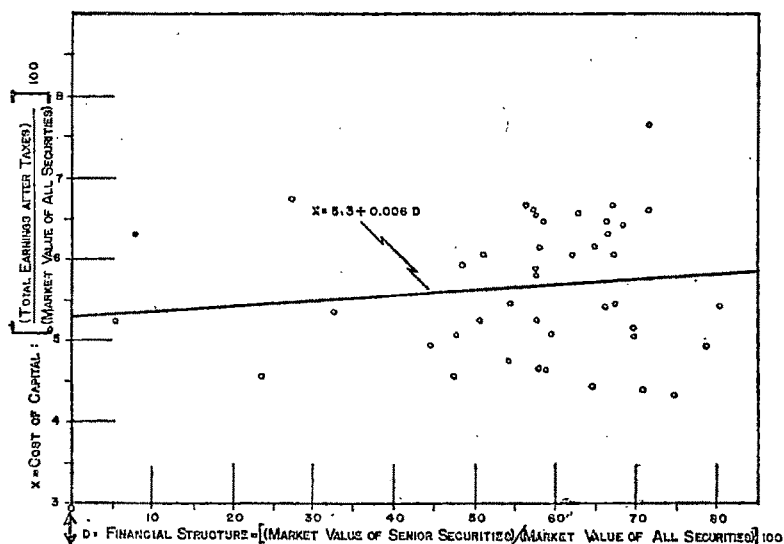
FIGURE 3. COST OF CAPITAL IN RELATION TO FINANCIAL STRUCTURE
FOR 43 ELECTRIC UTILITIES, 1947–48
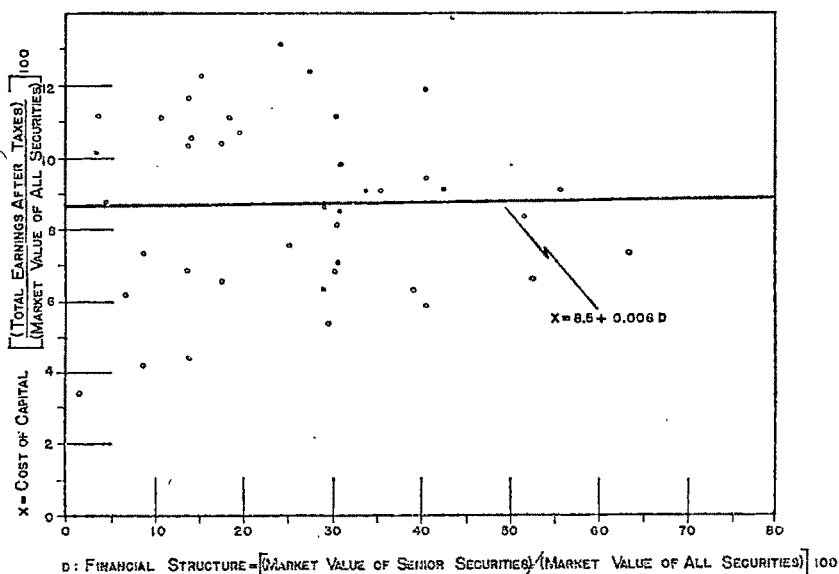
FIGURE 4. COST OF CAPITAL IN RELATION TO FINANCIAL STRUCTURE
FOR 42 OIL COMPANIES, 1953

Both correlation coefficients are very close to zero and not statistically significant. Furthermore, the implications of the traditional view fail to be supported even with respect to the sign of the correlation. The data in short provide no evidence of any tendency for the cost of capital to fall as the debt ratio increases.[38]

It should also be apparent from the scatter diagrams that there is no hint of a curvilinear, U-shaped, relation of the kind which is widely believed to hold between the cost of capital and leverage. This graphical impression was confirmed by statistical tests which showed that for both industries the curvature was not significantly different from zero, its sign actually being opposite to that hypothesized.[39]

Note also that according to our model, the constant terms of the regression equations are measures of $\rho_k{}^r$, the capitalization rates for unlevered streams and hence the average cost of capital in the classes in question. The estimates of 8.5 per cent for the oil companies as against 5.3 per cent for electric utilities appear to accord well with a priori expectations, both in absolute value and relative spread.

The Effect of Leverage on Common Stock Yields. According to our Proposition II—see equation 12 and Figure 2—the expected yield on common stock, $\bar{\pi}^r/S$, in any given class, should tend to increase with leverage as measured by the ratio D/S. The relation should tend to be linear and with positive slope through most of the relevant range (as in the curve MM' of Figure 2), though it might tend to flatten out if we move

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[38] It may be argued that a test of the kind used is biased against the traditional view. The fact that both sides of the regression equation are divided by the variable V which may be subject to random variation might tend to impart a positive bias to the correlation. As a check on the results presented in the text, we have, therefore, carried out a supplementary test based on equation (16). This equation shows that, if the traditional view is correct, the market value of a company should, for given $\bar{X}^r$, increase with debt through most of the relevant range; according to our model the market value should be uncorrelated with D, given $\bar{X}^r$. Because of wide variations in the size of the firms included in our samples, all variables must be divided by a suitable scale factor in order to avoid spurious results in carrying out a test of equation (16). The factor we have used is the book value of the firm denoted by A. The hypothesis tested thus takes the specific form:

$$V/A = a + b(\bar{X}^r/A) + c(D/A)$$

and the numerator of the ratio X^r/A is again approximated by actual net returns. The partial correlation between V/A and D/A should now be positive according to the traditional view and zero according to our model. Although division by A should, if anything, bias the results in favor of the traditional hypothesis, the partial correlation turns out to be only .03 for the oil companies and −.28 for the electric utilities. Neither of these coefficients is significantly different from zero and the larger one even has the wrong sign.

[39] The tests consisted of fitting to the data the equation (19) of footnote 27. As shown there, it follows from the U-shaped hypothesis that the coefficient α of the variable $(D/V)^2$ $/(1-D/V)$, denoted hereafter by d^*, should be significant and positive. The following regression equations and partials were obtained:

Electric Utilities $x = 5.0 + .017d − .003d^*$; $r_{xd^* \cdot d} = − .15$

Oil Companies $\quad x = 8.0 + .05d − .03d^*$; $r_{xd^* \cdot d} = − .14$.

far enough to the right (as in the curve MD'), to the extent that high leverage tends to drive up the cost of senior capital. According to the conventional view, the yield curve as a function of leverage should be a horizontal straight line (like ML') through most of the relevant range; far enough to the right, the yield may tend to rise at an increasing rate. Here again, a straight-forward correlation—in this case between $\bar{\pi}^r/S$ and D/S—can provide a test of the two positions. If our view is correct, the correlation should be significantly positive; if the traditional view is correct, the correlation should be negligible.

Subject to the same qualifications noted above in connection with $\bar{X}^r$, we can approximate $\bar{\pi}^r$ by actual stockholder net income.[40] Letting z denote in each case the approximation to $\bar{\pi}^r/S$ (expressed as a percentage) and letting h denote the ratio D/S (also in percentage terms) the following results are obtained:

Electric Utilities $z = 6.6 + .017h$ $r = .53$
$$(+.004)$$

Oil Companies $z = 8.9 + .051h$ $r = .53.$
$$(\pm.012)$$

These results are shown in scatter diagram form in Figures 5 and 6.

Here again the implications of our analysis seem to be borne out by the data. Both correlation coefficients are positive and highly significant when account is taken of the substantial sample size. Furthermore, the estimates of the coefficients of the equations seem to accord reasonably well with our hypothesis. According to equation (12) the constant term should be the value of ρ_k^r for the given class while the slope should be $(\rho_k^r - r)$. From the test of Proposition I we have seen that for the oil companies the mean value of ρ_k^r could be estimated at around 8.7. Since the average yield of senior capital during the period covered was in the order of $3\frac{1}{2}$ per cent, we should expect a constant term of about 8.7 per cent and a slope of just over 5 per cent. These values closely approximate the regression estimates of 8.9 per cent and 5.1 per cent respectively. For the electric utilities, the yield of senior capital was also on the order of $3\frac{1}{2}$ per cent during the test years, but since the estimate of the mean value of ρ_k^r from the test of Proposition I was 5.6 per cent,

[40] As indicated earlier, Smith's data were for the single year 1953. Since the use of a single year's profits as a measure of expected profits might be open to objection we collected profit data for 1952 for the same companies and based the computation of $\bar{\pi}^r/S$ on the average of the two years. The value of $\bar{\pi}^r/S$ was obtained from the formula:

$$\left(\text{net earnings in 1952} \cdot \frac{\text{assets in '53}}{\text{assets in '52}} + \text{net earnings in '1953} \right) \frac{1}{2}$$
$$\div \text{ (average market value of common stock in '53).}$$

The asset adjustment was introduced as rough allowance for the effects of possible growth in the size of the firm. It might be added that the correlation computed with $\bar{\pi}^r/S$ based on net profits in 1953 alone was found to be only slightly smaller, namely .50.
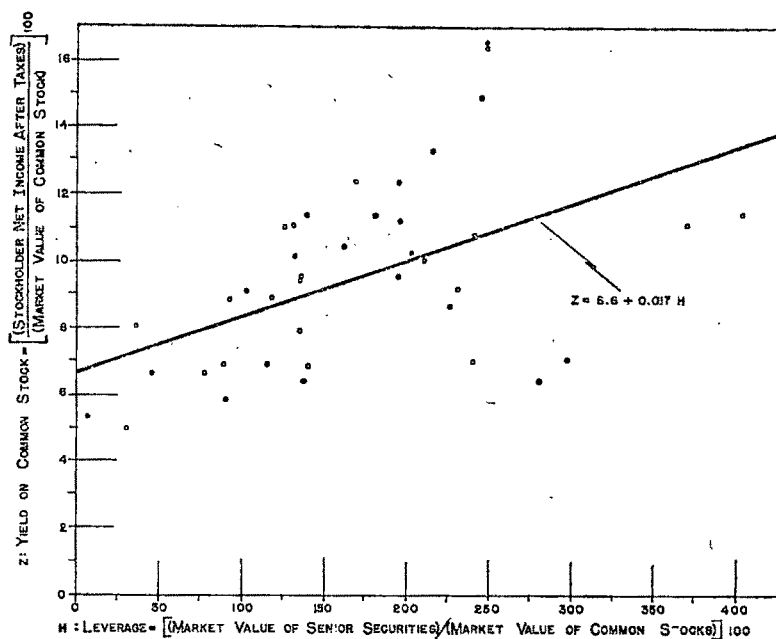
FIGURE 5. YIELD ON COMMON STOCK IN RELATION TO LEVERAGE FOR
43 ELECTRIC UTILITIES, 1947–48
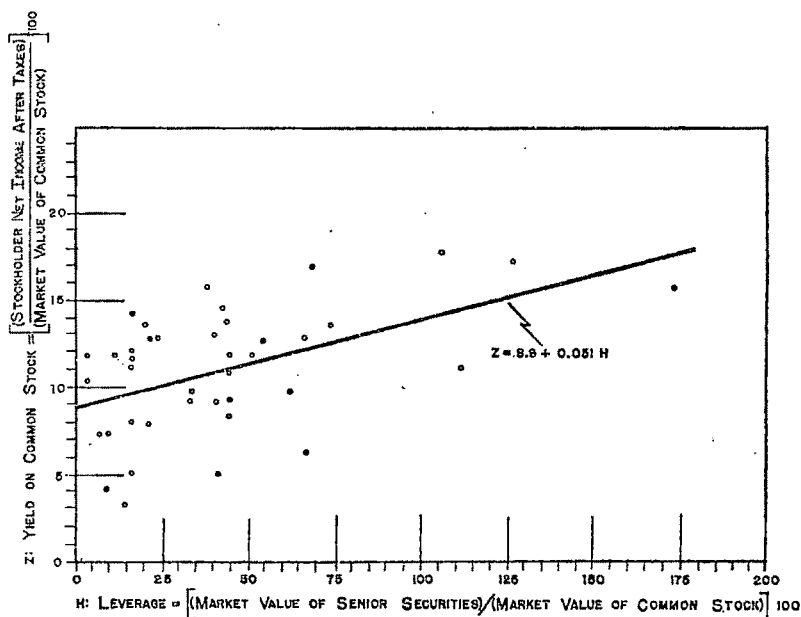
FIGURE 6. YIELD ON COMMON STOCK IN RELATION TO LEVERAGE FOR
42 OIL COMPANIES, 1952–53

the slope should be just above 2 per cent. The actual regression estimate for the slope of 1.7 per cent is thus somewhat low, but still within one standard error of its theoretical value. Because of this underestimate of the slope and because of the large mean value of leverage ($\bar{h} = 160$ per cent) the regression estimate of the constant term, 6.6 per cent, is somewhat high, although not significantly different from the value of 5.6 per cent obtained in the test of Proposition I.

When we add a square term to the above equations to test for the presence and direction of curvature we obtain the following estimates:

$$\text{Electric Utilities} \quad z = 4.6 + .004h - .007h^2$$

$$\text{Oil Companies} \quad z = 8.5 + .072h - .016h^2.$$

For both cases the curvature is negative. In fact, for the electric utilities, where the observations cover a wider range of leverage ratios, the negative coefficient of the square term is actually significant at the 5 per cent level. Negative curvature, as we have seen, runs directly counter to the traditional hypothesis, whereas it can be readily accounted for by our model in terms of rising cost of borrowed funds.[41]

In summary, the empirical evidence we have reviewed seems to be broadly consistent with our model and largely inconsistent with traditional views. Needless to say much more extensive testing will be required before we can firmly conclude that our theory describes market behavior. Caution is indicated especially with regard to our test of Proposition II, partly because of possible statistical pitfalls[42] and partly because not all the factors that might have a systematic effect on stock yields have been considered. In particular, no attempt was made to test the possible influence of the dividend pay-out ratio whose role has tended to receive a great deal of attention in current research and thinking. There are two reasons for this omission. First, our main objective has been to assess the prima facie tenability of *our* model, and in this model, based as it is on rational behavior by investors, dividends per se play no role. Second, in a world in which the policy of dividend stabilization is widespread, there is no simple way of disentangling the true effect of dividend payments on stock prices from their apparent effect,

[41] That the yield of senior capital tended to rise for utilities as leverage increased is clearly shown in several of the scatter diagrams presented in the published version of Allen's study. This significant negative curvature between stock yields and leverage for utilities may be partly responsible for the fact, previously noted, that the constant in the linear regression is somewhat higher and the slope somewhat lower than implied by equation (12). Note also in connection with the estimate of $\rho_k{}^\tau$ that the introduction of the quadratic term reduces the constant considerably, pushing it in fact below the a priori expectation of 5.6, though the difference is again not statistically significant.

[42] In our test, *e.g.*, the two variables z and h are both ratios with S appearing in the denominator, which may tend to impart a positive bias to the correlation (*cf.* note 38). Attempts were made to develop alternative tests, but although various possibilities were explored, we have so far been unable to find satisfactory alternatives.

the latter reflecting only the role of dividends as a proxy measure of long-term earning anticipations.[43] The difficulties just mentioned are further compounded by possible interrelations between dividend policy and leverage.[44]

II. *Implications of the Analysis for the Theory of Investment*

A. *Capital Structure and Investment Policy*

On the basis of our propositions with respect to cost of capital and financial structure (and for the moment neglecting taxes), we can derive the following simple rule for optimal investment policy by the firm:

Proposition III. If a firm in class k is acting in the best interest of the stockholders at the time of the decision, it will exploit an investment opportunity if and only if the rate of return on the investment, say ρ^*, is as large as or larger than ρ_k. That is, *the cut-off point for investment in the firm will in all cases be ρ_k and will be completely unaffected by the type of security used to finance the investment.* Equivalently, we may say that regardless of the financing used, the marginal cost of capital to a firm is equal to the average cost of capital, which is in turn equal to the capitalization rate for an unlevered stream in the class to which the firm belongs.[45]

To establish this result we will consider the three major financing alternatives open to the firm—bonds, retained earnings, and common stock issues—and show that in each case an investment is worth undertaking if, and only if, $\rho^* \geqq \rho_k$.[46]

Consider first the case of an investment financed by the sale of bonds. We know from Proposition I that the market value of the firm before the investment was undertaken was:[47]

$$(20) \qquad\qquad V_0 = \overline{X}_0/\rho_k$$

[43] We suggest that failure to appreciate this difficulty is responsible for many fallacious, or at least unwarranted, conclusions about the role of dividends.

[44] In the sample of electric utilities, there is a substantial negative correlation between yields and pay-out ratios, but also between pay-out ratios and leverage, suggesting that either the association of yields and leverage or of yields and pay-out ratios may be (at least partly) spurious. These difficulties however do not arise in the case of the oil industry sample. A preliminary analysis indicates that there is here no significant relation between leverage and pay-out ratios and also no significant correlation (either gross or partial) between yields and pay-out ratios.

[45] The analysis developed in this paper is essentially a comparative-statics, not a dynamic analysis. This note of caution applies with special force to Proposition III. Such problems as those posed by expected changes in r and in ρ_k over time will not be treated here. Although they are in principle amenable to analysis within the general framework we have laid out, such an undertaking is sufficiently complex to deserve separate treatment. *Cf.* note 17.

[46] The extension of the proof to other types of financing, such as the sale of preferred stock or the issuance of stock rights is straightforward.

[47] Since no confusion is likely to arise, we have again, for simplicity, eliminated the subscripts identifying the firm in the equations to follow. Except for ρ_k, the subscripts now refer to time periods.

and that the value of the common stock was:

(21) $$S_0 = V_0 - D_0.$$

If now the firm borrows I dollars to finance an investment yielding ρ^* its market value will become:

(22) $$V_1 = \frac{\overline{X}_0 + \rho^*I}{\rho_k} = V_0 + \frac{\rho^*I}{\rho_k}$$

and the value of its common stock will be:

(23) $$S_1 = V_1 - (D_0 + I) = V_0 + \frac{\rho^*I}{\rho_k} - D_0 - I$$

or using equation 21,

(24) $$S_1 = S_0 + \frac{\rho^*I}{\rho_k} - I.$$

Hence $S_1 \gtreqless S_0$ as $\rho^* \gtreqless \rho_k.$[48]

To illustrate, suppose the capitalization rate for uncertain streams in the kth class is 10 per cent and the rate of interest is 4 per cent. Then if a given company had an expected income of 1,000 and if it were financed entirely by common stock we know from Proposition I that the market value of its stock would be 10,000. Assume now that the managers of the firm discover an investment opportunity which will require an outlay of 100 and which is expected to yield 8 per cent. At first sight this might appear to be a profitable opportunity since the expected return is double the interest cost. If, however, the management borrows the necessary 100 at 4 per cent, the total expected income of the company rises to 1,008 and the market value of the firm to 10,080. But the firm now will have 100 of bonds in its capital structure so that, paradoxically, the market value of the stock must actually be reduced from 10,000 to 9,980 as a consequence of this apparently profitable investment. Or, to put it another way, the gains from being able to tap cheap, borrowed funds are more than offset for the stockholders by the market's discount-ing of the stock for the added leverage assumed.

Consider next the case of retained earnings. Suppose that in the course of its operations the firm acquired I dollars of cash (without impairing

[48] In the case of bond-financing the rate of interest on bonds does not enter explicitly into the decision (assuming the firm borrows at the market rate of interest). This is true, more-over, given the conditions outlined in Section I.C, even though interest rates may be an increasing function of debt outstanding. To the extent that the firm borrowed at a rate other than the market rate the two I's in equation (24) would no longer be identical and an additional gain or loss, as the case might be, would accrue to the shareholders. It might also be noted in passing that permitting the two I's in (24) to take on different values provides a simple method for introducing underwriting expenses into the analysis.

the earning power of its assets). If the cash is distributed as a dividend to the stockholders their wealth W_0, after the distribution will be:

$$(25) \qquad W_0 = S_0 + I = \frac{\overline{X}_0}{\rho_k} - D_0 + I$$

where $\overline{X}_0$ represents the expected return from the assets exclusive of the amount I in question. If however the funds are retained by the company and used to finance new assets whose expected rate of return is ρ^*, then the stockholders' wealth would become:

$$(26) \qquad W_1 = S_1 = \frac{\overline{X}_0 + \rho^* I}{\rho_k} - D_0 = S_0 + \frac{\rho^* I}{\rho_k}.$$

Clearly $W_1 \gtreqless W_0$ as $\rho^* \gtreqless \rho_k$ so that an investment financed by retained earnings raises the net worth of the owners if and only if $\rho^* > \rho_k$.[49]

Consider finally, the case of common-stock financing. Let P_0 denote the current market price per share of stock and assume, for simplicity, that this price reflects currently expected earnings only, that is, it does not reflect any future increase in earnings as a result of the investment under consideration.[50] Then if N is the original number of shares, the price per share is:

$$(27) \qquad P_0 = S_0/N$$

and the number of new shares, M, needed to finance an investment of I dollars is given by:

$$(28) \qquad M = \frac{I}{P_0}.$$

As a result of the investment the market value of the stock becomes:

$$S_1 = \frac{\overline{X}_0 + \rho^* I}{\rho_k} - D_0 = S_0 + \frac{\rho^* I}{\rho_k} = N P_0 + \frac{\rho^* I}{\rho_k}$$

and the price per share:

$$(29) \qquad P_1 = \frac{S_1}{N + M} = \frac{1}{N + M} \left[N P_0 + \frac{\rho^* I}{\rho_k} \right].$$

[49] The conclusion that ρ_k is the cut-off point for investments financed from internal funds applies not only to undistributed net profits, but to depreciation allowances (and even to the funds represented by the current sale value of any asset or collection of assets). Since the owners can earn ρ_k by investing funds elsewhere in the class, partial or total liquidating distributions should be made whenever the firm cannot achieve a marginal internal rate of return equal to ρ_k.

[50] If we assumed that the market price of the stock did reflect the expected higher future earnings (as would be the case if our original set of assumptions above were strictly followed) the analysis would differ slightly in detail, but not in essentials. The cut-off point for new investment would still be ρ_k, but where $\rho^* > \rho_k$ the gain to the original owners would be larger than if the stock price were based on the pre-investment expectations only.

Since by equation (28), $I = MP_0$, we can add MP_0 and subtract I from the quantity in bracket, obtaining:

$$P_1 = \frac{1}{N + M}\left[(N + M)P_0 + \frac{\rho^* - \rho_k}{\rho_k}I\right]$$

(30)

$$= P_0 + \frac{1}{N + M}\frac{\rho^* - \rho_k}{\rho_k}I > P_0 \text{ if,}$$

and only if, $\rho^* > \rho_k$.

Thus an investment financed by common stock is advantageous to the current stockholders if and only if its yield exceeds the capitalization rate ρ_k.

Once again a numerical example may help to illustrate the result and make it clear why the relevant cut-off rate is ρ_k and not the current yield on common stock, i. Suppose that ρ_k is 10 per cent, r is 4 per cent, that the original expected income of our company is 1,000 and that management has the opportunity of investing 100 having an expected yield of 12 per cent. If the original capital structure is 50 per cent debt and 50 per cent equity, and 1,000 shares of stock are initially outstanding, then, by Proposition I, the market value of the common stock must be 5,000 or 5 per share. Furthermore, since the interest bill is $.04 \times 5,000 = 200$, the yield on common stock is $800/5,000 = 16$ per cent. It may then appear that financing the additional investment of 100 by issuing 20 shares to outsiders at 5 per share would dilute the equity of the original owners since the 100 promises to yield 12 per cent whereas the common stock is currently yielding 16 per cent. Actually, however, the income of the company would rise to 1,012; the value of the firm to 10,120; and the value of the common stock to 5,120. Since there are now 1,020 shares, each would be worth 5.02 and the wealth of the original stockholders would thus have been increased. What has happened is that the dilution in expected earnings per share (from .80 to .796) has been more than offset, in its effect upon the market price of the shares, by the decrease in leverage.

Our conclusion is, once again, at variance with conventional views,[51] so much so as to be easily misinterpreted. Read hastily, Proposition III seems to imply that the capital structure of a firm is a matter of indifference; and that, consequently, one of the core problems of corporate finance—the problem of the optimal capital structure for a firm—is no problem at all. It may be helpful, therefore, to clear up such possible misundertandings.

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[51] In the matter of investment policy under uncertainty there is no single position which represents "accepted" doctrine. For a sample of current formulations, all very different from ours, see Joel Dean [2, esp. Ch. 3], M. Gordon and E. Shapiro [5], and Harry Roberts [17].

B. *Proposition III and Financial Planning by Firms*

Misinterpretation of the scope of Proposition III can be avoided by remembering that this Proposition tells us only that the type of instrument used to finance an investment is irrelevant to the question of whether or not the investment is worth while. This does not mean that the owners (or the managers) have no grounds whatever for preferring one financing plan to another; or that there are no other policy or technical issues in finance at the level of the firm.

That grounds for preferring one type of financial structure to another will still exist within the framework of our model can readily be seen for the case of common-stock financing. In general, except for something like a widely publicized oil-strike, we would expect the market to place very heavy weight on current and recent past earnings in forming expectations as to future returns. Hence, if the owners of a firm discovered a major investment opportunity which they felt would yield much more than ρ_k, they might well prefer not to finance it via common stock at the then ruling price, because this price may fail to capitalize the new venture. A better course would be a pre-emptive issue of stock (and in this connection it should be remembered that stockholders are free to borrow and buy). Another possibility would be to finance the project initially with debt. Once the project had reflected itself in increased actual earnings, the debt could be retired either with an equity issue at much better prices or through retained earnings. Still another possibility along the same lines might be to combine the two steps by means of a convertible debenture or preferred stock, perhaps with a progressively declining conversion rate. Even such a double-stage financing plan may possibly be regarded as yielding too large a share to outsiders since the new stockholders are, in effect, being given an interest in any similar opportunities the firm may discover in the future. If there is a reasonable prospect that even larger opportunities may arise in the near future and if there is some danger that borrowing now would preclude more borrowing later, the owners might find their interests best protected by splitting off the current opportunity into a separate subsidiary with independent financing. Clearly the problems involved in making the crucial estimates and in planning the optimal financial strategy are by no means trivial, even though they should have no bearing on the basic decision to invest (as long as $\rho^* \geq \rho_k$).[52]

Another reason why the alternatives in financial plans may not be a matter of indifference arises from the fact that managers are concerned ·

[52] Nor can we rule out the possibility that the existing owners, if unable to use a financing plan which protects their interest, may actually prefer to pass up an otherwise profitable venture rather than give outsiders an "excessive" share of the business. It is presumably in situations of this kind that we could justifiably speak of a shortage of "equity capital," though this kind of market imperfection is likely to be of significance only for small or new firms.

with more than simply furthering the interest of the owners. Such other objectives of the management—which need not be necessarily in conflict with those of the owners—are much more likely to be served by some types of financing arrangements than others. In many forms of borrowing agreements, for example, creditors are able to stipulate terms which the current management may regard as infringing on its prerogatives or restricting its freedom to maneuver. The creditors might even be able to insist on having a direct voice in the formation of policy.[53] To the extent, therefore, that financial policies have these implications for the management of the firm, something like the utility approach described in the introductory section becomes relevant to financial (as opposed to investment) decision-making. It is, however, the utility functions of the managers per se and not of the owners that are now involved.[54]

In summary, many of the specific considerations which bulk so large in traditional discussions of corporate finance can readily be superimposed on our simple framework without forcing any drastic (and certainly no systematic) alteration of the conclusion which is our principal concern, namely that for investment decisions, the marginal cost of capital is ρ_k.

C. The Effect of the Corporate Income Tax on Investment Decisions

In Section I it was shown that when an unintegrated corporate income tax is introduced, the original version of our Proposition I,

$$\overline{X}/V = \rho_k = \text{a constant}$$

must be rewritten as:

$$(11) \qquad \frac{(\overline{X} - rD)(1 - \tau) + rD}{V} \equiv \frac{\overline{X}^\tau}{V} = \rho_k^\tau = \text{a constant}.$$

Throughout Section I we found it convenient to refer to $\overline{X}^\tau/V$ as the cost of capital. The appropriate measure of the cost of capital relevant

[53] Similar considerations are involved in the matter of dividend policy. Even though the stockholders may be indifferent as to payout policy as long as investment policy is optimal, the management need not be so. Retained earnings involve far fewer threats to control than any of the alternative sources of funds and, of course, involve no underwriting expense or risk. But against these advantages management must balance the fact that sharp changes in dividend rates, which heavy reliance on retained earnings might imply, may give the impression that a firm's finances are being poorly managed, with consequent threats to the control and professional standing of the management.

[54] In principle, at least, this introduction of management's risk preferences with respect to financing methods would do much to reconcile the apparent conflict between Proposition III and such empirical findings as those of Modigliani and Zeman [14] on the close relation between interest rates and the ratio of new debt to new equity issues; or of John Lintner [12] on the considerable stability in target and actual dividend-payout ratios.

to investment decisions, however, is the ratio of the expected return *before* taxes to the market value, *i.e.*, $\overline{X}/V$. From (11) above we find:

$$(31) \qquad \frac{\overline{X}}{V} = \frac{\rho_k{}^\tau - \tau_r(D/V)}{1 - \tau} = \frac{\rho_k{}^\tau}{1 - \tau}\left[1 - \frac{\tau r D}{\rho_k{}^\tau V}\right],$$

which shows that the cost of capital now depends on the debt ratio, decreasing, as D/V rises, at the constant rate $\tau r/(1-\tau)$.[55] Thus, with a corporate income tax under which interest is a deductible expense, gains can accrue to stockholders from having debt in the capital structure, even when capital markets are perfect. The gains however are small, as can be seen from (31), and as will be shown more explicitly below.

From (31) we can develop the tax-adjusted counterpart of Proposition III by interpreting the term D/V in that equation as the proportion of debt used in any additional financing of V dollars. For example, in the case where the financing is entirely by new common stock, $D=0$ and the required rate of return $\rho_k{}^S$ on a venture so financed becomes:

$$(32) \qquad \rho_k{}^S = \frac{\rho_k{}^\tau}{1 - \tau}.$$

For the other extreme of pure debt financing $D=V$ and the required rate of return, $\rho_k{}^D$, becomes:

$$(33) \qquad \rho_k{}^D = \frac{\rho_k{}^\tau}{1 - \tau}\left[1 - \tau \frac{r}{\rho_k{}^\tau}\right] = \rho_k{}^S\left[1 - \tau \frac{r}{\rho_k{}^\tau}\right] = \rho_k{}^S - \frac{\tau}{1 - \tau} r.[56]$$

For investments financed out of retained earnings, the problem of defining the required rate of return is more difficult since it involves a comparison of the tax consequences to the individual stockholder of receiving a dividend versus having a capital gain. Depending on the time of realization, a capital gain produced by retained earnings may be taxed either at ordinary income tax rates, 50 per cent of these rates, 25 per

[55] Equation (31) is amenable, in principle, to statistical tests similar to those described in Section I.E. However we have not made any systematic attempt to carry out such tests so far, because neither the Allen nor the Smith study provides the required information. Actually, Smith's data included a very crude estimate of tax liability, and, using this estimate, we did in fact obtain a negative relation between $\overline{X}/V$ and D/V. However, the correlation $(-.28)$ turned out to be significant only at about the 10 per cent level. While this result is not conclusive, it should be remembered that, according to our theory, the slope of the regression equation should be in any event quite small. In fact, with a value of τ in the order of .5, and values of $\rho_k{}^\tau$ and r in the order of 8.5 and 3.5 per cent respectively (*cf.* Section I.E) an increase in D/V from 0 to 60 per cent (which is, approximately, the range of variation of this variable in the sample) should tend to reduce the average cost of capital only from about 17 to about 15 per cent.

[56] This conclusion does not extend to preferred stocks even though they have been classed with debt issues previously. Since preferred dividends except for a portion of those of public utilities are not in general deductible from the corporate tax, the cut-off point for new financing via preferred stock is exactly the same as that for common stock.

cent, or zero, if held till death. The rate on any dividends received in the event of a distribution will also be a variable depending on the amount of other income received by the stockholder, and with the added complications introduced by the current dividend-credit provisions. If we assume that the managers proceed on the basis of reasonable estimates as to the average values of the relevant tax rates for the owners, then the required return for retained earnings ρ_k^R can be shown to be:

$$(34) \qquad \rho_k^R = \rho_k^\tau \frac{1}{1 - \tau} \frac{1 - \tau_d}{1 - \tau_g} = \frac{1 - \tau_d}{1 - \tau_g} \rho_k^s$$

where τ_d is the assumed rate of personal income tax on dividends and τ_g is the assumed rate of tax on capital gains.

A numerical illustration may perhaps be helpful in clarifying the relationship between these required rates of return. If we take the following round numbers as representative order-of-magnitude values under present conditions: an after-tax capitalization rate ρ_k^τ of 10 per cent, a rate of interest on bonds of 4 per cent, a corporate tax rate of 50 per cent, a marginal personal income tax rate on dividends of 40 per cent (corresponding to an income of about $25,000 on a joint return), and a capital gains rate of 20 per cent (one-half the marginal rate on dividends), then the required rates of return would be: (1) 20 per cent for investments financed entirely by issuance of new common shares; (2) 16 per cent for investments financed entirely by new debt; and (3) 15 per cent for investments financed wholly from internal funds.

These results would seem to have considerable significance for current discussions of the effect of the corporate income tax on financial policy and on investment. Although we cannot explore the implications of the results in any detail here, we should at least like to call attention to the remarkably small difference between the "cost" of equity funds and debt funds. With the numerical values assumed, equity money turned out to be only 25 per cent more expensive than debt money, rather than something on the order of 5 times as expensive as is commonly supposed to be the case.[57] The reason for the wide difference is that the traditional

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[57] See e.g., D. T. Smith [18]. It should also be pointed out that our tax system acts in other ways to reduce the gains from debt financing. Heavy reliance on debt in the capital structure, for example, commits a company to paying out a substantial proportion of its income in the form of interest payments taxable to the owners under the personal income tax. A debt-free company, by contrast, can reinvest in the business all of its (smaller) net income and to this extent subject the owners only to the low capital gains rate (or possibly no tax at all by virtue of the loophole at death). Thus, we should expect a high degree of leverage to be of value to the owners, even in the case of closely held corporations, primarily in cases where their firm was not expected to have much need for additional funds to expand assets and earnings in the future. To the extent that opportunities for growth were available, as they presumably would be for most successful corporations, the interest of the stockholders would tend to be better served by a structure which permitted maximum use of retained earnings.

view starts from the position that debt funds are several times cheaper than equity funds even in the absence of taxes, with taxes serving simply to magnify the cost ratio in proportion to the corporate rate. By contrast, in our model in which the repercussions of debt financing on the value of shares are taken into account, the *only* difference in cost is that due to the tax effect, and its magnitude is simply the tax on the "grossed up" interest payment. Not only is this magnitude likely to be small but our analysis yields the further paradoxical implication that the stockholders' gain from, and hence incentive to use, debt financing is actually smaller the lower the rate of interest. In the extreme case where the firm could borrow for practically nothing, the advantage of debt financing would also be practically nothing.

III. *Conclusion*

With the development of Proposition III the main objectives we outlined in our introductory discussion have been reached. We have in our Propositions I and II at least the foundations of a theory of the valuation of firms and shares in a world of uncertainty. We have shown, moreover, how this theory can lead to an operational definition of the cost of capital and how that concept can be used in turn as a basis for rational investment decision-making within the firm. Needless to say, however, much remains to be done before the cost of capital can be put away on the shelf among the solved problems. Our approach has been that of static, partial equilibrium analysis. It has assumed among other things a state of atomistic competition in the capital markets and an ease of access to those markets which only a relatively small (though important) group of firms even come close to possessing. These and other drastic simplifications have been necessary in order to come to grips with the problem at all. Having served their purpose they can now be relaxed in the direction of greater realism and relevance, a task in which we hope others interested in this area will wish to share.

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POLICY ANALYSIS

By CHARLES E. LINDBLOM*

Where quantitative methods in economics are explicit, many other analytical methods are not. And lacking explicit statement, some are not even recognized to be distinctive methods. To be sure, everyone is familiar with paradigms of the scientific method, especially those describing the construction and use of theory; and of late increasing attention has been given to concept formation.[1] But many kinds of analysis still remain to be formalized, among which public policy analysis is one.

In seeking to formalize some of our methods of policy analysis, this paper is one of a growing family of ventures into clarification of non-quantitative and largely nontheoretical methods.[2] One noteworthy characteristic of these studies is that they are not argumentative: they do not urge this or that method upon social scientists; they merely make explicit and formalize the methods already in use.

In the analysis of public policy, economists will quickly recognize the following three (among many) characteristics of common procedures:

(1) A body of theory is applied to the particular situation in which a policy problem arises. (2) A comprehensive overview of all important variables is attempted by the analyst; or, if any important variable is neglected in the theory, the results are qualified to take it into account, or the user of the results is warned. (3) Postulated values specify criteria by which alternative policies are to be judged; or, if they are not simply postulated, their derivation is in any case a process separate from the purely scientific or positive analysis of variables. A fourth

[1] For example, C. G. Hempel, "Fundamentals of Concept Formation in Empirical Science," *International Encyclopedia of Unified Science*, Vol. II, No. 7, Chicago 1952.

[2] Related studies include, among others that might be mentioned: P. F. Lazarsfeld and A. H. Barton, "Qualitative Measurement in the Social Sciences," in Daniel Lerner and H. D. Lasswell, eds., *The Policy Sciences*, Stanford 1951, Ch. 9; P. F. Lazarsfeld and R. K. Merton, "Friendship as Social Process: A Substantive and Methodological Analysis," in Monroe Berger, Theodore Abel, and C. H. Page, eds., *Freedom and Control in Modern Society*, New York 1954, Ch. 2; and Abraham Kaplan, "Definition and Specification of Meaning," *Jour. Philosophy*, May 23, 1946, XLIII, 281-88.

characteristic, though less frequently encountered, is nevertheless also common: (4) A presumption in favor of a particular kind of policy in a particular problem situation is derived from the general theoretical argument for that kind of policy treated as a general rule. For example, the general case for the price system and against direct controls creates, for those who accept it, a presumption (but only a presumption) for the use of the price system in particular policy-problem areas; or the general case for "planning," for those who accept it, creates a presumption for "planning" in a particular situation.

It should be noted that the first two listed refer to the handling of empirical material; the last two, to the handling of values. For simplicity, let me refer to these four procedures taken together as the "conventional" method of policy analysis, without implying anything about its correctness, frequency of use or academic respectability.

Now contrast to this familiar method a second which departs from—even negates—the first on each of the four characteristics. In the second method is found: (1) Relatively less reliance on theory. (2) A partial or fragmented view of the important variables. (3) A close intertwining of the search for values and the search for facts. (4) No policy presumption of the kind employed in the conventional method. In frequency of use, the second is quite as commonplace as the first and in its proper place no less accurate; but it cannot be described in the conventional terms we employ to describe scientific work. It is all the more interesting as a method because our first impulse is to look askance at it, believing that the merit of each of the four characteristics of the conventional method cannot be denied.

The second method is the subject of this paper. I shall try to make its processes explicit and display its contrasts with the conventional method by considering in turn each of the four points of difference.

I. Theory and Alternatives to It

Just how the second method can and does proceed with a minimum of theory, and just what the second method consists of are best explained by reference to the limitations of theory, even at the risk of appearing to digress. For all the richness of economic theory, we realize that it embraces a limited number of variables and contains propositions about a limited number of possible situations. Much of our analysis carries us far beyond theory, and for some of our analysis theory is largely irrelevant. By observing our own practices, we know this to be true; and the point would not be worth dwelling on were it not for our common tendency to speak of theory and analysis as synonymous, as when a reader of an earlier draft of this paper claimed that analysis is "just another word for the use of theory."

To avoid quibbling over words, let me say that we often work out problems, organize our thoughts, find answers to questions, and come to conclusions without the aid of theory; and for that matter in most of the problem areas of the social sciences, no theory exists. In these senses, analysis and theory are not identical (and it is reasonable to ask what methods we employ for analysis other than theory).

Quite aside from *evaluation* of alternative policies, to which we turn later, the mere prediction or estimate of empirical consequences of alternative policies is often beyond our theoretical competence. Many of us would believe, for example, that while we could employ theory to predict that an economy without money and prices would in certain specified ways misallocate resources, we cannot rely heavily on theory in predicting that recent business mergers will or will not increase the extent of resource misallocation. While we rely on theoretical analysis to lead us to the conclusion that wartime government spending will produce inflation, it helps us only part way when we try to determine the relative merits of direct and indirect anti-inflation controls, as the extent of disagreement among economists and the content of their debate prove. In citing these examples, I am not trying to attack theory or pick a quarrel with it; I am only pointing out, as a basis for further clarification of method, something I assume we all agree on: specifically, that we commonly go in analysis where theory cannot take us. Something else is often, perhaps always, required.

Now one reason that we are forced to analyze variables outside the embrace of theory and establish propositions that cannot be found within our body of theory is that policy alternatives sometimes differ by degrees too fine for our theory to distinguish. If with theory one can contrast consequences of perfect and imperfect competition, one is nevertheless often unable to predict the different consequences of several alternative forms of competition all of which are imperfect. Yet policy must choose among forms of imperfect competition. Theory is often competent on the gross difference, but not on the more subtle ones.

Why do alternative public policies often differ only slightly? It is because our political procedures are what might be called incremental. Just what is embraced in the idea of incremental politics requires explanation because this kind of politics is central to the analytical method to be clarified. In incremental politics:

1. Political parties and leaders compete for votes by agreeing on fundamentals and offering only incrementally different policies in each of the policy areas in which they wish to compete. Since this has been well and frequently demonstrated to be a prerequisite condition for the

survival of democracy itself,[3] it is hardly to be questioned as a characteristic of political life in the Western democracies.

2. Each of the competing political parties shifts its own policies only incrementally at any one time. Such incremental alteration of party policies is in fact the normal though not invariable rule in all two-party democracies and in some multiparty democracies.[4]

3. Policy-making proceeds through a sequence of approximations. A policy is directed at a problem; it is tried, altered, tried in its altered form, altered again and so on. In short, incremental policies follow one upon the other in the solution to a given problem.

These are easily recognizable fundamental processes in American democracy and indeed in most if not all of the stable, well-established, deeply rooted democracies of the world. To be sure, ideological rhetoric pervades political debate and runs through general statements of purpose in legislative enactments; but that incremental politics dominates is evident in the manner in which the legislators themselves, as well as administrators and judges, incrementally implement the ideologically described purposes.

Why is a political body incrementalist? One set of conditions leading to the regular and persistent use of political incrementalism is the attitudes, interests and values that support political incrementalism. It would carry us too far afield to examine the incremental attitude in detail, but it requires brief elaboration. It is to be contrasted with the disposition to approach policy as though great ideological issues were to be decided. Its hold on the citizens of the most stable democracies is attributable to many factors, among which I mention three: (a) widespread consensus on fundamental values, (b) frequent widespread agreement on the general direction and character of desired social change, (c) relatively greater confidence in the predictability of consequences of incremental as against drastic social change. When an individual lives in a society marked by the kinds of consensus referred to and when he is dubious about the predictability of large-scale change, his demands upon political leadership and his response to their appeals to him will have the effect of buttressing incremental policies.

If then—to return to the main line of exposition—theory is sometimes inadequate and if its inadequacies are sometimes attributable to incremental politics, what alternative analytical method do we sometimes substitute? The second method rests on incremental politics and incremental attitudes and might be called incremental analysis.

[3] A more exact statement of the condition and an explanation of its necessity to democracy will be found in R. A. Dahl and C. E. Lindblom, *Politics, Economics and Welfare*, New York 1953, pp. 294ff.

[4] *Loc. cit.*

The method is *incremental* in a specific sense. When we practice it, we foreswear the analysis of any large institutional complex as is often attempted with theory and attempt instead to predict the consequences of a specific hypothetical incremental alteration in public policy. We isolate the effects of the incremental change by examining differences in result between the *status quo* and each of several possible new situations that could be produced by a small change in public policy. Theory can be used incrementally too, but we shall see how this method becomes an alternative to the application of theory.

The method is one of *comparisons*. We compare results of several different possible policies, comparing each also with the policy of no change at all. The comparisons are chronologically *successive*. A succession of actual incremental policy alterations over a long period of time permits repeated comparisons among similar groups of incremental alterations in policy.

Whether we consider a hypothetical policy alternative to be incremental apparently depends upon: (a) the number of consequential variables that would be affected, and (b) the magnitude of the effect on each. A choice is less or more incremental on a continuum; at the incremental extreme is a choice that, with respect to a group of variables, affects only one and affects that one by the smallest amount that is still consequential. (But if, at the extreme, an incremental comparison is necessarily limited to one variable, in most actual cases, as will be later explained, we deliberately limit the number of variables by neglecting some of them.)

A policy will affect many or few variables depending upon what they are specified to be. And whether the variables are consequential, or the magnitude of the changes in them consequential, will depend upon how consequences are evaluated. For any analyst, therefore, a policy is incremental if, given the variables he considers consequential to his analysis and given his evaluations, the policy affects few of them and alters them only by small magnitudes. Clearly a choice can be incremental with respect to some values and nonincremental with respect to others, as well as incremental to one analyst and nonincremental to another.

The occasional eccentric who proposes to wipe the slate clean and begin again with a "new" human society and civilization is, of course, not an incremental analyst. Nor is the socialist, nor the doctrinaire nineteenth-century liberal in the twentieth century, nor the advocate of a centrally planned economy. On a continuum, the segment of policies to be described as incremental are such proposals as—and even these examples show considerable range themselves—lowering reserve requirements for banks, changing the retirement age under Old Age and Survivors' Insurance, requiring business notification of intention to merge

to the Federal Trade Commission, increasing military expenditures by 50 per cent, repealing the Taft-Hartley Act, establishing a Missouri Valley Authority, or removing certain agricultural subsidies or price supports.

It follows from what was said about the difficulties of theory and their relation to politics that our success with successive incremental comparisons will depend in largest part on whether political policy-making does in actual fact proceed through a sequence of what we consider to be incremental choices. In short, where politics is incremental, successive incremental comparisons help to make analysis feasible in the absence of adequate theory. But just what specific characteristics of incremental analysis do we find most significant for its capacity to assist us in the absence of adequate theory?

1. We find that the method sharply reduces our need for either a wide-ranging body of empirical generalizations or the propositions of a large formal theoretical system. We do not need propositions that describe a large institutional complex like competition because we limit our analysis to changes embracing only a few variables. Not that what theory we possess will be useless—it will often be a useful guideline or assist us in other ways—but it will not be essential.

2. We find that it similarly reduces our need for high-level generalizations on the fewer variables we do encounter. Specifically, we do not require relationships that hold over a large domain because we can be satisfied with understanding the relationship for the restricted domain within which our variables change in a particular policy choice. We can often so greatly reduce our need for generality in propositions as to make us independent of any that would be dignified by the name of scientific generalization.

The contrast on this point between incremental and conventional analysis as traditionally described is clear. Traditionally, we cannot proceed with a particular applied problem until we have found a generalization or group of generalizations that applies to our case. We must subsume our problems under a more general one. This means that behind our work lie many observations and derivations of generalizations from them, from which we now select what is appropriate to the particular instance. In incremental analysis the prior observations and derived generalizations are not always required, for we proceed directly to ascertaining the relationships among the few relevant variables in the particular situation that concerns us without troubling ourselves with whether there is a more general set of relations under which ours can be subsumed.

Presumably theories—and even scattered generalizations—are capital assets. To be able to do without them is not necessarily a virtue.

I do not deny this; I am simply clarifying a process for analysis in the absence of adequate theory or empirical generalizations.

3. From another point of view, we find that incremental analysis is a method for reducing the number and complexity of relations among variables to be considered. In predicting the consequences of incremental change only, it both reduces the required amount of information and at the same time limits problems to those within the grasp of the human intellect. More ambitious analysis very commonly embraces so many variables—demanding such quantities of information and such heroic organization of it—that the variables can only be treated implicitly; and neither the analyst nor his audience can be confident that they have been adequately considered.

Incremental analysis as a form of marginalism in analysis can be compared with consumer choice as another kind of marginalism in behavior. It is a commonplace that consumers make the problem of alternative product mixes manageable by ignoring all aspects of alternative mixes except the increment by which they differ. In the same way, incremental analysis simplifies the problem of alternative institutional mixes.

4. The product of our incremental analysis is a proposition stated in a form that often permits testing. Our outputs are fairly, specific statements about policy that are either implicitly or explicitly in the form of "if this, then that." For example, if a soil bank program is enacted, then certain predicted consequences will follow. Where actual policy is altered by increments like this, such a modest predictive statement is often testable in practice. Hence, incremental analysis is to a degree self-corrective; and, moreover, sequences of tests through policy choices permit analysts to improve their skill in prediction.

5. The sequence of policy choices made up to any given time offers us a useful standard of relevance for choosing new incremental alterations to analyze. The number of possible alternative institutions or policies is beyond counting, hence beyond the funds, time, and manpower available to social science. Some principle of selection is in fact necessary, and we often find past sequences helpful.

II. *Comprehensiveness and Fragmentation of Analysis*

The second point of difference between conventional and incremental analysis is the comprehensiveness of the former and the fragmentary quality of the latter. To the three essential elements of the incremental method, identified in the term "successive incremental comparisons," we add a fourth element: the comparisons are severely limited; they exclude certain important variables.

In considering why certain variables are ignored, it ought to be

remembered that I am describing not an idealized method but one prac-
ticed by fallible scholars. It follows that one reason that important
variables are ignored is that the analyst unwittingly misses them. A
second possibility is that certain policy consequences that in any one
study are trivial—and are therefore ignored—become cumulatively
significant through a succession of policy alterations. But a third possi-
bility is that the analyst deliberately ignores what he knows to be an
important variable.

Now as long as scholars look upon these omissions simply as aber-
rations, they will fail to understand that systematic omission is a
method of analysis rather than a failure of method. But often omission
is methodical. The important sense in which limitation or omission is
part of a method and not a failure of method turns on the relation be-
tween analysis, on one hand, and the social processes in which analysis
takes place, on the other. This point is critical and requires develop-
ment.

The usefulness of an analytical method cannot be understood in
isolation from the social processes through which it is applied. Limited
analysis is suited to certain social processes in which analysis takes
place. Consider a situation in which analysis of policy issues is widely
dispersed among many different analysts representing many different
scholarly interests, as in the United States. When analysis is thus "frag-
mented," limitation or omission in any one piece of analysis may actu-
ally be desirable. For while fragmentation may result in much lost
motion and may sometimes undercut the cumulation of scientific knowl-
edge that a centrally coordinated group of scholars might accomplish,
it is also a process or method through which the successive limited in-
cremental comparisons of many different scholars are coordinated, not
in the sense that each knows what the other is doing but in the sense
that each piece of analysis is nevertheless adjusted to each other.

Just as prices serve as parameters to the individual consumer solving
his own consumption-behavior equations, so a state of affairs being ana-
lyzed by any one analyst is sometimes taken as a parameter, if not a
given, by other analysts. More generally, what one analyst neglects,
because his analysis is limited, another analyst sometimes makes central
to his work. By analogy, in price-system behavior, an optimizing proc-
ess of sorts can be shown. For fragmented analysis, any possibility of
demonstrating an optimizing process, in which all relevant variables
are adequately investigated by some analysts and taken account of as
givens or parameters by all others, hinges upon what I shall call frag-
mentation of policy-making.

By fragmentation of policy-making, I mean to denote a political
situation in which (a) there are more than one or a few policy-making

groups in the government, and (b) they display many different points of view as well as specialized functions. This is clearly the case in all large governments, even in those most rigidly hierarchical; and fragmentation can go to extremes if the governmental process is little more than warfare between interest groups.

For several reasons too obvious to mention, fragmentation of policy-making may go so far as to be undesirable, even taking account of the variety of standards by which it might be judged. And it may, other objections to it aside, make prediction in policy analysis more difficult. But one should not take these possible evils for granted; and, in order to throw light on possible uses of limited and fragmented analysis, I should like to develop a line of argument displaying possible fruitful interconnections between it on the one hand, and fragmentation of policy-making on the other.

To begin with, fragmentation of policy-making as in the United States, probably encourages fragmentation of analysis. Many different centers of decision-making marked by many different interests and attitudes stimulate a diversity of analytical attacks on policy problems, possibly achieving therefore the advantage of analytical fragmentation. Whether fragmentation in politics induces fragmentation in analysis or not, it is in any case helpful to the successful employment of limited and fragmented analysis in two ways. First, when policy functions are dispersed among many different specialized groups, any one line of policy is more likely to be pursued through a succession of incremental policies. The group specialized to it has both time and motivation. Even where all group policies must be formally funneled through a hierarchically superior group, fragmentation places the initiation, intimate consideration, and execution of increments of policy in the hands of those who will carry on with a sequence. Hence, there will be many opportunities for analytical reconsideration when variables have been neglected.

Secondly, and more important, fragmentation in politics is helpful because the different points of view taken by the different groups in government serve to make each group something of a watchdog for certain variables against others. Now whatever the merits of this phenomenon generally, its importance to limited incremental analysis is enormous where in the analysis of any one policy problem certain variables are neglected. Fragmentation in politics, like analytical fragmentation, means that variables peripheral to one policy group become central to some others; hence variables peripheral to one analyst become central to another.

At best in any large government a large proportion of policy decisions —probably most of them—are attempts to correct the mistakes of previous policies. With political fragmentation, policy adjustments can be

rapid and persistent; and out of the somewhat specialized points of view of the decentralized policy groups a process can sometimes be generated in which a wide variety of variables are taken into account even if in every single policy problem taken alone some important variables are disregarded.

At their idealized best the relations between incremental analysis and political fragmentation constitute a method for the mutual adjustment of elements of policy to each other in the absence of sufficient human skill in both calculation and control to permit a single complex policy decision to achieve coordination. Neither scholar nor policy-maker has intelligence and accumulated information—and both are necessary—sufficient to permit an overview of the interrelationships among the variables of public policy for making one integrated economic policy decision or even sometimes a decision in a limited field of economic policy. Coordination or integration of policy is then to be achieved sometimes only through countless small adjustments of policy. It should be easy to understand the mutual adjustment of small policies in this fashion, since it is comparable—to turn again to the familiar analogy—to the mutual adjustment of prices through fragmented decisions where no over-all central simultaneous solution of the equations of the price system is attempted.

But, again, where the price system can produce a kind of optimum, it is not clear that fragmentation of policy-making can ever do so; and it is clear that in many circumstances it does not, for it can degenerate into an undesirable kind of minorites rule. However, the optimum achieved through the price system is a very limited one; hence, to demonstrate a comparable optimizing process in fragmentation of policy-making is perhaps a less ambitious task than first appears. In the works of a variety of political scientists a beginning has been made; they provide a description of a very rough and somewhat capricious optimizing process.[5]

Optimization or not, however, our point is that severe limitation of analysis, characteristic of incremental analysis, is a method adapted to a situation in which analysis is fragmented, and fragmentation of analysis is in turn abetted by political fragmentation.

III. *Values*

The third respect in which incremental analysis differs from what I have arbitrarily called the conventional is in the method in which

[5] A. F. Bentley, *The Process of Government*, Chicago 1908; Pendleton Herring, *The Politics of Democracy*, New York 1940; John Dickinson, "Democratic Realities and Democratic Dogma," *Am. Pol. Sci. Rev.*, May 1930, XXIV, 283-309; David Truman, *The Governmental Process*, New York 1951; Earl Latham, *The Group Basis of Politics*, Ithaca 1952. See also C. E. Lindblom, *Bargaining: The Hidden Hand in Government*, U. S. Air Force Project RAND Research Memo. RM-1434-RC, Santa Monica 1955.

values are handled. Conventionally it is required that the analyst postulate a social welfare function or, lacking that, at least determine his own utility surfaces on the assumption that he treat them as relevant for the problem at hand. Policy choices are then made to maximize the welfare function.[6] More informally, the view is often put in the form of such a question as: "I really don't see how it is possible to take a position with respect to proposed policy alternatives if one does not have in mind a version of some good society and select a solution 'most' in accord with that vision."

Actually we find we often cannot postulate a welfare function, cannot describe our good society, or cannot with any confidence employ a picture of a good society in the appraisal of alternatives. First, we often lack a general formula or agreed procedure for aggregating individual welfare functions into a social welfare function. Second, even more important, the preferences or values of the individuals with whom we are concerned are often not known either to us or to the individuals themselves except through actual policy choices made. Individuals often learn and express their preferences through the sequence of policy choices they actually make rather than deduce the appropriate choice from a preknowledge of their preferences. Third, while many individuals are satisfied that they know their values in a general way in advance of choice situations, the relevant valuations are shifting marginal valuations; and these they do not know and ordinarily cannot know except when faced with an actual choice.

What we economists often actually do, therefore, is make successive limited incremental comparisons of ends or values as well as of means in policy analysis. The analyst who employs incremental analysis obtains the required information on his own or some group's values by observing his own or some group's choices made in the recent past. Because in incremental analysis he observes and studies incremental choice and because he excuses himself from acquiring competence on any values except the temporary marginal evaluations of relatively few variables, the requirement upon him that he comprehend the values relevant to his policy analysis is not an impossible one. And because his data on values are the data of actual choices that have been made in previous policy decisions, they will often be superior to any other value data to which he could turn.

Moreover, it is a characteristic of democratic societies that policies are not even ideally to be derived logically from a unified set of values, as could conceivably govern the policy-making of a dictatorship. In actual fact, it is common for citizens or their political leaders to agree on policies without their even raising the question of whether they agree

[6] See, as one of many possible examples, J. Tinbergen, *On The Theory of Economic Policy*, Amsterdam 1952, pp. 1-5.

on ultimate values, it being possible to agree on the former despite different values. Hence general postulated values are irrelevant to many policy choices. The analyst employing incremental analysis simplifies his analytical problem by avoiding the assumption that there is always, in addition to conflict on policy preferences, an additional conflict on more ultimate values with which he must come to grips.

The difference in the treatment of values in incremental and conventional analyses can be illustrated. In incremental analysis, the analyst realizes that marginal preferences among values are constantly shifting with different degrees of achievement of each value. Where in nonincremental analysis he may conceive of a group or of himself as, say, to a degree equalitarian, in incremental analysis he explores through a succession of policies the attitudes of the population (or of himself) toward more or less equality in a variety of fields, is unable to generalize widely on public attitudes (or his own attitude) toward equality, and is sensitive to the changing priorities of equality goals in different areas and to changing priorities in the whole list of goals, of which equality is only one. He knows that a population (or he himself) can value certain incremental alterations in policy in the direction of equality of income, without necessarily being disposed either toward or against equalization of income as a general rule, because a general rule is not very meaningful. In incremental analysis, values are not simply formulae for the guidance of policy choices but interact with policy choices in such a way that each unendingly alters the other as more is learned about both values and appropriate policies from the results of each incremental alteration in policy.

Again, the usefulness of the method depends upon the character of the society in which it is practiced. The incremental approach to values is best suited to a society in which values are incrementally explored and adjusted by the citizenry or by whatever part of the population whose interests concern the analyst. At an extreme, each individual in the society makes successive limited incremental comparisons of his own values. Hence, again as earlier, incremental analysis rests on incremental attitudes and politics.

IV. *Policy Presumptions*

The last differentiating characteristic of incremental analysis is that it avoids the kind of policy presumption that is found in conventional analysis. I can illustrate the conventional type of presumption by a set of ideas like the following: The price system, it is argued, is a highly specialized or differentiated social mechanism for economizing. It possesses fairly obvious merits for meeting most of the tasks of economic organization. Hence, although it is deficient in certain identifiable respects, one should approach a judgment on a policy question with

a presumption in favor of the price system.

This is a common procedure. For example, after postulating certain value objectives for monetary policy, Friedman writes: "I believe . . . that all three objectives can best be realized by relying, as far as possible, on a market mechanism within a 'competitive order' to organize the utilization of economic resources."[7]

Now since no mechanism is to be pressed "as far as possible" unless possible means desirable (in which case the statement is empty), Friedman's statement has to be read as establishing a presumption. But a general case for the competitive system to which he presumably appeals is clearly not *logically* sufficient to create a presumption for its use in any particular area in which a policy problem arises. Often, therefore, we will avoid such a presumption. In incremental analysis we will simply examine alternative incremental adjustments, our only presumption being that a policy is best developed if at any one time it is considered and instigated as an incremental adjustment of an otherwise unquestioned and unaltered institutional complex. Toward this institutional complex we bring no presumption, favorable or unfavorable, because for our analytical task it is simply a given.[8]

Furthermore, while terms like "competition," "capitalism," "planning" and "socialism" continue to be useful for historical writing and for certain other purposes, we recognize that in a politically incremental society they do not describe actual policy alternatives. Since all relevant real-world systems are combinations of these "systems," what is at stake at any point of marginal policy adjustment is a "systems-mix." Hence the general case for or against any of these pure systems, we find, is not a case for or against any actual policy choice that is in fact open, any more than it is a logically sufficient basis for a presumption for or against any specific alteration of policy.[9]

Does it follow that without policy presumptions we make policy decisions without giving thought to the fundamental implications of the policy steps taken? Only superficially does this appear to be the case. To be sure, any analyst, regardless of method, may miss the funda-

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[7] Milton Friedman, "A Monetary and Fiscal Framework for Economic Stability," *Am. Econ. Rev.*, June 1948, XXXVIII, 246.

[8] Although use of such a policy presumption is possible only if values are postulated in the conventional manner discussed above, it misses the point to say that its use is merely a special case of the use of postulated values. The point turns not on the separation of value analysis from empirical analysis, which is at the heart of the third distinction between conventional and incremental analysis, but instead turns on the derivation of evaluations of particular policy alternatives from prior evaluations of large institutional complexes.

[9] Behind the conventional presumption there sometimes lies the implicit assumption that market controls and government control are in competition with each other. In incremental analysis lies the contrary assumption that they supplement each other, are indeed essential to each other, and compete with each other only at certain margins.

mental implications of a policy. But when we refuse to employ a *presumption* for competition, we are not therefore logically bound to be indifferent to the implications of a policy for the competitive process.

V. *Some By-Products of Clarification*

Presumably we make our common methods explicit in order to inspect and improve them, as well as to communicate them. In the case of incrementalism, improvement in communication is no small gain, especially in view of the difficulty some foreign scholars meet in understanding how we can so easily dispense with the ideological issues around which their own policy analyses revolve. But in addition, clarification produces by-products.

From an understanding of the nature of incremental analysis, some clarification of "muddling through" may be obtained. It appears superficially that "muddling through" sometimes works brilliantly and at other times produces almost farcical errors in policy. The difference in results is understandable on the hypothesis that "muddling through" has come to embrace both highly skilled incremental analysis at one extreme and near refusal to think at all at the other. Similarly the British reputation for inarticulate decision-making and the French reputation for extreme articulateness and theoretical refinement of issues may be attributable to the difference between the incrementalist who needs only to articulate significant differences and a kind of nonincrementalist who attacks each policy problem as though a whole economic or political system were called up 'for analysis.

The nature of incremental analysis also suggests a possible explanation of certain kinds of analytical noncomparability. Not infrequently a student or an acquaintance who is neither a professional scholar nor a public policy-maker will in discussion propose a somewhat original solution to a policy problem. The proposal is usually put in the form, "Why not . . .?" or "What would be the matter with . . .?" Sometimes the proposal is a good one. But where I think it is not, I am only sometimes able to give reasons for looking askance at it. In a number of puzzling cases, I am unable to do so. I believe my difficulty is not always due to difficulties in language or lack of technical competence.

More than to these factors or to some unspecified difference in background, my inability to provide reasons for rejecting a proposal is perhaps due to my employing successive incremental comparisons and to my inability to fit the new proposal into my customary sequence of comparisons. The student or acquaintance may be thinking nonincrementally or—more probably—with a sequence different from mine. Why might our chains of successive comparisons be different? Because mine but not his is a somewhat specialized professional chain shared with a group of scholars who through similar professional experience

come to a relatively high degree of agreement on the sequences of choice they consider relevant.

This hypothesis may answer such a question as why over the past thirty years or so economists have given short shrift to a number of proposals for monetary reform that were not crack-pot. They arose from a series of incremental comparisons different from the one or few that major groups of economists were implicitly working with. Of one scheme for monetary reform, Albert Hart writes revealingly: "... the scheme has probably not had as much attention as it deserves. The author has been rather a lone wolf in the academic profession. His notions do not fit easily into either academic or political discussion."[10] Such a passage can be read not as a comment on regrettable narrowness in the economics profession, which is what it superficially would appear to be, but as a piece of evidence that, in the employment of incremental analysis, analysts of substantially different backgrounds often cannot compare their results, cannot "appreciate" each other's conclusions.

And as for our difficulties in international comparisons of economic policy, each country's policy is also the end product of a distinctive succession of policy steps. Each policy has been chosen not so much by reference to some standard or postulated goal as by historical comparison with alternative increments. Since each country "begins" its development from a unique position and since each country develops a unique succession of incremental adjustments in both values and policies, it follows that different countries' policies often cannot be compared.

But just as explication of incremental analysis indicates why policies are often noncomparable, it points the way to making them comparable under circumstances in which we have often failed to find comparability. Once it is understood that policies chosen are the product of unique incremental sequences, it becomes clear that to some degree they can be compared if one is willing to take the trouble to work backward through each sequence of comparisons through which the respective policies were produced until some comparable elements are uncovered.

Finally, having found incremental analysis to lean on both incremental politics and incremental attitudes among the citizenry, it remains to be added that incrementalism in the large becomes potentially a major political orientation, or even a philosophy pertinent to still other areas of choice. To explore this would carry us too far afield. In politics, however, we find it increasingly difficult to apply the right-left or conservative-liberal dichotomy; this may be symptomatic that a growing number of individuals are neither conservatives nor liberals but philosophic and political incrementalists.

[10] A. G. Hart, *Money, Debt and Economic Activity*, New York 1948, pp. 445f.

CAPITAL LONGEVITY AND ECONOMIC DEVELOPMENT

By RUDOLPH C. BLITZ*

Diametrically opposite analytical conclusions and policy recommendations have been reached in recent discussions on the proper choice of capital longevity. These discussions deal mostly with the issue of capital longevity within the framework of problems of underdeveloped countries. It is not necessary, however, to deal with the longevity problem in such a restricted manner; virtually everything that need be said about capital longevity actually holds for any stage of development.

Thus W. A. Lewis argues that it is a frequent fault of development programs to build capital structures for an unduly long life and too solidly. These are luxuries which underdeveloped countries can ill afford because capital is so scarce for them [8, p. 395]. On the other hand, Galenson and Leibenstein hold that long-lived capital offers to underdeveloped areas the great advantage of a longer period during which no replacement has to be made. This in turn, makes possible a greater net output and greater reinvestment per man, which may be crucial for overcoming a critical early stage in the development process [4, p. 362].[1]

Neisser, commenting on the Galenson-Leibenstein article and considering the issues of lesser cost of short-lived equipment versus the gain of postponed replacement of more durable equipment, states: "I do not know of a criterion for weighing the advantages of a more rapid initial growth of operating capital against the disadvantages of an earlier temporary interruption" [10, p. 646].

[1] Galenson and Leibenstein cite Domar's calculations which show, for example, that with a rate of growth of gross investment of 5 per cent per year and an average length of life of capital of 4 years, the equilibrium ratio of replacement to gross investment will be 82 per cent per year. If the average length of life of capital should be 10 years, the ratio of replacement to gross investment drops to 61 per cent; and if the average length of life of capital is 30 years the ratio of replacement to gross investment would be merely 22 per cent per year [2, p. 8]. Domar, however, makes it quite clear that more long-lived equipment, while it has the agreeable quality of postponed replacement, also has the disagreeable quality of being more expensive; he states explicitly: "There is no doubt that beyond a certain range greater capital longevity becomes very expensive . . ." [2, p. 7]. Galenson and Leibenstein overlook this reservation of Domar.

In their "Reply" Galenson and Leibenstein admit that ". . . long-lived investment implies less output per unit of capital and therefore less reinvestment per unit of capital" [5, p. 647]. They nevertheless maintain their original position in favor of long-lived capital.

Thus the issue stands, and neither the advocates of short-lived or long-lived capital have stated with any precision just exactly how short-lived or long-lived they think capital should be, or what criteria they would use to make a decision in specific cases. Common sense would suggest that the advocates of neither position would propose the most extreme alternatives. Lewis would hardly advocate capital equipment with zero or close to zero life for all underdeveloped areas and neither would Galenson and Leibenstein advocate under all circumstances and at any cost the most long-lived capital equipment obtainable—even if one abstracts from the problem of obsolescence. In the subsequent discussion we shall develop a model which, we believe, contains the relevant criteria for the determination of optimum longevity of capital equipment for any stage of economic development.

I. *Long-Lived versus Short-Lived Capital*

All capital goods can be conceived as having stored up in them a certain volume of physical output or service which they release in form of a flow during their lifetime. In order to avoid ambiguity we shall assume that a machine which has a lifetime of k years does not deteriorate for k years but falls apart and has no scrap value at the end of the kth year. We can then distinguish two types of capital goods with respect to their longevity: (1) Physical service (or output) is rendered in annual fixed installments. It is, however, possible for capital goods yielding the same annual service to have different longevities. For example, roofs made of different materials yield the same annual square footage of shelter, but for different lengths of time. (2) The annual rate at which physical service (or output) is released can be varied; within limits this type of capital goods can be used more or less intensively without affecting the total output which the capital good yields over its lifetime. The respective chronological life-spans of two machines of this type will be determined by differences in the stock of service and by differences in the intensity of use.[2]

A. *The Cost of Longevity: Present Supply Price*

In the case of two pieces of capital equipment producing the same commodity or service at the same rate per unit of time, the piece with

[2] *E.g.*, machines can be run for one to three shifts without penalty to their lifetime output, but their chronological life-span will depend on the number of shifts the machines are run. The chronological life of a machine "containing" 300,000 units of service will be no longer than that of a machine "containing" 100,000 units of output if the first one is operated on a three-shift schedule and the second one on a one-shift schedule.

the longer life-span, which thus achieves the greater output during its life, will cost more than the machine with the shorter life-span. The same holds true for a machine capable of yielding a larger number of units during its total life compared to one which yields fewer units, but where the rate of output and thus the chronological life can be varied.

We may suppose that there exists a wide range of machines, each producing at the same rate of annual output, but producing it for different lengths of life.[3] Then we may think of a total cost schedule $A = A(k)$ which relates the initial cost of the machines, A, to their longevity, k. This is a technical or engineering relationship. From this total cost schedule one may derive an average cost curve, which we may call $a = a(k)$. It is the total cost of the machines divided by length of life; it shows the amount of annual "straight-line" depreciation of the machines as a function of longevity. As usual, where there is a total or average cost schedule, there is also a marginal cost schedule; we shall call it $A' = A'(k)$. If $A(k)$ is continuous—although, of course, it may have corners—then the cost of increments of longevity are defined for every value of k.

In very many instances the average and marginal cost curves of longevity are likely to be U-shaped. Of course, there may be very great variety in the shape of the individual "U's." The production of any piece of durable equipment, be it a man's suit or a locomotive, involves certain construction or assembly costs which over a wide range will be frequently independent of the durable quality of the material used in the construction of this piece of equipment. The case here is somewhat similar to one factor which makes for economies of scale, namely, that up to a certain point the assembly cost of a smaller or a larger piece of machinery may be the same and this, therefore, will result in a scale advantage in the case of the larger piece of equipment. On the other, in the case of longevity, after a certain point is reached additional years of life will be subject to increasing cost increments both because the price of the more durable material may increase very drastically and also because the exacting workmanship, care in assembling the parts and inspection of the finished product, which all make for greater longevity, become very expensive.

For convenience we will assume that $A(k)$ starts at the origin and therefore for $k = 0$ $a = A'$. This means that there exists a continuous choice for longevity. The a, A' curves are drawn on these assumptions in Figure 1. We may call these cost curves "objective" cost curves; they indicate the terms on which buyers can purchase longevity.

For our purposes it is unnecessary to linger on the question whether

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[3] The lifetime of a machine was defined previously as the period of k years during which the machine yields service without deteriorating and this lifetime is terminated by sudden, complete disintegration of the machine.
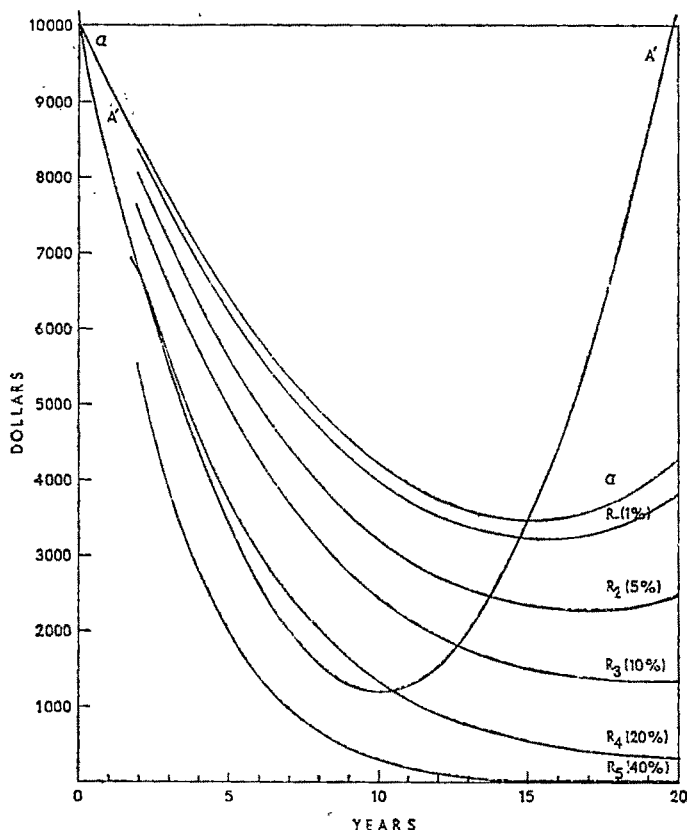
a U-shaped marginal longevity cost function constitutes the general or merely a special case. This is a matter which should be settled not on a priori grounds but by engineering research. All that is needed for our argument is that additional longevity costs something. We will consider both U-shaped marginal cost functions and a constant marginal cost function.

B. Optimal Longevity[1]

We may divorce the problem of optimum longevity from demand and variable-input considerations by considering in isolation the problem of determining what constitutes—as far as longevity alone is concerned—the lowest cost method of production. If machines of alterna-

[1] This topic is treated more precisely in the appended mathematical note. A similar solution for optimal longevity has been reached by Frederick and Vera Lutz [9, pp. 120-22]; also see R. G. D. Allen [1, pp. 404-5]. After completing this manuscript my attention was called to the discussion of Gustaf Åkerman's *Realkapital und Kapitalzins* by Knut

tive longevity respectively (1) produce at the same rate of total output. per year and (2) require the same rate of variable input per year, then the annual "gross profit" per year (annual gross revenue minus annual variable cost) will be the same for all the machines regardless of longevity. Moreover, in order to put machines of varying longevity on the same footing, we shall suppose that a current investment commitment for a machine of k years longevity obliges us also to replace the machine at the cost of $A(k)$ every kth year for all eternity.

If we make these simplifying assumptions, we find that current investment commitments of $A(k)$ and its perpetual replacement have a present equivalent cost which will depend solely upon longevity and on the rate of interest, r.[5] The latter is equal to the marginal efficiency of capital in the economy and is assumed to be unchanging over time. This present equivalent cost is designated as $C = C(k,r)$. Then, if we subtract from $C(k,r)$ the initial cost of a machine of longevity k, *i.e.*, $C(k,r) - A(k)$, we obtain the present discounted cost of all future replacements.

When r is given and constant over time, the optimum longevity is obtained where C is a minimum.[6] Since neither annual revenues nor variable cost depend on k, if any investment at all is to be made, it must be of this longevity. As longevity is increased by an increment this brings about an increase in total objective cost, A, but it also brings about "savings." The savings arise from the fact that as k is lengthened the resources for all *future* replacements will be needed an increment later in time every time the replacement has to be made; and this holds forever. These savings are a sort of capital gain. The optimal condition will therefore be achieved when the initial increment in outlay, A', equals the capitalized value of all future savings arising from these

Wicksell [13, pp. 258-99]. I was pleased to discover that there was already here the foundation of my analysis.

George Terborgh develops the concept of "operating inferiority" [12, pp. 61ff., 74-91, 168ff]. This concept refers *inter alia* to increases in operating costs due both to old age of the machine and to its obsolescence. Thus the accumulating "operating inferiority" is calculated by comparing the operating costs of the machine in actual operation to those of a new machine *of the same type* and also by making allowance for obsolescence on the basis of a comparison of operating costs of the machine in operation and those of *later improved models*. The initial cost of a machine together with its accumulating "operating inferiority" and the prevailing interest rate determine the policy for replacement. By contrast our model assumes that operating costs are unaffected by the age of the machine and it abstracts from obsolescence. Our model also assumes that differences in the initial cost of the machine are solely due to differences in longevity.

[5] In the absence of risk the "natural" rate of interest would seem to be the proper one for any economy to utilize for the allocation of scarce resources.

[3] The appended mathematical note shows that under the assumptions of a U-shaped average and marginal longevity cost curve, C as a function of k will have a regular minimum at least for some interest rates.

postponed replacements. The savings can be visualized as a "marginal revenue" of increased longevity. It is these savings which Galenson and Leibenstein rightly emphasize. However, they neglect the cost side of the problem. Only as long as the marginal cost of longevity is less than the marginal revenue of longevity is it economical to increase the total cost, A, of longevity. Five hypothetical marginal revenue curves, which of course depend on the nature of the total cost functions themselves, are labeled R_1 to R_5 in Figure 1 for interest rates ranging from 1 to 40 per cent.

The optimal conditions are discussed more precisely in the appended mathematical note. In general, the higher the interest rate, the smaller the optimal value of A' and k will be, because the present value of the future savings, discounted at a higher interest rate, will be less. As the interest rate approaches zero, the appropriate longevity approaches the minimum of the objective longevity average cost curve, but will always be somewhat to the left of it.[7] If the average and the marginal cost of longevity are constant, the proper choice of k is the very lowest value possible, no matter how high or low the interest rate, just as long as it is positive. This case can easily be understood; the reader need merely ask himself which machine he would pick, if given the choice between machines M_1 lasting two years and costing $2000 and machine M_2 lasting four years and costing $4000. The proper choice is obviously M_1, given a positive rate of interest, as interest can be earned on the $2000 which becomes freed if the lower-priced machine is purchased.

Hence some longevity will be purchased as long as the objective average cost of longevity is falling, the exact amount depending on the shape of A' and the interest rate; but only the least possible amount of longevity will be purchased if the a and A' cost functions are virtually a horizontal line. This conclusion is really very much in accord with Böhm-Bawerk's notions on the roundaboutness of capital and its virtues and vices. When the average cost of longevity is falling, additional longevity (roundaboutness) has additional virtues up to a certain point, and it is therefore worth while to purchase longevity; whereas if the average cost of longevity is constant, additional longevity would constitute an unnecessary burden.

C. *Expectation of Future Fall in the Interest Rate*

It is quite possible that an underdeveloped country which is initially plagued by a scarcity of capital and by high interest rates may nevertheless anticipate a substantial fall in the rate of interest. This may

[7] If the interest rate is so high that no intersection between the A' and the R function takes place, in our case at 40 per cent, this would mean that the appropriate policy would be to utilize machinery of zero longevity, that is, to operate without the piece of capital equipment under consideration and to rely instead on hand methods.

be so because more savings are anticipated from a growing national income, or because it is hoped that the country will become more "credit-worthy" for both foreign and domestic lenders after having made a good start on the road to economic development, or because of both these factors.

In this situation the optimal choice of longevity will be affected. It would be perfectly rational to buy even less longevity at a high interest rate, if it is expected to fall, than would be purchased if the high interest rate were expected to prevail. If a fall in the interest rate is anticipated, the appropriate choice is to store up only very little longevity for the time being and to buy more of it later on, when the interest rate has fallen. A fall in the interest rate is really the same thing as a fall in the price of longevity itself. This situation is similar to that of a man who, when down and out, might borrow $25 from a usurer to purchase a cheap suit in order to make himself appear respectable and thus obtain a job. This person may know full well that the suit will wear out in six months and that a $50 suit would last two years. This choice, however, was not open to him at that time, or rather the interest on the additional $25 necesssary would have been prohibitive. However, after having found employment, he will eventually replace his $25 suit with the $50 suit.[8]

D. *The Cost of Longevity and the Gestation Period*

Starting from the point of time of the investment decision it will very frequently take longer to put long-lived capital equipment into actual operation than short-lived equipment. This problem is merely a variation of the more general proposition that long-lived capital equipment is more expensive; it nevertheless warrants separate discussion since it may be of special importance for underdeveloped areas.

Frequently the difference in longevity between the more and the less expensive piece of capital equipment is due to the use of more labor, tools, and material in the construction of the more expensive, longer-lived piece. In this instance the period of construction for the long-lived equipment can only be the same as that of short-lived equipment designed for the same task if it is possible to use labor and/or tools more intensively in the first case than in the second case. The intensification and shortening of the construction process may become very expensive, because it may require premium payments such as overtime. Furthermore, especially in the underdeveloped areas, where there is a shortage of skilled labor and of equipment to build equipment, it may be quite impossible to speed up the construction of the more long-lived equipment. There is, for example, a limit on how many men

[8] We assume the suits to be similar in all other respects except longevity.

can work on the face of a rock or even on two faces in the construction of a tunnel. It may therefore be a perfectly rational decision to construct "in the mean time" a cheap railroad bed which travels around the hill, although such a route may appear "uneconomical" in the long run *if time preference is ignored* as compared to the more direct and more durable roadbed which would instead go through the hill.

Many mining shanty towns or city slums of the United States are the almost necessary by-product of a very rapid pace of industralization.[9] In military operations, where time preference is extremely high, it is standard procedure to build a temporary bridge and to replace it subsequently with a more permanent structure. Similarly, and the analogy is an uncomfortably close one, developing countries must also find means to cross their rivers in a hurry (*cf.* [4, p. 369]).

The more long-lived equipment, which in any case requires more resources, becomes even more expensive as compared to the less long-lived equipment because of the additional cost of waiting. The magnitude of this additional cost will depend on the rate of interest and the size of the expected income stream. The larger the anticipated income stream the greater is the cost of waiting. Many basic investments in underdeveloped areas, such as roads, housing, schools and clinics may provide external economies for many other ventures. To the extent that any industry provides external economies to other industries, the delay and cost arising from the penchant for longevity will be compounded once more. In terms of Figure 1 the effects of a gestation period can be visualized as either an upward shifting of the A' curve or downward shifting of the appropriate R curve.

II. *Maintenance Costs and Obsolescence*

In the formal model used so far optimum longevity was determined solely by the marginal longevity cost function, A', and the interest rate, r. We assumed perpetual replacement, which implies perfect foresight and the absence of all technological change. We also assumed a constant rate of output over time in perpetuity, and constancy of both variable factor input and product prices. We abstracted from such issues as maintenance costs and obsolesence. In the real world the relationship between longevity and the rate of interest may turn out to be much different from that suggested by our simplified model; this divergence may be due largely to the role of maintenance cost and obsolescence.

The term maintenance cost will be used here as a concept quite distinct from fixed and variable costs. Whether or not a certain out-

[9] Housing facilities in the newly industrialized areas of the Soviet Union too are reported to be very crude and frequently of a makeshift character (*e.g.*, [6, pp. 186-87]).

lay constitutes a maintenance cost according to our definition depends on two criteria, both of which must be satisfied simultaneously: (1) an outlay on service of equipment must extend the longevity of the equipment; (2) the outlay can be undertaken only after the equipment has been installed and has been in operation for some time.

To make the concept of maintenance costs quite clear it is necessary to introduce another concept which we will call durability outlays, which are quite distinct from maintenance costs. Durability will refer to an enduring quality of equipment which is achieved without maintenance outlays. The concept of durability is also distinct from our earlier concept of longevity, which is broader. Longevity, we will now recognize, can be bought as some mix of durability and/or maintenance outlays, subject only to the restraint that it must contain *some* durability, but it need not contain any maintenance outlays.[10] In Section I we abstracted from maintenance costs and therefore the broader concept of longevity became synonymous with the narrower concept of durability as those concepts have here been defined.

The following examples will make clear the distinction between our concept of maintenance costs on the one hand, and fixed, variable, and durability costs on the other hand. The periodic blasting of loose rock from the face of a mountain, which is traversed further down by a highway or a railroad, is frequently referred to in common language as maintenance of way. In economic terms, however, this constitutes simply a fixed cost which has to be undertaken periodically to allow the safe passage of traffic; it is a fixed cost because it is independent of the volume of traffic. On the other hand much of what is commonly called maintenance cost constitutes really variable cost in the economic sense. The removal of ashes and slags from a furnace is simply a variable cost and is directly proportional to the rate of input of coal and other raw materials and is, of course, also related to the rate of output of this furnace. Other cleaning operations, commonly referred to as maintenance costs—such as the removal of accumulated lint from cutting machines—are of the same nature.

In what follows we will deal primarily with the appropriate principle of choosing between durability and maintenance outlays. A very simple example of such a choice would be whether to use for construction purposes material which costs more initially but need not be painted, or to use less expensive material which must be painted periodically in order to last as long as the unpainted material.

If certain expenditures on maintenance are necessary to make a

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[10] A somewhat loose analogy may help here: the life-span of a human being may be due to two factors. One is his innate physical endowment (durability), the other is the intensity of effort of the doctors (maintenance).

machine last j years longer, then these these maintenance costs over the lifetime of the machine can be discounted to the present and can be treated in our framework as part of the initial total outlay A. It should be clearly understood that while we treat discounted maintenance costs as a substitute for greater outlays on durability for the purpose of extending longevity, this does not imply that if longevity in the form of durability of m years is extended to, say, $m + 5$ years, this can be done simply by spending zero on maintenance for m years and then incurring certain annual outlays on maintenance for the next five years. It
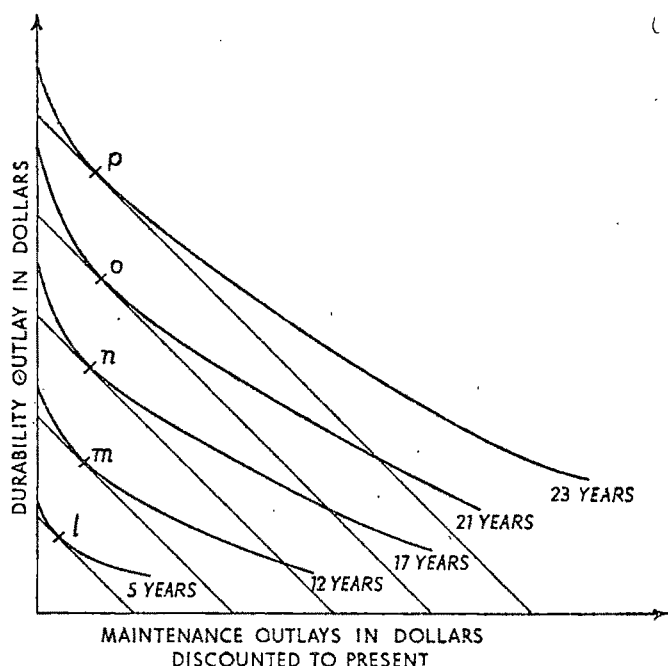
FIGURE 2

may be necessary to spend part of the maintenance outlay long before the expiration of m years.

The relationship between durability and maintenance is shown in its simplest form in Figure 2 with isoquant curves. A more precise geometric exposition will be given subsequently. These isoquants can be drawn either on the assumption of a zero interest rate or on the assumption that all "maintenance-work hours" are discounted appropriately to the present; in the latter case, of course, the shape of the isoquants themselves would change with changes in the interest rate. Moreover, each present discounted value of future maintenance expenditures located on any isoquant represents an optimal pattern of maintenance-work hours through the time which can be purchased with a certain

amount of cash on hand today. The family of 45 degree lines are constant outlay curves which show that a dollar spent on durability can be substituted for maintenance outlays whose present discounted value is a dollar. The isoquants start from the Y-axis because, as mentioned previously, longevity can be obtained by some outlay on durability without any additional maintenance outlay, but not with an outlay on maintenance and no outlay on durability. From the points l to p both total and marginal longevity cost curves can be constructed. These points minimize the total present costs for specified amounts of longevity.

The same relationship can also be shown by means of a conventional total cost curve. Curve A in Figure 3 is a total durability cost curve.
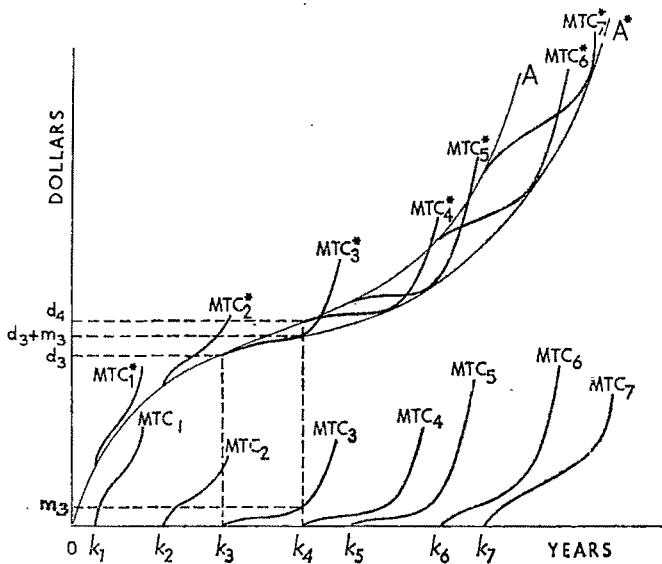
FIGURE 3

It indicates the dollar cost of varying durabilities or, alternatively, of varying longevities if no maintenance costs are to be incurred. If, for example, a durability of k_4 years is to be acquired the cost will be d_4 dollars. If a machine of that durability has been purchased, however, its lifetime (longevity) may be extended beyond k_4 years upon incurring maintenance costs. The maintenance costs necessary to extend the longevity of this machine beyond k_4 years vary in accord with the maintenance total cost curve MTC_4. To attain various longevities in excess of k_4 years by using a machine of k_4 years durability and maintaining it so that it lasts longer, the combined durability and maintenance costs will vary according to MTC_4*. Such a curve is obtained by adding

to the MTC curve the cost of the durability which defines the MTC curve. Thus $MTC_4^* = MTC_4 + d_4$.

Consider next the envelope of the MTC^* curves. This envelope curve is A^*. It shows the minimum cost of obtaining any prescribed longevity by an optimal combination of durability and maintenance outlay. To obtain longevity k_4, for example, the optimal durability cost would be only d_3 which provides a durability of k_3 years. Then maintenance outlays of m_3 dollars would be incurred to extend the life of this machine to k_4 years, as shown by the curve MTC_3. The total cost of that longevity would then be $d_3 + m_3$, the height of the curve A^*.

We can now transform the total cost curves of Figure 3 into average cost curves (see Figure 4). Corresponding to our earlier graph (Figure 1) and to the a and A' curves we can now construct two new curves which we label a^* and A'^*, the latter two being derived from the A^* curve (Figure 3). The a^* curve is an envelope curve and will be the same as curve a for any range in which additional longevity is obtained more cheaply by buying more durability than by incurring more maintenance expense; but it will be below the a curve and will be an envelope of the average joint longevity cost curves (labeled $AJLC$) in any range in which some additional longevity is purchased more cheaply in the form of maintenance rather than in the form of greater durability. A'^* shows the marginal cost of longevity, given an optimal allocation of expenditure on durability and maintenance, and supplants the role of the curve A' in Figure 1 in determining the optimal longevity.

The envelope curve a^* is in many ways similar to the more familiar envelope curve used for the exposition of economies of scale. The similarity consists in the fact that we get, as long as the envelope curve is falling, a phenomenon of undermaintenance at the optima, just as one gets underutilization in the case where the envelope curve of conventional cost analysis is falling because of economies of scale. It is always worth while, if additional longevity is desired, to buy more durability and to undermaintain it (*i.e.*, not to maintain it to the point where the $AJLC$ is minimal) as long as the average cost of durability is falling, rather than to buy less durability and to operate where average maintenance cost is at a minimum.[11]

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[11] Our use of the concepts of durability and maintenance may appear artificial, especially the notion of obtaining durability without maintenance outlays. Therefore, before pointing out specific practical implications of this model, it should be explained that the model presented here has been designed with a particular sort of capital in mind. We have thought in terms of capital such as roads, bridges, and buildings which have significant durability even in the absence of maintenance outlays, although maintenance outlays may extend their longevity further. Our model may be less adapted to instances of capital equipment which can have only very short durability without substantial maintenance expenditure. In such a case the envelope curve a^* may be formed, over most of its range, by a single $AJLC$ curve.

If maintenance costs are incorporated in the a^* and A'^* functions and if maintenance costs differ as between countries because of the different relative scarcities of the requisite labor, we will actually obtain distinct a^* and A'^* curves for each individual country even though durability costs are the same. This is a substantial modification of our
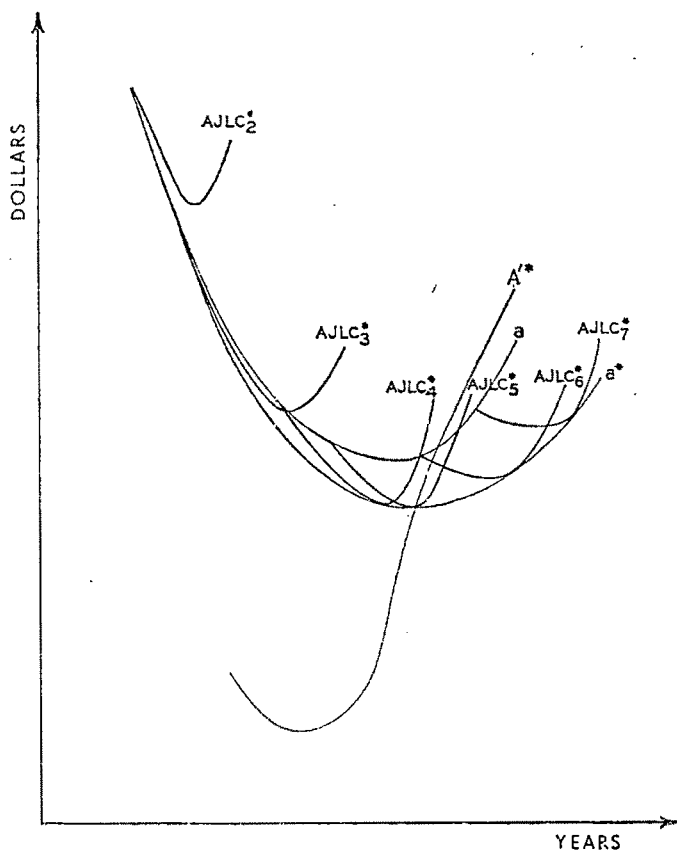
FIGURE 4

earlier model where we used common a and A' curves as between different countries, and different longevity decisions were solely due to differences in the interest rate.

A relatively ample supply of labor should help to keep maintenance costs low, although what really counts is not abundance of aggregate labor but of appropriately skilled labor. Thus maintenance costs may be extremely high in underdeveloped countries with an abundance of cheap but unskilled labor. But maintenance costs may be low in, say, industrialized Europe with aggregate labor much scarcer but skilled

craftsmen making up a much higher proportion of the total labor supply.

Moreover, at the time the capital equipment is installed, maintenance costs in contrast to durability outlays appear as postponable expenditures. Some maintenance expenditures, to be sure, cannot be postponed for very long, but others—and these are likely to be the more substantial expenditures—only loom in the more distant future. Countries where efficient labor is relatively abundant, and which for this reason have relatively low maintenance costs, are also likely to have to contend with higher interest rates. For both reasons maintenance outlays are likely to be substituted for durability outlays in these areas. These considerations may help to explain why countries which suffer from much greater capital scarcity than the United States may yet turn out to use capital equipment of greater longevity than is used in the United States.

Not only will the amount of maintenance expenditure per period on a certain type of machine differ from country to country depending on the cost of maintenance, but there are differences in the maintenance techniques employed which are attributable to differences in the relative factor scarcities in different countries. These differences in maintenance techniques may have many subtle effects on the design of capital equipment. For example, in contrast to British open hearth furnaces, American furnaces are designed to allow free access to the vulnerable parts of the furnace for mechanical handling equipment, such as high-lift and fork-lift trucks. On the other hand, English repair methods rely less on mechanical aids, but are more labor intensive and require more time.[12]

We have glossed over the difficulties of international empirical comparisons of longevity. Because maintenance costs would be different, we would expect to get different longevity cost curves for different countries. But because of different relative scarcities of factors in different countries it would be virtually impossible to find capital equipment which is the same in all other aspects and differs only in regard to longevity. An interesting illustration of this problem is the blast furnaces used in the United States and Great Britain. On the one hand American furnaces are lined with more expensive material than their British counterparts, and should therefore be more durable. This greater durability, however, is offset by higher driving rates and greater wind velocity applied in the American smelting process. Comparisons are further

[12] In this instance the difference in maintenance technique accounts not only for a different design of the capital equipment, but because the American maintenance techniques are less time-consuming than the British, the average ratio of idle furnaces to total furnace capacity is much lower in the United States than in Britain. [14, p. 99]. The adaptation of equipment to peculiar "maintenance scarcities" is also a paramount problem in the design of much military equipment.

beclouded by the fact that American furnaces are on the average larger and leave both a different volume and quality of slag from those used in Britain. Moreover, the coke used in the United States has a lower sulphur content than British coke. The net result of all this is that the average life of American furnace linings is slightly less than that of British linings but a greater tonnage is achieved by American furnaces between relinings [14, pp. 33-34].

There is a widespread belief that both the average age of much European capital equipment is greater and also its life span is longer than in the case of its American counterparts.[13] This view has been presented by L. Rostas [11, pp. 55-58]. He also indicates that there is very little reliable information on this matter and is therefore forced to leave the whole question unsettled. The reader of this paper is cautioned that much capital goods used in different countries for similar purposes will turn out at best to be different species of the same genus.

Expected obsolescence and expected changes in the demand for the product will, of course, also have to be taken into account in making decisions as to the longevity of equipment that is desirable. Although a rigorous analysis of the necessary modification in our model to take these variables into account is not possible, a few general observations are in place here. If a precise comparison could be made between the United States and industrial Europe as to the effects of expected obsolescence on capital longevity, it might be found that expected obsolescence has created more of a bias in favor of short-lived capital equipment in the United States. Because for long periods the rate of progress expected in the United States has been more rapid, or merely because of fashion-induced style changes, much capital equipment and also durable consumers' goods may have been constructed for a shorter life span than has been the case in Europe.[14]

On the other hand, anticipated obsolescence and similar uncertainties may be especially important considerations for countries which are just passing through the initial stages of industrialization and which expect very drastic—not to say revolutionary—technological and social

[13] Comparisons as to the average age of capital equipment of course reflect longevity of equipment, but they also reflect the historical path of the preceding birth and death rates of the machine population. An investigation for Western Germany shows that in 1952, 30 per cent of German machine tools were less than 10 years old and 69 per cent less than 20 years old [15, p. 44]. In the United States as of the same date 54 per cent of similar equipment was less than 10 years old and 79 per cent less than 20 years old. It is not unreasonable to assume that this difference may in part be due to the fact that in Germany recovery based on rearmament proceeded very rapidly after 1933, whereas the recovery lagged behind in the United States by several years.

[14] It is interesting to observe that both the Rolls-Royce and the German Volkswagen, automobiles on opposite ends of the price range, strongly emphasize the absence of fashion changes in their sales appeal.

changes.[15] Anticipated changes of this sort then will push the choice even further in favor of short-lived equipment, made already on the basis of the capital scarcity prevailing in these countries.

III. *Conclusions*

A model has been presented in which the optimum life of capital equipment was related to the interest rate, the longevity cost function, and the substitution of maintenance outlays for durability. In the first section of the paper maintenance aspects of the problem were ignored with the result that optimal longevity was inversely related to the level of the interest rate. This solution was the result of the fact that the discounted value of "savings" of postponed replacement would become less the higher the interest rates. This might be referred to as the direct effect of the interest rate on longevity. Nonobservance of the rules established here will result in misallocation of resources in the same manner as nonobservance of other rules of resource allocation.

Then the maintenance aspects were explored explicitly. It was shown that the interest rate has a pronounced effect on the substitution between the amount of initial capital expenditure and maintenance outlays. In the language of this paper this involves a substitution between durability and maintenance outlays and this, in turn, results in movements of the longevity cost curve. This might be referred to as the indirect effect of the interest rate on longevity.

The interest rate, therefore, really affects capital longevity in two different ways: One is the simple and direct impact on longevity. The second effect is of an indirect nature. It asserts itself through determining *inter alia* the rate of substitution between durability and maintenance outlays. The higher the interest rate the more heavily should maintenance be substituted for durability. Of course the possibilities and the nature of such substitution are primarily determined by technological considerations. The direct and the indirect effects of the interest rate, just discussed, really have opposite impacts on optimum longevity.

Areas in different stages of economic development should vary systematically in their choices between durability and maintenance, and these choices will have implications for the appropriate longevity of capital. If high interest rates and low maintenance costs prevail in a country, then maintenance outlays should be substituted for durability outlays. In this case the substitution of maintenance outlays for durability would overshadow the direct discouraging effect of high interest

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[15] *E.g.*, A. E. Kahn emphasizes the ". . . additional leaks, lags and frictions . . ." which make investment undertakings in primitve economies unusually risky and subject to the danger of rapid obsolescence [7, pp. 50-51]. For a similar view also see W. A. Lewis [8, p. 395].

rates on longevity. Thus it may be the correct policy under these circumstances for countries where high interest rates prevail to decide nevertheless on capital equipment of greater longevity than would be appropriate for countries where the interest rate is lower. Furthermore, if expectations of obsolescence are lower in the former as compared to the latter case, this should reinforce the pattern of choice just discussed. It is a shortcoming of the formal analysis of this paper that it has not come to grips satisfactorily with problems of risk and uncertainty.

Although most maintenance work is likely to be very labor-intensive and although most underdeveloped countries are blessed with an ample supply of labor this does not imply that these countries should invariably spend heavily on maintenance and stint on durability. Some maintenance work requires little skill and in these cases the choice should obviously be in favor of maintenance outlays. The problem is more complex in the case of maintenance work which requires much skill and judgment. The cost of very skilled maintenance work may be extremely high in underdeveloped countries despite an ample supply of labor.

In the short run then it would appear as a bad policy for such a country to attempt to substitute much of this type of maintenance for durability. We cannot within the framework of this paper discuss the issue of investment in capital goods versus investment in human beings. Our foregoing analysis, however, has thrown light on at least one relationship which would have to be considered among others, if this choice is to be made correctly. The training of appropriate skills may frequently allow a drastic extension of the longevity of capital equipment and in turn, as has been shown in the beginning of this paper, increased longevity may have most important effects on the rate of economic growth.

A MATHEMATICAL NOTE ON OPTIMUM LONGEVITY

By FRED M. WESTFIELD*

1. Under the assumptions made in *Section* 1 the optimal longevity is determined by finding the minimum of the expression:

$$(1) \quad C = C(k, r) \equiv A(k) + A(k)e^{-kr} + A(k)e^{-2kr} + A(k)e^{-3kr} + \cdots$$

$$= \sum_{j=0}^{\infty} A(k)e^{-jkr},$$

where r represents the given and constant instantaneous rate of interest

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and C, the present equivalent cost of an investment commitment costing $A(k)$ initially and again $A(k)$ every kth year thereafter.[16] Since this is a geometric progression, for any positive interest rate its sum may be written as (cf. [9, p. 120]):

$$(2) \qquad\qquad C(k, r) \equiv \frac{A(k)}{1 - e^{-kr}} .$$

2. A necessary condition for the minimum of $C(k,r)$ is that its first partial derivative with respect to k, $\partial C/\partial k$, be zero:

$$\frac{\partial C}{\partial k} = 0 = \frac{(1 - e^{-kr})A'(k) - A(k)re^{-kr}}{(1 - e^{-kr})^2},$$

where $A'(k)$ is the first derivative of $A(k)$. This can be simplified to:

$$(3) \qquad\qquad A' = \frac{Are^{-kr}}{1 - e^{-kr}} \equiv Cre^{-kr} \qquad\qquad (k, r > 0).$$

This summarizes the conclusion that optimum longevity is determined by the value of k for which the "objective" marginal (undiscounted) cost of longevity (A') is just equal to the present discounted value (Cre^{-kr}) of the savings from the incremental deferral of all replacements—the "marginal revenue."

3. The necessary second-order condition for a minimum is that the second partial derivative of C with respect to k, $\partial^2 C/\partial k^2$, must be positive. Carrying out this differentiation and simplifying, we can obtain the condition:

$$(4) \qquad\qquad A''(k) > - rA'(k),$$

where A'' is the slope of the marginal cost curve A', and $-rA'$ turns out to be the slope of the marginal revenue curve. This inequality is the usual requirement that the marginal cost curve intersect the marginal revenue curve from below. Since A' is everywhere positive and U-shaped, if the two curves intersect at all this condition will be met at one of the intersections. But even if A' is not U-shaped, but steadily falling, the condition can still be fulfilled provided that the A' curve does not fall more rapidly than the marginal revenue curve.[17]

4. A higher interest rate must necessarily reduce the optimum length of

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[16] The instantaneous interest rate r is related to the interest rate i compounded once per year, by the relationship:

$$e^r = (1 + i),$$

where e is the base of the natural logarithms. (Cf. [3, pp. 359–62].)

[17] In the extreme case where the marginal longevity cost curve is a horizontal straight line, and the average longevity cost curve $A(k)/k = a(k)$ is also the same straight line, the first-order conditions hold in the limit as k approaches zero. Under these conditions $C(k,r)$ approaches its minimum as a limit as k approaches zero.

longevity, k. We differentiate the equilibrium conditions (3) totally with respect to the parameter r; or

$$\frac{d}{dr}\left[A'(k) - C(k, r)re^{-kr}\right] = 0.$$

After performing the indicated operation and simplifying, we obtain the expression for the desired rate of change of the optimum k as a result of an incremental change in r:

$$\frac{dk}{dr} = \frac{- Ce^{-kr}\left[rke^{-kr}/(1 - e^{-kr}) + rk - 1\right]}{A'' + rA'}.$$

Is this expression positive or negative? We know that the present equivalent costs, C, and the discount factor, e^{-kr}, are positive. We also know that the denominator, $A'' + rA'$, is positive; the second-order condition (4) requires this. Therefore, the sign of dk/dr is governed by the sign of the term in brackets. If

$$\frac{rke^{-kr}}{1 - e^{-kr}} + rk - 1 > 0,$$

dk/dr will be negative; and, vice versa, if this inequality is reversed. Dividing the numerator and denominator of the first term by e^{-rk} and multiplying the entire expression by -1 times the resulting positive denominator of the first term, the condition becomes

$$e^{+rk}(1 - rk) < 1.$$

This inequality is clearly satisfied for all r and k such that $rk \geq 1$. Therefore, in these cases, dk/dr is negative. Is it also satisfied for small values of k or r such that $0 < kr < 1$? Taking (natural) logarithms of both sides, one obtains

$$rk + \ln (1 - rk) < 0.$$

This is of help because the second term may be expanded so that we obtain

$$rk + \left(-rk - \frac{(rk)^2}{2} - \frac{(rk)^3}{3} - \cdots\right) < 0, \qquad 0 < rk < 1,$$

which is obviously satisfied for such small positive values of r or k. Hence an increase in the interest rate cannot under our assumptions lead to an increase in the optimum longevity.

5. As the interest rate approaches zero, the optimum value of k approaches the minimum of the objective average longevity cost curve, $a(k)$. Although the right-hand side of expression (3)—the marginal revenue curve—is not defined for $r = 0$, its limiting value as r approaches zero is defined. We may evaluate this limit according to a well-known rule: Differentiate the numerator and denominator separately and evaluate the quotient at $r = 0$. This gives us

$$\lim_{r \to 0} \frac{Are^{-kr}}{1 - e^{-kr}} = \frac{\dfrac{\partial Are^{-kr}}{\partial r}}{\dfrac{\partial (1 - e^{-kr})}{\partial r}}\Bigg|_{r=0} = \frac{-Are^{-kr}k + Ae^{-kr}}{ke^{-kr}}\Bigg|_{r=0}$$

$$= \frac{A}{k} \equiv a(k).$$

As r approaches zero, our marginal revenue function approaches the objective average longevity cost function; therefore, at least cost,

$$A' = a(k).$$

Hence, as r approaches zero the optimum longevity approaches the minimum of the average longevity cost curve.

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"PEOPLE'S CAPITALISM" AND STOCK-OWNERSHIP

By Victor Perlo*

It has become the fashion to classify the economy of the United States as a new form, "People's Capitalism." The expression was developed by the Advertising Council, which prepared a "People's Capitalism" exhibit, shown internationally under the auspices of the United States Information Agency.[1] The term is accepted by publicists and widely propagated in corporation reports and advertisements. It is used by some research and academic economists. Future editions of economic texts can hardly fail to discuss the theory—or slogan—of "People's Capitalism."

The central component of "People's Capitalism" is the contention that ownership of American industry has become democratic in character through the dispersion of stockholdings among the population.[2] This is not only given the most attention, but also is the feature that involves an alleged qualitative change in structure. The present paper is devoted to this theme.

There follow typical statements by professional economists, corporations and their officers. Marcus Nadler writes:

> The economy of the United States is rapidly assuming the character of what may be termed 'People's Capitalism,' under which the production facilities of the nation—notably manufacturing—have come to be increasingly owned by people in the middle and lower income brackets or indirectly by mutual institutions which manage their savings.[3]

This is the principal one of the "striking political and social transformations" which Nadler states have taken place during the past three decades—that is since the late 1920's.

Roger M. Blough, chairman of the Board of U.S. Steel Corporation, states that our economic system has a "remarkable propensity—to divide the benefits of its own multiplying among those who contribute

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[1] *New York Times*, Feb. 14, 1956, p. 20; Sept. 3, 1956, p. 14.

[2] An exception was the American Round Table discussion of People's Capitalism, jointly sponsored by Yale University and The Advertising Council. While stressing the importance of widespread ownership, the panel members "agreed" that this "need not be regarded as the most essential part of a people's capitalism"—The American Round Table, *Discussions on People's Capitalism, November, 1956*, New York 1956, p. 8.

[3] M. Nadler, *People's Capitalism*, pamph., New York 1956, p. 5.

to it."[4] According to Blough, industrial executives since the depression of the 1930's have sought consciously to foster this division of benefits. He cites as a significant example:

> . . . the change that has occurred in the ownership of our larger enterprises. Today fewer businesses—especially our biggest businesses—are owned by a few wealthy individuals or groups, as many were back in the Nineties. They are owned by millions of people in all walks of life. In United States Steel, for example, the owners of our business outnumber the employees by a considerable margin; and no one of them holds as much as three-tenths of one per cent of the outstanding stock.[5]

The General Electric Corporation heads a full-page advertisement: "People's Capitalism: The 376,000 owners with savings invested in General Electric are typical of America, where nearly every citizen is a capitalist."[6]

Standard Oil Company (N.J.), tells its employees that Karl Marx:

> . . . devised a theory. . . . Ownership of the mills, as with ownership of the land, was the key to the future. Ownership should, therefore, be vested not in the hands of the few, but with something he identified as The People.

Today, says Standard Oil, this is realized in the United States:

> Yes, the people own the tools of production. . . . By his own definition, Karl Marx' prophecy has been realized. . . . How odd to find that it is here, in the capitalism he reviled, that the promise of the tools has been fulfilled.[7]

A. D. H. Kaplan writes:

> The number of stockholders now equals or exceeds the number of employees in many large American corporations. The effect of prevailing tax rates on inheritance and income is toward progressive diffusion of the personal capital holdings in American corporations.[8]

A secondary component of "People's Capitalism" is the contention that incomes have been redistributed from the rich to the poor. That has been presented most authoritatively by Simon Kuznets[9]; the opposite viewpoint is presented in my booklet, *The Income "Revolution."*[10]

An auxiliary feature is the high American living standard. This has

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[4] R. M. Blough, address before the Economic Club of New York, Jan. 15, 1957, p. 6.
[5] *Ibid.*, p. 9.
[6] *Wall Street Jour.*, May 13, 1957.
[7] Esso Corp., *The Story of Creative Capital*, pamph., undated.
[8] A. D. H. Kaplan, *Big Enterprise in a Competitive System*, Washington 1954, pp. 178-79.
[9] S. Kuznets, *Shares of Upper Income Groups in Income and Savings*, New York 1953.
[10] New York 1954.

been taken over from earlier positive appraisals of the workings of capitalism in the United States, and involves little that is new in principle.

The conclusion of the analysis in this paper is that the main justification of the term "People's Capitalism"—widespread stock-owner-ship—is without substance.

I. *Trend in Number of Stockholders*

Has there actually been a sharp rise, since 1929, in the number and proportion of the population owing stocks? Table I presents available estimates:

TABLE I.—PROPORTION OF STOCKHOLDERS TO POPULATION IN UNITED STATES 1927–56

Year	Number of Stockholders	Population (millions)	Stockholders as Per Cent of Population
1927 (a)	4–6 million	119	3.4–5.0
1927 (b)	5–6 million	119	4.2–5.0
1930	9–11 million	123	7.3–8.9
1937	8–9 million	129	6.2–7.0
1952	6,490,000	157	4.1
1954	7,500,000	162	4.6
1956	8,630,000	168	5.1

Sources: Population from U.S. Bureau of Census.
   Number of stockholders:
      1927 (a) A. A. Berle Jr. & G. C. Means, *The Modern Corporation and Private Property*, New York 1932, p. 374.
      1927 (b) and 1930 N. R. Danielian and others, *The Security Markets*, New York 1935, pp. 49–50, 723 ff.
      1937 Temporary National Economic Committee, Monogr. No. 29, Washington 1940, p. 168.
      1952 L. H. Kimmel, *Share Ownership in the United States*, Washington 1952, p. 89.
      1954 and 1956, New York Stock Exchange, shown in *Economic Report of the President*, *January 1957*, Washington 1957, Table D-21, p. 112.

The three postwar estimates are limited to stockholders in publicly owned corporations, whereas the attempt was made also to include in the prewar estimates stock-owners of small private family corporations. Kimmel estimated that in 1952 there were 3 million owners of privately held stocks, but commented: "These do not, of course, constitute net additions to the number of share owners, for many of them also own publicly held issues."[11] The description of the estimating methods used by the TNEC suggests that its figure for 1937 might have been lower by 1½-2 million if privately held corporations had been omitted. With this qualification in mind, the following comments based on Table I are in order:

[11] L. H. Kimmel, *Share Ownership in the United States*, Washington 1952, p. 126.

The percentage of the population owning stock in 1956 was, if any-
thing, lower than the corresponding percentage for 1930, but higher
than that for 1927. There is a cyclical movement in the percentage of
the people owning stocks. It increased sharply during the culminating
years of the bull market of the 1920's; declined during the subsequent
depression, and increased again during the bull market of the middle
1950's. But the statistics do not establish a secular uptrend in the pro-
portion of the population participating directly in ownership of Ameri-
can industry over the past three decades.

However, the percentages in Table I understate the dispersion of
stock-ownership among economic units, because one stockholder often
carries the stock for the benefit of his family. Kimmel estimates that the
6,490,000 stock-owners in 1952 were distributed among 4,750,000 family
units, or 9.5 per cent of the 50,000,000 such units in the country.[12] The
Michigan Survey Research Center estimated that 8 per cent of all spend-
ing units held stock in 1955.[13] Roughly speaking, we may say that about
one out of ten families own stock—a significant proportion, but hardly
large enough to justify assertions of the *general* participation of the
population in ownership of the means of production.

Blough and Kaplan drew unwarranted conclusions concerning dis-
persion of stock-ownership from their observation that the numbers of
stockholders in particular companies exceed the numbers of employees
of the same companies. They failed to take account of ownership of
stock in several corporations by the same person. Kimmel has estimated
that there were about 30 million *shareholdings* in American corpora-
tions, or more than 4 per individual stock-owner.[14] The latest estimate
of the total number of stockholders, 8,630,000, compares with
47,282,000 private employees in 1955,[15] of whom perhaps 35 million
were employed by corporations.[16] Thus, while the number of corporate
employees did not exceed the number of individual *shareholdings* in
corporations by a wide margin, it equaled about 4 times the number
of *stock-owners*.[17]

Stock-ownership remains very unevenly distributed among various
occupational groups and income groups. Kimmel found the per cent of
different groups owning stocks ranging downward from 44.8 per cent
of administrative executives and 19.4 per cent of operating supervisory

[12] *Ibid.*, p. 97.

[13] *Fed. Res. Bull.*, June 1955, XLI, 612, Table 3.

[14] Kimmel, *op. cit.*, p. 122.

[15] *Surv. Curr. Bus.*, July 1956, XXXVI, 19, Table 26.

[16] Estimated on basis that 74.5 per cent of compensation of private employees is paid
by corporations.

[17] References in this section are to stock-owners in publicly owned corporations, the
type relevant to the issue under discussion. By setting up a family-owned corporation, a
retail merchant becomes a stock-owner, but there is no real structural change involved.

officials to 1.4 per cent of semiskilled workers and 0.2 per cent of un-skilled workers. Among propertied, managerial, and professional classes generally, 13.4 per cent were stock-owners, while among employee classes generally, 3.5 per cent were stock-owners.[18] Similarly, 24.7 per cent of people in families with incomes over $10,000 owned stock, while only 1.4 per cent of those in families with incomes under $4,000 owned stock. The majority of the population was in the latter group, but it included only one-fifth of the stock-owners.[19]

While exact comparisons are not possible, owing to changes in the purchasing power of the dollar, the proportion of lower-income indi-viduals owning stock does not appear to be larger than estimated for 1927 by Berle and Means, or for 1937 by the TNEC.

A similar comparison is available for the number of employees own-ing stock in the companies for which they work. In 1927, according to National Industrial Conference Board figures cited by Berle and Means, 800,000 employees had become stock-owners in their employing companies.[20] Kimmel's estimate for 1952 was 780,000.[21] In view of the substantial increase in the number of corporate employees over the 25-year interval, it appears that there was a definite decline in employee stock-ownership. Presumably this resulted from the depression sale by workers under financial stress of stocks previously acquired under company stock-purchase plans. Since such plans have been revived recently, it is likely that the number of employee stock-owners has increased somewhat since 1952.

Kimmel's figures show 3.2 per cent of corporate employees owning shares in their employing companies. The percentage is much higher in specialized types of corporations, notably public utilities and finan-cial companies. For manufacturing, where two-thirds of the corporate employees are to be found, it is only 1.4 per cent.[22]

Thus, a substantial frequency of stock-ownership appears only in those occupational classes and income groups customarily associated with property ownership. Stock-ownership is still occasional, rather than typical, for workers, and rare for industrial workers. All but a handful remain separated from ownership of means of production.

II. Concentration of Stock-Ownership

The concept of an economic democracy based on stock-ownership requires not only that there be a large number of owners, but that a

[18] Kimmel, op. cit., p. 98.

[19] Ibid., p. 97.

[20] A. A. Berle, Jr. and G. C. Means, The Modern Corporation and Private Property, New York 1932, p. 59.

[21] Kimmel, op. cit., p. 113.

[22] Loc. cit.

large number own enough shares to have economic significance. It also requires that the situation should not be one in which a small number of stock-owners, by virtue of dominant holdings, are in a position of partial or total control.

A man owning a single share, or even 10 to 20 shares, of a typical industrial corporation, obviously has but a token stake in the ownership of the means of production. Consider the man who has invested $1,000 in corporation stocks. At recent yields, he might expect dividends of $40 per year. This is equal to about 2 days' wages for the workers in such industries as steel and automobiles. Even with allowance for possible capital appreciation, the return will not provide a significant addition to his living standard, nor represent a major degree of profit participation. After-tax profits of manufacturing corporations, in 1955, amounted to $702 per manufacturing employee, or 17½ times the dividend receipts of the $1,000 investor.[23] The $1,000 is a similarly small fraction of the total capital invested per worker in basic industries.

This represents, in fact, the typical situation of the comparatively small number of workers owning stock. The 1955 Survey of Consumer Finances reported 3 per cent of spending units headed by skilled and semiskilled workers owning some stock. The median amount owned was between $500 and $999. None covered in the survey (or too few to be recorded in the percentage table), had as much as $5,000 in stock. The figures were identical for unskilled workers. In the case of clerical and sales workers, 9 per cent held stocks, and the median holding was a little over $1,000. All of those reported as holding over $25,000 in stock were in the managerial, propertied, and professional groups.[24]

The Survey of Consumer Finances would indicate stock-ownership by about three-quarters of a million spending units headed by wage earners (skilled, semiskilled and unskilled workers), the figure swelled to a certain extent by the inclusion of foremen's holdings. Assuming a mean stockholding of $1,000, the total value of stocks held by all wage earners' families in the country came to something like $750 million. That was equal to 0.3 per cent of the marketable supply of stock in the United States.

The TNEC compiled, as of 1937, the main stockholdings of some wealthy families. For example the du Pont family was estimated to have $574 million in stock, the Rockefeller family $397 million, and the Mellon family $391 million.[25] Allowing only for publicly reported

[23] Computed from *Surv. Curr. Bus.*, July 1956, XXXVI, 17, Table 20, and XXXVI, 19, Table 25.

[24] *Fed. Res. Bull.*, June 1955, Suppl. XLI, 622, Table 19.

[25] TNEC, *Investigation of Concentration of Economic Power*, Monogr. No. 29, *The Distribution of Ownership in the 200 Largest Nonfinancial Corporations*, Washington 1940, Table 6, p. 116.

changes in these family holdings—and there is no evidence of their significant dispersal—by 1956 the value of holdings of the Rockefeller and Mellon families exceeded $3 billion each, and of the du Pont family $4 billion.[26]

In short, any one of these families—or more properly speaking, groups of related families—owned many times as much stock as all the wage earners in the United States. Indeed, the market value of Rockefeller holdings in a single corporation, Standard of New Jersey, was twice the market value of all the holdings of all American wage earners. Apparently the Corporation neglected to take this into account in preparing the pamphlet quoted above.

The distribution of shareholdings in Standard Oil (New Jersey) is revealing. As of 1938 the 100 largest stockholders of record had 12,584,000 shares, or 46.2 per cent of the total. Most of these were various holdings of the Rockefeller, Harkness, Payne, Pratt, Whitney, and a few other families.[27] On the other end of the scale, 103,626 stockholders, each with 100 shares or fewer, and comprising 79 per cent of all stockholders, had 2,302,000 shares, or 8.4 percent of the total.[28] While the number of stockholders has increased with the splitting of the stock, there is no reason to believe that there has been any material change in the distribution.

No stock is so widely dispersed as that of the American Telephone and Telegraph Co. But in 1937-39, the very small holders, with 1 to 10 shares each, numbered 358,000 and had only 9.5 per cent of the total stock; while the large holders with over 500 shares each numbered 2,478 and held 44.3 per cent of the stock.[29] Since the share of the 20 largest holders has increased since then, there is no reason to believe there has been any material reduction in this contrast.

The over-all picture of concentration of stock-ownership is equally striking. The staff of the Senate Committee on Banking and Currency deduced from the 1952 Survey Research Center report that 8 per cent of all stock-owners, comprising: "less than one per cent of all American families owned over four-fifths of all publicly held stocks owned by individuals."[30]

Butters, Thompson, and Bollinger made similar estimates as of 1949. They found that 50,000 spending units, or about one-tenth of one per

[26] V. Perlo, *The Empire of High Finance*, New York 1957, Table I, p. 45. Subsequent estimates by *Fortune*, Nov. 1957, LVI, 177, for individual members of these families are consistent with the cited figures.

[27] T.N.E.C., *Hearings*, Part 14-A, *Petroleum Industry*, Washington 1940, pp. 8031-34.

[28] T.N.E.C., Monogr. No. 29, Basic Statistical Data, Appendix III, Sheet 6, facing p. 242.

[29] *Ibid.*, Appendix III, Sheet 1, facing p. 242.

[30] Senate Committee on Banking and Currency, Staff Report, *Factors Affecting the Stock Market*, Washington 1955, p. 90.

cent of all spending units, owned over $100,000 of stock each. Their combined holdings were estimated at 65-71 per cent of the total of marketable stock outstanding. On the other extreme, 2,470,000 spending units, more than half of the total owning stock, had less than $1,000 each. Their combined holdings amounted to only 1 per cent of the total outstanding.[31]

These authors also estimated the distribution of stockholdings by family-income level. Naturally, the degree of concentration shown in this way was somewhat less, because not all of the largest stockholders are in the highest income group. Their minimum estimates of the concentration at the top were: 1 per cent of the spending units (with incomes over $15,000), held 65 per cent of the stock; one-half of one per cent of the spending units (incomes over $25,000) held slightly over 50 per cent of all stock; and one-tenth of one per cent of the spending units (incomes over $50,000), held 35 per cent of all stock.[32]

Prewar studies showed a similar concentration. Kuznets showed that there has been a decline in the percentage of concentration of dividend receipts among the top 1 per cent of the population, from 71 per cent of total dividends in 1929 to 53 per cent in 1948.[33] However, this estimate is not adjusted for the effects of changes in the tax laws on the methods of individual income reporting. Butters and associates, taking this into account, made the estimate for 1949, already cited, of 65 per cent, not far below Kuznets' 71 per cent for 1929. Qualifications cited by Butters and associates, and the reasonable range of statistical error, could account for the entire difference. At any rate, it is evident that the concentration of stock-ownership among a comparatively few individuals and families remains exceedingly great. The important stock-owners are numbered in the hundreds of thousands, rather than the millions. And the decisive stock-owners are numbered in the tens of thousands.

This conclusion is further supported by the specialized data available concerning current concentration in stockholdings within individual corporations. Information is limited to public utility and railroad corporations required to submit to government agencies figures as to the holdings of the largest stockholders of record. The data show, for most railroad and communication companies, an increase in the proportion of stock owned by the 20 largest holders of record since 1937. The 10 largest holders of stock in electric power companies in 1954 generally held a smaller share than in 1937, owing to the forced distribution of

[31] J. K. Butters, L. E. Thompson, L. L. Bollinger, *Effects of Taxation, Investments by Individuals*, Boston 1953, Table XVI-3, p. 382. This is the authors' estimate by the "residual method" which they regard as the "most reliable" of two methods used.

[32] *Ibid.*, p. 440.

[33] S. Kuznets, *op. cit.*, Table 123, p. 646.

holding company shares under the Public Utility Holding Company
Act. Examples, for the largest companies, are shown in Table II.

Study of comparable data for smaller railroad and power companies
shows that the results of Table II are representative. Unfortunately,
similar data are not available for industrial corporations for the postwar
period. However, the special legal conditions which caused a decline in
power-company stockholding concentration were not duplicated among
industrial corporations. The slight increase in concentration in Ameri-
can Telephone and Telegraph shares is noteworthy, since this is the
most popular stock of all.

TABLE II.—PERCENTAGE OF COMMON STOCK OWNED BY 20 LARGEST HOLDERS OF RECORD
SPECIFIED CORPORATIONS,[a] 1937 AND 1954

	1937	1954
Communications		
American Tel. & Tel.	3.8	4.2
Western Union	12.9	24.1
Railroads		
Pennsylvania	6.1	19.2
New York Central	23.7	42.6
Southern Pacific	15.0	15.3
Power companies (largest 10 holders)		
Pacific Gas & Electric[b]	25.1	10.0
Consolidated Edison	12.9	8.9
Commonwealth Edison	18.2	7.7

[a] The railroads and power companies shown are the three largest. 1954 statistics for Gen-
eral Telephone, 2nd largest communications company, are not available.

[b] Holders of common and preferred combined.

Sources: 1937 from TNEC Monogr. No. 29. 1954 from reports to the Federal Communica-
tions Commission, Interstate Commerce Commission, and Federal Power Commission.

The evidence, limited as it is, certainly gives no support for the
hypothesis that stock-ownership has become less concentrated than
prior to the second world war. The largest holders of record for com-
panies supplying such data are almost all financial institutions, and the
domination of institutions among the largest holders is more marked
than in 1937. This is consistent with the known fact that institutional
holdings of stock have increased relatively as well as absolutely.

III. *Institutional Stockholdings*

Goldsmith estimated that the share of financial intermediaries in
total domestic stock outstanding increased from 7.9 per cent in 1900
to 14.2 per cent in 1929 and 23.6 per cent in 1949.[34] I. Friend estimated

[34] R. W. Goldsmith, *The Share of Financial Intermediaries in National Wealth and
National Assets, 1900-1949*, New York 1954, Table 16, p. 69.

a further rise in the proportion of institutional holdings subsequently.[35] My estimate for 1954, on a basis not strictly comparable with Goldsmith's, is 33 per cent.[36] Nadler and others interpret the growth of institutional holdings as a further evidence of the widespread distribution of ownership of the means of production.

Nadler gives particular stress to stock-ownership by mutual institutions, apparently regarding it as a more important means by which the mass of the population participates in ownership of equity capital than by direct ownership. Examples he cites include life insurance companies, mutual savings banks, and mutual investment funds.[37] It is my contention, however, that the great bulk of financial-institution stockholdings are of a character that cannot by any stretch of the imagination be regarded as representing ownership by masses of the population; that institutional holdings, in fact, reinforce the extreme concentration of stock-ownership in the hands of a small minority in the upper-income brackets.

Table III shows the New York Stock Exchange estimate of distribution of institutional holdings of corporation stocks, as of the end of 1954. These figures are close to estimates prepared by the Securities and Exchange Commission. They compare with estimates of the total potential market supply of stocks of $250 billion and $268 billion.[38] Thus institutions accounted at the end of 1954 for about one-fourth of all stockholdings, strictly financial institutions accounting for 21-22 per cent.

Two facts stand out in Table III. The stock owned by institutions of a "mutual" character, in which large numbers of the general public have any degree of proprietary interest, is a small part of the total. Holdings of life insurance companies, most of which are mutual, mutual savings banks, mutual-fund investment companies, and pension funds total $12,960 million. Close to half of this, or $5,340 million, consists of the mutual-fund holdings. Since the stockholders in these are included in the estimated totals of direct stock-owners, they represent no addition to the numbers participating in ownership of the means of production. That leaves $7,120 million as the combined stockholdings of mutual institutions through which many millions of people, not otherwise involved in equity ownership, have an indirect beneficial interest, however attenuated. This sum represents a little over one-tenth of all institutional stockholdings, and roughly 3 per cent of all outstanding stock.

[35] Senate Com. on Banking and Currency, *op. cit.*, Table 3, p. 92.

[36] Perlo, *op. cit.*, p. 62.

[37] Nadler, *op. cit.*, p. 12.

[38] Senate Com. on Banking and Currency, *op. cit.*, p. 88.

TABLE III.—ESTIMATED INSTITUTIONAL HOLDINGS OF EQUITY SECURITIES, 1954
(million dollars)

Type of Institution	Market Value
Financial institutions:	
Bank-administered personal trust funds	$37,800
Fire and casualty insurance companies	6,460
Open-end investment companies (mutual funds)	5,840
Life insurance companies	3,400
Closed-end investment companies	1,450
Mutual savings banks	620
Subtotal	$55,570
Other institutions:	
Pension funds (noninsured)	3,100
College-university endowment funds	2,500
Foundations, religious, and other charitable organizations	5,100
Grand Total	$66,270

Source: U.S. Senate, Committee on Banking and Currency, Staff Report, *Factors Affecting the Stock Market,* Washington 1955, Table 6, p. 96.

The other notable feature is the domination of institutional stock-holdings by one type, the bank-administered personal trust funds. This situation has prevailed throughout the 20th century, although the extent of domination has fluctuated. The share of personal trust departments in total stockholdings has grown markedly: according to Goldsmith's figures, from 5.1 per cent in 1900 to 8.4 per cent in 1929 and 15.2 per cent in 1949.[39]

It is, however, doubtful whether the New York Stock Exchange estimate of 1954 holdings by trust departments, cited in Table III, is adequate. The periodical *Trusts and Estates,* which specializes in the study of trust departments of banks, estimated that stockholdings of the latter amounted at the end of 1954 to $62.6 billion.[40] Using the

[39] Goldsmith, *op. cit.,* Table 16, p. 69.

[40] *Trusts and Estates,* Feb. 1956, XCV, 100. For direct comparison with the Stock Exchange estimates, the *Trusts and Estates* figure should be reduced by $3.1 billion to take out the pension-fund holdings. The difficulty of making an accurate estimate of trust-department holdings of stock arises from the fragmentary character of published reports, compounded by the practice of carrying securities at ledger rather than market value. A large part of the securities held in trust have multiplied many times in value since their acquisition, and this is only reflected in the accounts when switches of securities are made, a practice which is comparatively rare because of tax considerations. The rein-vestment of dividends and the addition of new trust accounts at current market values correct for only a minor part of this bias. While both the New York Stock Exchange and *Trusts and Estates* have estimated corrections of this type, it is doubtful whether even the higher estimate of *Trusts and Estates* reflects the entire market value of trust depart-ment stock holdings.

Trusts and Estates figures, the personal trust departments of banks hold close to one-fourth of all corporation stocks, and by either estimate, they hold more than all other types of institutional investors put together.

Are these massive holdings an evidence of "People's Capitalism," or of a still greater concentration of stockholdings in the hands of the very wealthy than measured by their nonfiduciary holdings alone? In 1954 trust departments of national banks administered 289,000 personal trust accounts with $43.4 billion of assets.[41] The main concentration of personal trust business is in New York, and it is primarily handled by state banks, hence for the most part not included in the report of the Comptroller of the Currency. The Federal Reserve Bank of New York reported that in 1954, 83 banks in the 2nd Federal Reserve District handled 115,000 personal trust accounts with $48 billion of assets.[42]

From these figures it is clear that the enormous holdings of stocks and other properties by trust departments represent but a few hundred thousand accounts, quite large in average size. Since a single individual is often the beneficiary of several trusts, and since there are often a number of individuals in the same family for whom separate trusts are established, the number of families beneficiaries of these trusts may be but a fraction of the number of accounts.[43]

Moreover, these largely coincide with the largest direct stockholders. Butters, Thompson, and Bollinger found that among a sample of active

Confirmation of the superior reliability of the *Trusts and Estates* estimate is provided by the latest annual Federal Reserve Board survey of common trust funds (groupings by banks of smaller trust accounts of many individuals). At the end of 1956, 60.5 per cent of the market value of these funds was in stocks (*Fed. Res. Bull.*, June 1957, XLIII, 623, Table 1). While the common trust funds account for only a small proportion of total bank fiduciary holdings, there is no reason to believe that the proportion of stocks is higher in the common trust funds than in the much larger and older individual trust accounts. Indeed, Richard B. Chapman, president of the Trust Division of the American Bankers Association, has said that the proportion of common stocks in many of the older trusts has increased to from 70 to 85 per cent (*Trusts and Estates*, Sept. 1956, XCV, 813). Reports of the Comptroller of the Currency, and of various state banking departments, show that the total assets of personal trust accounts, at book value, exceed $100 billion. Obviously, the market value is much higher, and if stocks comprise anywhere near 60 per cent of the total, the *Trusts and Estates* estimate is justified, as a minimum.

[41] U. S. Treasury Department, *Annual Report of the Comptroller of the Currency, 1954*, Washington 1955, Table no. 17, pp. 121-22.

[42] Federal Reserve Bank of New York, *Mo. Rev. Credit and Bus. Conditions*, June 1955, XXXVII, p. 73.

[43] In recent years, there has been a move towards making bank trustee services available to smaller investors, through the use of common trust funds. By the end of 1956 there were 243 of these funds, holding securities worth slightly less than $2 billion for 93,000 fiduciary accounts (*Fed. Res. Bull.*, June 1957, XLIII, 622, 625). Clearly, this fringe service is still trifling in the total picture of bank fiduciary activities, and does not involve a significant part of the population.

investors, the proportion who were beneficial owners of trusts increased from 3 per cent among those with less than $25,000 of wealth to 48 per cent for those with over a million dollars of wealth. Millionaires with trusts had 47 per cent of their wealth in that form.[44]

Thus the effect of the main form of institutional stockholding is to *increase* the concentration of stock-ownership. Butters and associates recognized this, commenting in relation to their estimates of concentration in stock-ownership cited above: "These figures would all be several percentage points higher if stock managed by corporate trustees for individual beneficiaries were included in the total."[45]

The "several" percentage points may be quite substantial indeed. The authors' estimates of personal trust stockholdings preceded the publication of data by the New York Federal Reserve Bank revealing that the New York banks alone held as much in personal trust accounts as had previously been estimated for the entire country, and before the recent *Trusts and Estates* estimates of a higher value of personal trust stockholdings than others current.

IV. *Influence in Corporate Affairs*

Effective participation in the business life of the country requires not only the beneficial ownership of a corporation's stock, but also at least a minimum of influence in the affairs of the corporation. It is scarcely necessary to belabor the point that the millions of small stockholders, and even the hundreds of thousands of medium-sized stockholders, have not an iota of influence in corporate affairs. The great majority of small stockholders either ignore corporate business, or limit their participation to signing proxies sent to them by a committee of the Board of Directors. Even in those corporations with highly publicized annual meetings, only a tiny proportion of stockholders attend. In the case of General Electric, where the proportion is unusually high, it slightly exceeds one per cent.

Moreover, even if all of the small stockholders could combine their forces to influence corporate affairs—an unlikely event—their votes would be insufficient. The 94 per cent of all stockholders having $25,000 of stock or less have only 15-18 per cent of the total stock, and hence of the total votes.[46] As for the 55 per cent owning under $1,000 of stock, and with combined holdings of 1 per cent of the total, they are utterly powerless.

On the other hand, the 50,000 spending units with $100,000 or more of stock, and two-thirds of the total stock, or part of these 50,000, are

[44] Butters, *et al., op. cit.,* Tables XV-5, 6, pp. 362-63
[45] *Ibid.,* p. 440.
[46] *Ibid.,* Table XVI-3, p. 382.

the group in a position to have genuine influence in corporate affairs. Of course, comparatively few of these 50,000 are stock-owners in any given corporation. But it is from among these few that the dominant forces arise.

In 1937-39, the 20 largest stockholders of record in each of the 200 largest nonfinancial corporations owned altogether 31.6 per cent of the common stock and 30.47 per cent of the preferred stock.[47] In practical terms, these 20 very large holders, voting typically about one-third of the stock, exercise full effective voting control of the corporation. Thus Samuelson writes of large corporations: "The largest single minority ownership groups typically hold only about a fifth of all voting stock. Such a small fraction has been deemed more than enough to maintain 'working control'."[48] Reports of proxy contests during recent years make it clear that invariably both groups are led by those among the very largest holders. In short, contests are not between the small group of very large stockholders and the mass of small stockholders, but between rival groups of giant stockholders.

Concentration of control is furthered by the role of institutional stockholders, particularly the large trustee banks. Just nine New York City banks handled four-fifths of the city's personal trust business in 1954, and hence perhaps two-fifths of the national total.[49] These New York banks appear again and again among the 20 largest stockholders of record in the country's largest corporations in the prewar TNEC tabulations. The National City Bank appears in 8 of the 10 largest nonfinancial corporations. The Hanover bank, which published Nadler's pamphlet, appeared among the leading stockholders in 6 of the 10 largest nonfinancial corporations.[50] The resulting influence is expressed, among other ways, by particularly frequent representations on boards of directors. In 1955 five New York banks, among the leaders in trust business, each had interlocking directorates with scores of corporations having combined assets ranging from $45 billion to $70 billion.[51]

One can search far and not find a real representative of small stockholders among the directors of large corporations, whether a representative is defined as one having an occupation similar to that of the typical small stockholder (small business man, salaried employee, wage-earner), or being an official of an organization of any of these groups. Nor has there been any real change since the 1930's. Individuals have died off or retired, but the interests represented on the boards of

[47] T.N.E.C. Monogr. No. 29, Appendix IX, Tables 93, 94, pp. 601-2.

[48] P. A. Samuelson, *Economics*, 3d ed., New York 1955, p. 89.

[49] Federal Reserve Bank of New York, *op. cit.*, p. 74 and Perlo, *op. cit.*, Table 6, p. 70.

[50] Perlo, *op. cit.*, Table 11, p. 94.

[51] *Ibid.*, Table 12, p. 95.

such corporations as American Telephone and Telegraph, General
Electric, and most others are virtually the same as they were 20 years
ago, except for certain changes in the distribution of influence as be-
tween particular financial houses (as in the case of A.T. & T.).[52] Legal
requirements have reduced the financial directorates on the boards of
electric power companies. However, the conclusions of the Senate com-
mittee investigating the Dixon-Yates contract suggest that here also the
real system of control remains despite formal changes.[53]

V. *Conclusion*

The basic claim of "People's Capitalism," that the rank and file of
the population are becoming owners of the means of production in
American industry, is without foundation in fact. The widespread dif-
fusion of this theory signifies only the effectiveness of organized propa-
ganda.

[52] *Ibid.*, pp. 96-98, 102-106.

[53] Senate Committee on the Judiciary, *Monopoly in the Power Industry*, interim
report, Washington 1955, pp. 6-7.

THE PRODUCTION CEILING AND THE TURNING-POINT OF 1920

By KENNETH D. ROOSE*

A primary objective of this article is to examine the thesis that a production ceiling plays a crucial role in reversing the direction of economic activity at the top of the business cycle. It seems appropriate to investigate the relationship of a production ceiling to the turning-point of 1920 because demand forces were extremely strong and sharp price rises also occurred.

The basic argument is that the production ceiling is normally encountered first in the investment-goods industries. Thereafter further growth in demand for consumption-goods output depends largely upon lagged expenditures generated by the multiplier. Since these do not generate sufficient consumption, however, induced investment falls off and shortly consumption also declines.[1] This explanation of the causal factors in an economic turning-point seems unsatisfactory for 1919-20, because production of consumer goods reached its peak in November 1919, before that of investment goods, and began to decline markedly in February 1920, although prices and real consumption expenditures were still rising. Moreover, demand for investment goods, as reflected in new and unfilled orders, in many industries continued to rise for some months after production of consumption goods began to decline.

The data appear consistent, however, with a second type of production ceiling which, according to Hicks, may occur on rare occasions, primarily after big wars. At such a time a production ceiling may be encountered first in consumption-goods industries, and lack of adequate capacity in these industries then leads to further increases in production in the investment-goods industries. Under such circumstances inflation generally sets in before the check to the growth in consumption is able to exercise its customary restraint on investment.[2]

I. Capacity, Price and Production Data Summarized

There was considerable transitional unemployment in the months immediately following the end of the war. The National Bureau of

* The author is professor of economics at Oberlin College. Most of the research for this article was undertaken as a faculty research fellow of the Social Science Research Council.
[1] J. R. Hicks, *A Contribution to the Theory of the Trade Cycle*, London 1950, pp. 128-32.
[2] *Cf. ibid.*, p. 134.

Economic Research places the trough in economic activity in April 1919. Thereafter industrial production and prices rose sharply, the former reaching a seasonally adjusted peak in January and February 1920, the latter in May 1920. The peak for the reference cycle of the National Bureau of Economic Research was in January 1920. The decline which followed was one of the sharpest on record. Industrial production fell by almost one-third while wholesale prices fell by 45.3 per cent, or by more than their decline from 1929 to 1933. The reference-cycle trough was reached in July 1921.

Although economic developments in 1919-20 lend support to the less common version of a production ceiling, a critical question is whether production of consumption goods actually attained levels at which further expansion was limited by ceiling factors. This would appear to have been so, judged by the high levels of consumer-goods production. It seems much more likely, however, that individual industries rather than consumption goods in general were limited by capacity factors. Therefore as a major part of this analysis, sectional ceilings for individual consumption-goods and investment-goods industries were examined.[3]

Because of the difficulties in defining and especially in measuring capacity, it seemed best to rely on indirect evidence as to capacity levels of output. Since loss in production from seasonal factors might be measured by calculating the average deviations from the peak seasonally unadjusted rate of production, such a method might also indicate the deviations from latent capacity.[4] One characteristic of periods during which capacity has been strained is that the seasonal factor tends to drop out.[5] Hence when the deviation from the peak seasonally unadjusted rate of production is at a minimum, and there are other symptoms of maximum output such as price rises and reports of heavy demand and shortages, this may be taken as evidence that production is approaching a capacity ceiling. On the basis of this hypothesis, the seasonally unadjusted industrial production indices of the Board of Governors of the Federal Reserve System were examined for seven periods when it might be presumed that resources were relatively fully employed either generally or in selected areas: 1919-20, 1923, 1926, 1928-29, 1946-48, 1950-51, and 1952-53. For each period the average deviation from capacity was computed for the 6 consecutive months closest to capacity, which was defined as the monthly peak in the seasonally unadjusted production data.[6]

[3] Cf. ibid., p. 132.

[4] Simon Kuznets, Seasonal Variations in Industry and Trade, New York 1933, p. 348.

[5] This observation was made to me by Moses Abramovitz of Stanford University.

[6] In a few instances the 6-month period does not include the peak production month from which the average deviation is computed.

The results of this analysis are quite suggestive. Of the 30 production series so examined, auto factory sales,[7] cigar, paper and pulp, and furniture production had their minimum average deviations from capacity in 1919-20 (when compared with the other 6 periods); each was a consumption good. In addition to these 4 series, 3 other consumption series, shoe, wheat flour, and newsprint production[8] were at their next-to-lowest average deviation. However, no investment-good industry had its minimum average deviation from capacity in this period.[9]

Statistics on prices and production of individual commodities also point to early pressure on resource use in consumption. By the fall of 1919, many more prices of consumer goods than of investment goods had exceeded their earlier peaks. Likewise, by December 1919, more than one-half of the series on consumer-goods production were at peaks for the 1919-20 period compared with just over one-tenth of the series on investment-goods production.

Production and price developments support the general conclusion that output in 1919-20 was limited by capacity in a number of consumption-goods industries. Their orders placed heavy demands on the investment-goods industries which continued to expand. New orders for investment goods reached peaks in the winter months but the backlog of orders continued to build up because of capacity limitations, because of speculative price rises which masked the fact that real consumer demand was declining, and because of the railroad strike in April 1920 which added to the confused picture of supply. Economic developments in 1919-20 thus appear to conform to the variant of Hicks' model in which an encounter with the production ceiling in the consumption-goods industries sets off inflationary price rises accompanied by a boom and price rises in the investment goods industries as well.

It does not follow, however, that the turning point of 1920 resulted *primarily* from the limitations on output imposed by production ceilings. In fact, a number of relatively autonomous factors also exerted deflationary pressures on the economy: (1) construction declined early in the period, residential construction contracts awarded reaching a peak in July 1919; (2) the commodity net balance of foreign trade began to decline in the middle of 1919; (3) the budget moved from a deficit to a surplus during 1919; and (4) monetary policy was tightened in November 1919 and was made more restrictive through the winter and spring of 1920. Taken together these factors reversed the inflation-

[7] If curtailed production of April 1920 (the month of the railroad strike) is included in the 6-month period, auto factory sales shift into the next-to-lowest average deviation.

[8] A small component of paper and pulp production.

[9] Crude petroleum, an investment-good series, was at its lowest average deviation from capacity in 1927 and at its next-to-lowest average deviation in 1919-20 and 1952-53.

ary spiral of 1919-20 and precipitated the economy into the depression of 1920-21.

II. *The Empirical Results in Greater Detail*

Over-all production of consumer goods appears to have neared capacity levels. The index of production of consumer goods[10] at 113 in November 1919 was as high as at any time in the 'twenties and 'thirties except for its level of 114 in December 1925 and November 1928. Moreover, the production of consumer nondurable goods at 109 in November 1919 was not exceeded in the 'twenties or 'thirties and was equaled only in October 1926. Even the level of consumer durable-goods production of 130 in November 1919 was not exceeded until December 1923. In the eight months from March to November 1919, production of consumer durable goods increased by 80 per cent.

Table I summarizes the results of calculating the average deviations from capacity for 30 production series. The 4 series which had their minimum average deviation from capacity in 1919-20 are listed first. These industries, not including furniture, had a weight of 14.7 per cent in the industrial production index in the 1919-20 period. In order to economize on space, data on the 1926 and 1950-51 period are excluded from the table. An industry was not included for a given period if the peak rate of production in that period fell short of the peak rate of production in the preceding period by more than 10 per cent. Thus newsprint production and bituminous coal in 1929, and bee-hive coke production in the prosperous 'twenties were excluded by this test. In the period following the second world war by-product and bee-hive coke were replaced by coke in the index of industrial production. Tin consumption was not available separately. In 1946-48 tobacco other than cigarettes was far below 1945 levels. In 1952-53, wool textile production, cane sugar meltings, and tobacco other than cigarettes failed to come within 10 per cent of previous peak rates of production. The decision to classify a series as investment or consumption good was arbitrary. If the predominant use is in heavy industrial production the series was regarded as investment good. If the product often ends in consumer use it was classified as consumption good. In two cases, petroleum refining and gasoline production, no assignment seemed possible so the series were not used.

The average deviation from the seasonally unadjusted peak is a highly deductive test of capacity and subject to an unknown amount of error. Production itself may be evened out over time thus reducing the average deviation. In other cases, stand-by capacity may not have

[10] Unpublished monthly data from the Federal Reserve Bank of New York. The index base 100 equals long-term trend. Data are seasonally adjusted.

TABLE I.—PERCENTAGE DEVIATION FROM CAPACITY[a] OF INVESTMENT- AND
CONSUMPTION-GOODS PRODUCTION SERIES FOR SELECTED PERIODS, 1919–53[b]

	1919–20	1923	1928–29	1945–48	1952–53
Consumption Series					
Auto factory sales	3.8[c]	6.3	8.2	6.8	4.8
Cigar production	5.0	7.2	6.9	8.2	10.1
Paper and pulp	1.0	4.3	1.8	2.8	2.0
Furniture[d]	0.6	1.8	7.7	2.0	2.0
Newsprint production[e]	1.4	5.0	—[i]	1.7	4.5
Shoe production	4.8	8.7	12.7	4.4	8.1
Wheat flour	7.1	16.9	9.1	7.0	3.6[m]
Kerosene	5.1	4.5	4.1	9.2	8.8
Cotton consumption	4.0	3.6	3.4	4.3	3.5
Wool textile	3.7	3.3	2.6	1.9	—[i]
Leather tanning	4.8	3.4	4.5	11.8	4.6
Cane sugar meltings	8.9	3.8	7.2	13.2	—[i]
Meat packing	16.1	9.1	13.6	13.3	10.5
Cigarette production	13.8	3.0	4.7	9.2	8.0
Other tobacco	6.4	5.4	5.5	—[j]	—[i]
Newsprint consumption	6.7	6.3	5.8	6.6	6.5
Anthracite	7.2	5.5	14.2	3.5	19.2
Investment Series					
Pig iron production	6.7[c]	5.0	2.7	2.1	2.0
Open hearth steel	5.8[c]	5.7	3.8	3.0	2.3
Tin consumption	11.5	13.3	4.3	—[k]	—[k]
Lumber production	5.4	1.9	2.3	5.0	5.0
Stone, clay, glass production	5.5	3.1	4.0	3.6	2.3
Cement[f]	7.9	3.8	4.3	2.3	2.4
Lubricating oil	5.1	4.4	4.5	3.5	3.4
Fuel oil	2.6	—[h]	0.9	4.4	2.0
By-product coke	7.9	1.7	1.0	0.9[j]	0.4[j]
Beehive coke	7.5	—[i]	—[i]		
Bituminous coal	6.7	6.3	—[i]	5.6	16.2
Crude petroleum	1.2[g]	1.5	3.8	2.4	1.2
Zinc production	5.6	2.9	2.9	11.1	2.1

[a] Average deviation of six consecutive months that depart least from the peak monthly rate in seasonally unadjusted production.
[b] 1926 and 1950–51 excluded for space reasons.
[c] April 1920 excluded because of railroad strike but additional month added.
[d] Federal Reserve Bank of New York unpublished data for period before second world war.
[e] Component of paper and pulp.
[f] Component of stone, clay, glass.
[g] Lowest average deviation in 1927.
[h] Rising throughout the period.
[i] Peak more than 10 per cent below peak in preceding period.
[j] Far below 1945 levels.
[k] Not available.
[l] Replaced by coke.
[m] But almost 30 per cent below peak of 1946.
Source: Board of Governors of the Federal Reserve System, *Federal Reserve Index of Industrial Production*, Oct. 1943; *Fed. Res. Bull.*, selected monthly issues.

been brought into production even at high levels of output. Supply shortages for seasonal reasons or as a consequence of major strikes in the coal, steel, and railway industries may also have held output below true capacity levels in particular areas. Nevertheless, current accounts in 1919-20 stressed the extreme shortage of goods, and data on use of capacity obtained from various industry trade associations show a high degree of plant utilization in areas such as paper and board and newsprint production.

Timing of Peaks in Production and Prices

The first large rise in wholesale prices was in July 1919, one month after the first major rise in industrial production.[11] The second large rise in wholesale prices was from November 1919 to January 1920 while industrial production also rose rapidly from December 1919 to January 1920. This second price rise, occurring even as capacity problems beset the consumption-goods industries, placed further pressure on production in the investment-goods industries. Total industrial production was at a peak in March 1920[12] but was followed by the third and final large price rise from March to April 1920.

Peak levels of output were attained earlier in the consumption-goods industries. Production series for consumer goods reached their peaks a number of months before those of the investment-goods industries. Thus seasonally unadjusted data (presented in Table II) show that 54 per cent (15 of 28) of a sample of consumption-goods series were at peaks by December 1919, but only 11 per cent (2 of 18) of the investment-goods series; in May 1920, 79 per cent (22 of 28) of the consumption-goods series had reached peaks compared with only 44 per cent (8 of 18) of the investment-goods series.[13]

The period of peak production for automobiles was from February through August 1920 and for paper and pulp from April through September 1920. As these were the two most important series with minimum average deviations from capacity in 1919-20, the timing of their peak periods of production might appear to invalidate the thesis that capacity problems emerged first in the consumption-goods industries. However, paper and pulp production had risen to within 10 per cent of capacity by October 1919 while automobile factory sales had done so by November 1919. Moreover, all of the consumption series listed in Table I had risen to within 10 per cent of their peak rates of production by December 1919 compared with fewer than one-half of

[11] Seasonally unadjusted data.

[12] Durable goods production, seasonally adjusted, moved to a belated peak in August 1920.

[13] Approximately the same time pattern may be observed in seasonally adjusted data.

TABLE II.—TIMING OF PEAKS IN 46 SEASONALLY UNADJUSTED PRODUCTION SERIES, 1919–20

Consumption-Goods Series	Cumulative Total	Cumulative Percentage	Date of Peak	Investment-Goods Series	Cumulative Total	Cumulative Percentage
Leather tanning	1	4	June '19		0	0
Lavatory shipments / Sink shipments / Small wares shipments	4	14	July '19		0	0
Other tobacco / Cigar	6	21	Sept. '19	Stone, clay and glass	1	6
Set of cards	7	25	Oct. '19	Lumber	2	11
Newsprint consumption / Wheat flour / Cigarette / Worsted spindles	11	40	Nov. '19		2	11
Combs / Kerosene / Furniture / Fine cotton goods	15	54	Dec. '19		2	11
Meat packing / Cotton consumption / Woolen spindles	18	64	Jan. '20	Beehive coke / Tin consumption / Copper	5	28
Wool, wide loom / Wool, narrow loom	20	71	Mar. '20	Pig iron / Open hearth steel / Zinc	8	44
Carpet and rugs	21	75	Apr. '20		8	44
Shoe	22	79	May '20		8	44
Auto / Newsprint	24	86	June '20	Lubricating oil / Woodworking machinery / Petroleum wax	11	61
Cane sugar meltings	25 / 25	89 / 89	July '20 / Aug. '20	Asphalt	11 / 12	61 / 67
Paper and pulp	26	93	Sept. '20	Fuel oil	13	72
Bathtub shipments	27	96	Oct. '20	Cement / By-product coke	15	83
	27	96	Nov. '20	Bituminous coal / Industrial pumps	16	89
	27	96	Dec. '20	Crude petroleum	18	100
Anthracite	28	100	Feb. '21		18	100

Source: Board of Governors of the Federal Reserve System, *Federal Reserve Index of Industrial Production*, Oct. 1943; Federal Reserve Bank of New York, unpublished data, and U. S. Department of Commerce, *Record Book of Business Statistics*, Suppl. *Surv. Curr. Bus.*, Pts. 1-3.

the investment series.[14] It seems likely, therefore, that these .development-ments encouraged order-placing in the investment-goods industries.

If individual commodities are studied with respect to the timing of price rises,[15] 60 per cent (9 of 15) of the consumption-goods series had exceeded their previous peaks of 1918-19 or earlier by August 1919. Only 33 per cent (4 of 12) of the investment-goods series had experienced such a rise by that date. By January 1920, the turning point in

[14] Also a much larger percentage of consumption-goods series than investment-goods series had risen to within their average deviations from capacity by the beginning of 1920.

[15] The sample of price series is not identical to that of the production series.

the cycle, 73 per cent (11 of 15) of the consumption-goods series had exceeded their previous peaks but only 42 per cent (5 of 12) of the investment-goods series had done so. The prices of various building materials and foods behaved much more like those of the consumption-goods series than investment-goods series. One-half of the series on farm products had exceeded their earlier peaks by April 1919. In 3 of 6 cases, the building series had exceeded their earlier peaks by July 1919. These price developments are reviewed briefly in Table III.

TABLE III.—DATES IN 1919–20 BY WHICH PRICE SERIES EXCEEDED THEIR 1918–19 PEAKS[a]

Date	Consumption Goods		Investment Goods		Food and Related Products		Building Materials	
	Cumulative Total	Cumulative[b] Per Cent	Cumulative Total	Cumulative[c] Per Cent	Cumulative Total	Cumulative[d] Per Cent	Cumulative Total	Cumulative Per Cent
June '19	6	40	3	25	3	50	2	33
Aug. '19	9	60	4	33	3	50	3	50
Jan. '20	11	73	5	42	4	67	4	67
Aug. '20	13	87	7	58	4	67	6	100

[a] Two consumption series, 5 investment series, and 2 food and related products series never exceeded their 1918 or earlier peaks.
[b] Based on 15 series.
[c] Based on 12 series.
[d] Based on 6 series.
Source: U. S. Bureau of Labor Statistics, *Wholesale Prices, 1913 to 1927*, Bull. no. 473.

In summary, just as the production of consumer goods tended to reach capacity levels earlier so prices of consumer goods also rose rapidly before those of investment goods.

Expenditures on Consumer Goods and the Turning Point

With the strain on resources apparently more acute and appearing first in consumption-goods production, it is surprising to find that _____ goods began to decline in December 1919 _____. Contemporary _____ initiating this _____ current prices _____ however, data _____ ent st _____

store sales in real terms actually hit their peak in July 1919. It would appear, therefore, that as consumption-goods industries neared capacity levels in October-November, 1919[17] and tended to induce expansion of production in the investment-goods industries, real consumption expenditures continued to rise, but only slightly, to their peak in February 1920. Then even as consumption began to weaken, investment series neared peak levels. A major reason for the weakening in production of consumer goods, apart from scarcity of productive factors, may have been the growing inelasticity of price expectations.[18] From August 1919 to October 1919, wholesale prices of finished products and raw materials declined, while those of semimanufactures declined from August to September. Because of these price declines, manufacturers may have concluded that the rise in prices which commenced again in November 1919 would be of short duration.

New and Unfilled Orders and Stocks

A final bit of information relating to the timing of the 1920 turning point and the problem of capacity output comes from data on new and unfilled orders and stocks.[19] Among consumption goods there are only new-orders series, seasonally unadjusted, for bathtubs, lavatories, and sinks, each of which reached a peak in July 1919, small wares which was at a peak in December 1919, and fine cotton goods which was at its peak in April 1919. In general, seasonally unadjusted new orders for investment goods reached their peaks much later. In most instances, too, unfilled orders were at their peak in the spring or early summer of 1920 while stocks generally were at their minimum in the summer and early fall of 1920. Exceptions to this,[20] and consistent with the early strength in consumption, were the lows in department store stocks in January 1920 and in raw cotton stocks in August 1919.

[17] Rates of expansion in production of consumer goods slowed down appreciably in the fall of 1919 as peak levels of production were reached, which is consistent with the that capacity factors began to limit output.

[18] For comments
Thomas Wilson, F

[19] Department st
System Fed. Res.
Record Book of

[20] Iron

RICARDO AND THE 93% LABOR THEORY OF VALUE

By GEORGE J. STIGLER*

> Mr. Malthus shows that in fact the exchangeable value of commodities
> is not *exactly* proportioned to the labour which has been employed on
> them, which I not only admit now, but have never denied.
>
> Ricardo, *Works*, II, 66.

Did Ricardo have a labor theory of value—did he believe that the
relative values of commodities are governed *exclusively* by the relative
quantities of labor necessary to produce them?

A considerable number of historians of economics have given a flat
affirmative answer to this question—a surprisingly large number con-
sidering the fact that there is not the slightest basis for such an answer.[1]
In the course of their expositions one encounters quite remarkable
statements such as that Ricardo assumed that labor and capital were
in a fixed proportion in all industries,[2] or that "Ricardo . . . constantly
takes no notice of capital."[3] Presumably these writers did not have
access to Ricardo's *Principles*.

More careful historians of doctrine have recognized the several and
important departures from a pure labor theory that Ricardo emphatic-
ally made. There is, in fact, an almost continuous spectrum of interpre-
tations. At one uninteresting extreme some writers argue that Ricardo
simply forgot or did not understand the import of the qualifications he
made. A very important group has advanced the view that Ricardo
wished to hold a labor theory of value—Cannan heads his treatment:
"Ricardo's Attempt to Revive the Pure Labour Theory."[4] They hold
that under adverse criticism and honest self-examination, Ricardo was
gradually forced to introduce in successive editions of the *Principles* a
series of qualifications of increasing importance, so that in the end it

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[1] Some examples are E. Whitaker, *A History of Economic Ideas*, New York 1940, pp.
422-25; P. C. Newman, *The Development of Economic Thought*, New York 1952, p. 85;
Emile James, *Histoire sommaire de la pensée économique*, Paris 1955, pp. 88-89; and C.
Gide and C. Rist, *A History of Economic Doctrines*, 2nd ed., New York, n.d., p. 164.

[2] G. Myrdal, *The Political Element in the Development of Economic Theory*, Cam-
bridge 1954, p. 62; similarly W. Stark, *The History of Economics*, New York 1944, p. 36.

[3] J. K. Ingram, *A History of Political Economy*, New York 1897, p. 125.

[4] *A Review of Economic Theory*, London 1929, p. 172.

really was not a labor theory.[5] But Ricardo was not willing to abandon the theory completely: "his heart clung to the pure labour theory,"[6] or he made a "brave show" of "identifying, as far as might be, value and the amount of labour necessary for production."[7]

In the most sophisticated versions of this view, the formal exposition of Ricardo's theory is, as I believe, correct and complete in substance, but there persists a strong implication that Ricardo attributes more than quantitative importance to labor in determining values.[8] Thus, after carefully stating Ricardo's "modifications" of a labor theory, St. Clair says that Ricardo "sweeps them all into the waste-paper basket"; for "he never got rid entirely of the idea with which he started, namely, that labour is the only price exacted by Nature for her gifts."[9]

The only economists to argue at some length that Ricardo had a cost-of-production theory of value, so far as I know, have been Marshall, Diehl, and Viner—but who could wish for more comforting allies?[10]

The widespread misinterpretation of a leading doctrine of an economist of the first rank is not a product only of later viewpoints and knowledge, for it occurred already in Ricardo's lifetime. The present essay seeks to set forth precisely what Ricardo's theory of value was, and to examine the interpretation placed upon it by his leading contemporaries.

I. Ricardo's Theory of Value

Ricardo's formulation of his theory of value was much influenced by his desire to correct what he believed to be the major errors of Adam Smith's theory. For Smith the long-run value of a commodity equaled

[5] This "evolution" in Ricardo's thought was apparently invented by Hollander, "The Development of Ricardo's Theory of Value," *Quart. Jour. Econ.*, 1903-4, XVIII, 455-91. Sraffa has recently shown that it rests upon a misconception; *Works and Correspondence of David Ricardo,* ed. Piero Sraffa, Cambridge, Eng. 1951, I, xxxvii ff. (Subsequent references to the latter will be given simply by volume and page numbers.)

[6] Cannan, *op. cit.,* p. 177.

[7] Alexander Gray, *The Development of Economic Doctrine,* New York 1931, p. 177.

[8] In addition to O. St. Clair, one may cite A. C. Whitaker, *History and Criticism of the Labor Theory of Value,* New York 1904, Ch. 5, p. 130-31, J. Schumpeter, *History of Economic Analysis,* New York 1954, pp. 590-95, and H. Biaujeaud, *Essai sur la théorie Ricardienne de la valeur,* Paris 1934.

[9] *A Key to Ricardo,* New York 1957, pp. 40, 348.

[10] Marshall's discussion is in the *Principles of Economics,* 8th ed. London 1920, Appendix I. It elicited from the leading historian of the English classical economics the remark that "Marshall endeavours to show, in defiance of all evidence, that Ricardo never desired to put forward the pure labour theory of value" (Cannan, *op. cit.,* p. 177n). Viner criticized Cannan's position in his distinguished review of Cannan's book, *Economica,* 1930, X, 78-80. Karl Diehl's extensive, but only moderately detailed, account is in *Sozialwissenschaftliche Erläuterungen zu David Ricardo's Grundgesetzen,* Leipzig 1905, Pt I, pp. 1-50.

At least two other historians of doctrine asserted what I take to be the correct view,

its cost of production: the "natural" price of a commodity was the sum of the necessary payments for labor, capital, and land. A rise in the price of one of these factors, and in particular a rise in wages, would lead to a rise in the prices of the commodities in which the factor entered.[11] If the changes in values were to be more than nominal price-level changes,[12] however, this was clearly a superficial analysis: why should the relative values of commodities be affected in any systematic way by the absolute level of input prices? The organization, although not the content, of Ricardo's chapter on value can be interpreted as one which presses the criticism of Smith's theory to the utmost limits.

The analysis is limited to useful goods, produced in free competition, and the element of rent is temporarily put aside (and later shown not to enter into marginal cost). Ricardo begins with the simplest case: the commodities are produced by one type of labor alone, working perhaps on free land (I, 12 ff.) In this simplest case the relative values of commodities will clearly equal the relative quantities of labor necessary to produce them, and will be wholly unaffected by the absolute level of wages (no matter in what unit they are measured).

Consider next, with Ricardo, the case in which only labor is required to produce the commodities, but different types of labor are used in differing proportions (I, 20 ff.). The market will establish wage differentials corresponding to the differences in skill and training of the occupations, and "the scale, when once formed, is liable to little variation." Hence a rise of wages will affect the money costs of all commodities in equal proportion, and leave relative values unaffected. Ricardo did not consider the possibility that the relative amounts of skilled and unskilled labor employed to produce a commodity might change and hence its relative value would change; he could have asserted, however, that the relative value of the commodity will change only if the "common labor" equivalent of the original labor input changed.[13]

but did not argue it: J. M. Ferguson, *Landmarks of Economic Thought*, 2nd ed., New York 1950, p. 106; and W. A. Scott, *The Development of Economics*, New York 1933, pp. 108-13.

[11] *Wealth of Nations*, Modern Library ed., Bk. I, Ch. 7. Ricardo quoted as a striking example of this argument the passage: "By regulating the money price of all the other parts of the rude produce of land, [the price of corn] regulates that of the materials of almost all manufactures. By regulating the money price of labour, it regulates that of manufacturing art and industry. And by regulating both, it regulates that of the complete manufacture. The money price of labour, and of every thing that is the produce either of land or labour, must necessarily either rise or fall in proportion to the money price of corn" (*ibid.*, p. 477).

[12] And Ricardo was not inclined to make this exception since he had a commodity theory of money.

[13] But a closer analysis would have indicated that the wages of superior labor contain interest on the investment in acquiring skill, and therefore the relative levels of wages and interest rates enter into relative values.

It is evident that we can still retain the proposition that relative values of commodities are independent of the absolute level of wages (and profits) if, when each worker is equipped with fixed capital, we assume that the ratio of fixed capital to labor is the same in every industry, provided the capitals have equal durability. And this is Ricardo's next case (I, 26 ff.). At this point it is not made clear whether the fixed capital earns a net return: Ricardo views the fixed capital as previously expended labor, and says, correctly but unnecessarily, that "exchangeable value of the commodities produced would be in proportion to the labour bestowed on their production; not on their immediate production only, but on all those implements or machines required to give effect to the particular labour to which they were applied."[14] At a later point it is made utterly clear that the contribution of the fixed capital consists of not only amortization quotas but also interest on the investment.[15]

And this is as far as Ricardo could go in attacking Smith's theory. The next step, and analytically the final step, is to allow the ratio of fixed capital to labor to vary between commodities, and when this is done: (I, 30):

> This difference in the degree of durability of fixed capital, and this variety in the proportions in which the two sorts of capital [fixed and circulating] may be combined, introduce another cause, besides the greater or less quantity of labour necessary to produce commodities, for the variations in their relative values—this cause is the rise or fall in the value of labour.

The variations among commodities in the productive role of capital are classified as variations in (1) the ratio of fixed capital to labor (I, 34), (2) the durability of fixed capital (I, 31, 40), and (3) the rate of turnover of circulating capital (I, 37). A rise of wage rates relative to profit rates will lead to a relative rise in the values of commodities made with little fixed capital, or capital of short life, or with raw materials that turn over rapidly.[16]

This is of course a cost-of-production theory, and differs from Smith's theory only in the exclusion of rents from costs: "By cost of production I invariably mean wages and profits."[17]

[14] I, 24. Since the indirect ("hoarded") labor is in fixed proportion to the direct labor, exchangeable values are of course proportional to either part or the total.

[15] I, 39, where the case of a virtually perpetual asset is discussed.

[16] Ricardo's measure of value, a hypothetical product made by labor working with the average amount of capital, the capital being of average durability, and having an average "period of production," leads to the proposition that profits fall (measured in this unit) when wages rise, and the actual direction of movement of the values can be predicted; see my "The Ricardian Theory of Production and Distribution," *Jour. Pol Econ.*, June 1952, LX, 202-4.

[17] *Notes on Malthus*, Ricardo's *Works*, II, 42.

Ricardo believed that the changes brought about in the relative values of commodities by fluctuations in wages and profits were very small relative to those brought about by fluctuations in the quantity of labor (direct and indirect)

The reader, however, should remark, that this cause of the variation of commodities is comparatively slight in its effects. With such a rise of wages as should occasion a fall of one per cent. in profits, goods produced under the circumstances I have supposed, vary in relative value only one per cent.; they fall with so great a fall of profits from 6,050 l. to 5,995 l. The greatest effects which could be produced on the relative prices of these goods from a rise of wages, could not exceed 6 or 7 per cent.; for profits could not, probably, under any circumstances, admit of a greater general and permanent depression than to that amount.[18]

And so, although it would be "wrong wholly to omit the consideration of the effect produced by a rise or fall of labour [wages], it would be equally incorrect to attach much importance to it" and therefore in the remainder of the book he will "consider all the great variations which take place in the relative value of commodities to be produced by the greater or less quantity of labour which may be required from time to time to produce them" (I, 36-37).

I can find no basis for the belief that Ricardo had an *analytical* labor theory of value, for quantities of labor are *not* the only determinants of relative values. Such a theory would have to reduce all obstacles to production to expenditures of labor or assert the irrelevance or non-existence of nonlabor obstacles, and Ricardo does not embrace either view. On the other hand, there is no doubt that he held what may be called an *empirical* labor theory of value, that is, a theory that the relative quantities of labor required in production are the dominant determinants of relative values. Such an empirical proposition cannot be interpreted as an analytical theory, any more than the now popular view that the price level is governed by the wage level and the productivity of labor can possibly be defended as an analytical proposition.

This is not to say that Ricardo's analytical theory was correct, for it contained several important deficiencies. It excluded rent from costs, and even if the supply of land were fixed the rent a piece of land could yield in one use would be a cost to other uses. (Ricardo's practice of assuming that land was used to grow only corn obscured this point.) His theory was wrong in reducing all capital to previously expended labor plus interest; except in some irrelevant day of Genesis all capital has been made by the cooperation of earlier capital and labor and land. This view may have fostered his empirical judgment that labor

[18] I, 36. This passage underlies the title of this paper.

quantities were decisive, but one could have adopted (wisely or not) the empirical proposition even if he had a correct concept of capital. And of course if all commodities are not produced subject to constant costs, an explanation of relative values that ignores demand is simply inadequate.

II. *The Interpretation by Contemporaries*

Ricardo's *Principles* received very diverse reviews, ranging from the adulation of McCulloch to the reaction of one anonymous reviewer that the volume "contains no valuable information in point of fact, and very little good reasoning in point of doctrine."[19] This same diversity extended to the interpretation of his theory of value.

J. B. Say could find only a simple labor theory of value. In the notes he added to the French translation of the *Principles,* he observed:

> M. Ricardo does not appear to have included [in the contribution of machinery to the value of a commodity] the profits or the interest on the capital as constituent parts of the prices of commodities.

> M. Ricardo . . . teaches throughout this book that the quantity of labor necessary to produce a product is the sole element of its price, . . .[20]

Malthus, the other leading economist of the period, did not attribute a labor theory to Ricardo, but chided him for his language:

> If to this cause of variation [differences in rate of durability of capitals] we add the exception noticed by Mr. Ricardo, arising from the greater or less proportion of fixed capital employed in different commodities, the effects of which would shew themselves in a very early period of savage life; it must be allowed that the rule which declares "that commodities never vary in value unless a greater or less quantity of labour be bestowed on their production," cannot possibly, as stated by Mr. Ricardo, be "of universal application in the early stages of society."[21]

It should be noticed that apropos of this discussion, Ricardo says, "In all the observations of Mr. Malthus on this subject I most fully concur" (II, 58). The only difference between Mathus and Ricardo in their concepts of costs of production was that the former included and the latter excluded the rent of land. The real dispute between them

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[19] *The British Critic,* N. S. VIII (1817), 354. The reviewer continued: "He holds, for example, and this is the leading principle of his system that the price of all commodities brought to market, consists solely of the wages paid to workmen, and of the ordinary profits on the stock. . . ."

[20] *Des principes de l'économie politique et de l'impôt,* transl. by F. S. Constancio, Paris 1819, I, 28; II, 297.

[21] *Principles of Political Economy,* 1st ed., London 1320, p. 90; reprinted in *Notes on Malthus,* Ricardo's *Works,* II, 59.

centered on the proper measure of value, rather than on the determination of relative values.

When James Mill wrote his primer on the Ricardian economics, *Elements of Political Economy* (1821), he restated Ricardo's theory in substance. After stating a labor-quantity theory, he went on to explain at length that because of differences in capital-labor ratios in various industries the fluctuations in wages and profits affect exchange values. He concluded:

> It is evident, however, that though this difference in the ratios according to which the wages of two kinds of labour were exchanged, and the different proportions in which they were applied in the production of commodities, would, upon a rise or fall in wages, alter the relative value of commodities, it would do so, without in the least affecting the truth of the previous proposition, that quantity of labour determined exchangeable value.[22]

The brazen illogic with which this passage closes was questioned by Ricardo: (IX, 127):

> In page 76 there is a passage ending with these words "without in the least affecting the truth of the previous proposition," etc. etc. If a watch and a common Jack altered in relative value without any more or less labour being required for the production of either of them, could we say that the proposition "that quantity of labour determines exchangeable value" was universally true? What I call exceptions and modifications of the general rule you appear to me to say come under the general rule itself.

Mill forfeited all hope of entering the economist's heaven when, in the second edition, he retained the passage unchanged and then went on to compound the sin by turning the labor theory into a tautology: "If the wine which is put in the cellar is increased in value one-tenth by being kept a year, one-tenth more of labour may be correctly considered as having been expended upon it."[23]

Ricardo's other fervent disciple, McCulloch, treated him with greater kindness. He repeated Ricardo's analysis, and then, before embarking on his own argument that the increased value of wine or timber arising merely from the passage of time was due to labor, warned the reader:

> But Mr. Ricardo was inclined to modify his grand principle, . . . so far as to allow that the additional exchangeable value that is sometimes given to commodities by keeping them after they have been purchased or produced, until they become fit to be used, was not to be considered as the

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[22] *Elements*, p. 76.
[23] *Elements of Political Economy*, 2d ed., London 1824, pp. 97-98.

effect of labour, but as an equivalent for the profits the capital laid out on the commodities would have yielded had it been actually employed.[24]

The final disciple we shall notice is De Quincy. His exposition of the Ricardian theory took the form of a series of dialogues between himself and Philebus, an anti-Ricardian, and Phaedrus, a neutral. The debates went better, for De Quincy, than any in which I have ever been participant or spectator: De Quincy carried every point, no really embarrassing questions were posed to him; and his adversary capitulated handsomely after every sally.[25]

The dialogues were concerned with a defense of the proposition that a rise in general wage rates will not affect the relative values of commodities. Early in the discussion De Quincy asserts:

> The ground of the value of all things lies in the quantity (but mark well that word "quantity") of labour which produces them. Here is that great principle which is the corner-stone of all tenable Political Economy; which granted or denied, all Political Economy stands or falls. Grant me this one principle, with a few square feet of the sea-shore to draw my diagrams upon, and I will undertake to deduce every other truth in the science.[26]

And again,

> It is Mr. Ricardo's doctrine that no variation in either profits or wages can ever affect price; if wages rise or fall, the only consequence is that profits must fall or rise by the same sum; so again, if profits rise or fall, wages must fall or rise accordingly.[27]

The complications raised by different ratios of labor to capital in various industries are not considered.

There is reason for believing that De Quincy did not mean to attribute a simple labor-quantity theory to Ricardo, despite the explicit clarity with which this is asserted. The dialogues were never completed, and the complications may well have been postponed to these unwritten parts. In the later *Logic of Political Economy* (1844), De Quincy summarized the complications which Ricardo raised with respect to differing capital-labor ratios, and did not challenge their basic significance.[28] Yet the reader of the *Dialogues* would have received only the

[24] *Principles of Political Economy*, 1st ed., London 1825, p. 313. The qualifications arising out of differing capital-labor ratios are summarized on page 309.

[25] "Dialogues of the Three Templars on Political Economy," which appeared as three articles in the *London Magazine* in 1824; I use the reprint in *The Collected Works of Thomas De Quincy*, ed. by David Masson, London 1897, Vol. IX.

[26] *Ibid.*, p. 55; his italics.

[27] *Ibid.*, p. 60.

[28] Sec. VII; *Collected Works*, p. 196: "In this case, it can no longer be said that the prices of the resulting articles, according to the general rule of Ricardo, vary as the quantities of the producing labour: a disturbance of that law occurs."

account of the first approximation, in which capital (and various types of labor) are ignored.

Samuel Bailey's penetrating analysis of the value concepts of Ricardo and his contemporaries revealed with admirable clarity the carelessness, ambiguity, and dubious metaphysics that saturated this literature.[29] Yet this clarity was achieved partly by avoiding a real problem with which these economists were grappling: how can one measure the value of commodity A not merely in comparison with commodity B (the case Bailey studies) but in comparison with all other commodities? This latter problem, of which the isolation of monetary fluctuations is one instance, was surely the rationale of most of the discussion of a measure of value.

So far as Ricardo's theory of value is concerned, Bailey makes no charge that it is a labor-quantity theory.

> Mr. Ricardo, indeed, explicitly allows the influence of other causes, such as time, differences in the proportion of fixed and circulating capital, and inequalities in the durability of capital, by which he admits the value of commodities is liable to be affected. Notwithstanding these modifications, however, his followers continue to lay down the position of quantity of labour being the sole cause of value in the most precise and positive terms; not that they deny the exceptions, but they appear to lose sight of their existence, and frequently fall into language incompatible with their admission; . . .[30]

It may be added that in substance Bailey accepts Ricardo's theory of value, including the exclusion of rent from costs of production.

We may recapitulate this brief survey. McCulloch, Bailey and Malthus correctly understood Ricardo's theory to be a cost-of-production theory excluding rent, and De Quincy should probably be added to this group. The theory was understood as a simple labor-quantity theory by Say and Mill, and also by Torrens.[31] It is worth repeating that Ricardo accepted Malthus' analysis and rejected Mill's. The theory was more widely understood in its correct sense in Ricardo's time than in later times.

III. Conclusion

How did the misunderstanding of Ricardo's theory arise? Although Ricardo's exposition has been often and justly denounced, the main argument stands out clearly enough: it does not require great generos-

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[29] *A Critical Dissertation on the Nature, Measures, and Causes of Value*, London 1825.

[30] *Ibid.*, pp. 230-31.

[31] "Mr. Ricardo has pushed this principle still further, and contended, that in all periods of society, whether before or after the accumulation of capital and appropriations of land, the labour expended upon production is the sole regulator of value." *An Essay on the Production of Wealth*, London 1821, p. vi.

ity or deep subtlety to comprehend the main structure of his value theory—indeed he has suffered from overly subtle reading. The confusion over his theory has arisen from more fundamental sources.

In Ricardo's period several factors probably played a minor role in the confusion. Ricardo's two leading disciples, Mill and McCulloch, asserted a labor-quantity theory with all emphasis, although actually neither held such a theory to the extent of denying that fluctuations in wage and profit rates affected commodity values. Their expositions naturally colored the interpretation of Ricardo, even though McCulloch expressly indicated his disagreement with Ricardo. Another source was the vast confusion of the causes of value with the proper measure of value, and in Ricardo's first edition a pure labor measure of value was used.

The main source of the confusion, however, was probably the failure of economists to distinguish clearly between analytical and empirical propositions. Among economists who were not methodologically self-conscious, who did not systematically consider the necessary and sufficient conditions for an equilibrium, the distinction would seldom be remarked. Ricardo's emphasis upon the quantitative importance of labor tended to be read as an analytical proposition that labour quantities were the sole regulators of value.[32]

The failure to distinguish between analytical and empirical propositions has been a source of much misunderstanding in economics. An analytical statement concerns functional relationships; an empirical statement takes account of the quantitative significance of the relationships. When Marshall viewed the demand for a commodity as a function of its price, the prices of closely related goods, and of income, he was criticized by members of the Walrasian school for failing to recognize that all prizes in principle influence the demand for any commodity. This is a characteristic instance of the distinction in question: No Marshallian ever denied the existence of the formal relationships that were omitted; no Walrasian ever presented an empirical example of important error resulting from their neglect.

One further source of misunderstanding of Ricardo increased through time. His exposition was much influenced by his desire to refute what he deemed to be popular and pernicious fallacies, such as that a rise in wage rates increases all commodities' values, and that high money-wage rates lead to low profit rates. When these views dropped from sight the thrust of the chapter on value became more obscure, so the view

[32] The "philosophical" and "empirical" theories of value distinguished by Wieser and elaborated by A. C. Whitaker, *op. cit.*, bear only verbal similarity to the present distinction. In fact their "empirical" is my "analytical" theory, and their "philosophical" is either my "empirical" or a metaphysical theory of value.

could ultimately emerge that Ricardo was desperately trying to stave off for 20 pages the admission that labor requirements are not the only determinants of value.

Schumpeter asked why if Marshall's (and the present) interpretation of Ricardo is correct, there should have been any controversy—would it then not amount simply to the current cost-of-production theory?[33] One is inclined to reply that there was no controversy, and that the controversy was about something else. There was in fact no active controversy over the so-called labor theory in Ricardo's lifetime. The main points of controversy were different. First, Ricardo eliminated rent from the costs of production, which was not in keeping with popular views. Second, he appeared to deny (but did not do so) that supply and demand governed value; in fact he considered this a wholly superficial view that merely postponed analysis of the real determinants of relative values, namely the factors governing supply.[34] Finally, the endless dispute between Malthus and the Ricardians concerned the measure of value, not its causes.

The basic reason Ricardo's theory is often misinterpreted is that it was often misinterpreted in the past. If a theory once acquires an established meaning, each generation of economists bequeaths this meaning to the next, and it is almost impossible for a famous theory to get a fresh hearing.[35] Perhaps one hearing is all that a theory is entitled to, but one may plead that Ricardo deserves at least a rehearing—his theory is relatively more widely misunderstood today than it was in his lifetime. One can build a strong case that the modern economist need not be acquainted with Ricardo's work, but there is no case for his being acquainted with an imposter.

[33] *History of Economic Analysis*, New York 1954, p. 594.

[34] "It is admitted by everybody that demand and supply govern market price, but what is it [that] determines supply at a particular price? cost of production." *Notes on Malthus, Works*, II, 45.

[35] Very occasionally a theory, unlike a dog, has its second day, as when Keynes persuaded many economists of the error of the century-long tradition that Malthus' criticisms of the full employment assumption of Ricardo were invalid. The example is the more remarkable because the tradition was correct and Keynes was wrong.

ADVANCES IN GAME THEORY

A *Review Article*

By HARVEY M. WAGNER*

What has happened to game theory since von Neumann and Morgenstern's monumental work first appeared[1]? Has it provided a tool with which to analyze neatly all problems of economic conflict? What misunderstandings and misapplications of game theory notions still persist? With commendable success, mathematicians R. D. Luce and H. Raiffa[2] furnish answers in a carefully written treatment of the subject. Their contribution serves both as an introduction to the rudiments of game theory and as a guide to the more esoteric facets of conflict analysis. Their treatment is only moderately technical, the more difficult parts being set aside in specially designated sections. Any economist interested in behavioral models of conflicts who has not kept abreast of game theory research over the last decade will certainly profit by reading this volume.

In the succeeding sections, we shall for the most part follow Luce and Raiffa's sequence of presentation; however, since they have chosen (*e.g.*, p. 253, n.) not to comment on the specific connections between their exposition and applications to economic theory, we shall make such indications where appropriate. Section I will be devoted to the conceptual framework of game theory models and some of the principal difficulties which must be overcome in a general approach to conflict analysis. In Section II the utility assumptions which are crucial for a systematic study of risky situations will be considered; and in Section III the important distinction between utility and money in a model of economic conflict will be examined. Sections IV and V outline the elements of two-person and *n*-person games. Section VI briefly discusses those themes which are common to modern statistical decision theory, group decision processes, and game theory. In Section VII we note some of the likely future developments in game theory.

[1] J. von Neumann and O. Morgenstern, *Theory of Games and Economic Behavior*, Princeton 1944 and 1947.

[2] *Games and Decisions: Introduction and Critical Survey*, by R. D. Luce and Howard Raiffa. (New York: John Wiley. 1957. Pp. xix, 509. $8.75. It is also available from the Library of Science, $5.95 to members.) All subsequent chapter and page references not otherwise identified are to this volume.

I. *The Game Theory Model*

Though there is no dearth of literature[3] on game theory, Luce and Raiffa are the first to state precisely in a language comprehensible to the non-mathematician all the axioms involved at each step of the analysis and to offer a critique of these assumptions.[4] Like any economic model, a "game" is an abstraction of real-life circumstances: Given the variables subject to each player's control, all possible eventualities in a conflict situation must be ascertainable. Nature may influence the game by operating chance devices at various stages. If Nature's probability distributions are known to the players, she assumes a dummy role; the potential outcomes are then specified in terms of the probabilities associated with each possible event. Otherwise, Nature is a bona fide player. Each opponent must choose from all the actions open to him, taking into account that his adversaries are doing the same thing.

As simple as the above characterization of a game may be, in this reviewer's opinion it seems to be one of the main stumbling blocks in an understanding of the theory. Luce and Raiffa carefully explain that a choice of action, or *pure strategy,* is not merely the decision to be made at a particular stage of play; rather, it is an entire prescription of what is to be done over the course of the game. For example, in a labor-management bargaining game consisting of a sequence of moves, a player's pure strategy is defined as one possible mode of behavior over the whole game. His strategy has allowed for every conceivable move that his opponent might make, and he has accordingly devised a counter move. Once both players have selected such comprehensive strategies, an umpire (in their absence) could match one choice of action against the other and thereby determine the outcome.

What are the drawbacks of this aspect of the model? As Luce and Raiffa indicate (pp. 7-8), one problem is that the full scope of potential outcomes cannot always be ascertained. Nature, for example, may have choices unimaginable to the players.[5] A second, more subtle difficulty takes on its full significance when we come to the notion of a utility function. Since a play encompasses the entire sequence of moves which are to be made, a fortiori the grand conflict is encountered only once. If the players envisage that they are

[3] For example, D. Blackwell and M. A. Girshick, *Theory of Games and Statistical Decisions,* New York 1954; H. W. Kuhn, A. W. Tucker, M. Dresher, and P. Wolfe, editors, *Contributions to the Theory of Games,* I, II, III, Princeton 1950, 1953, 1957; J. C. C. McKinsey, *Introduction to the Theory of Games,* New York 1952; S. Vajda, *The Theory of Games and Linear Programming,* London 1956; J. D. Williams, *The Compleat Strategyst,* New York 1954. The last-cited book is not only exceptionally entertaining reading, but one of the few *precise* treatments available to the nonmathematical reader. Luce and Raiffa also cite an extensive bibliography.

[4] Luce and Raiffa devote Chapters 1, 2, and 3 to the fundamental assumptions which underlie the game theory approach to conflicts. These three chapters form a basis for all the special game situations which they subsequently consider.

[5] An illustration which occurs to us is that allocating funds to research and development (ranging from pure research to the drawing of final blueprints) may conceivably be such a game. The statistician's notion of a game against Nature, which is somewhat different, will be discussed in Section VI.

going to meet on the battlefield several times, then the supergame to be analyzed is the complete sequence of scrimmages.[6] We believe that economists have sometimes erred in their criticisms of game theory by not realizing that the game model by definition has already included time-sequence considerations. Because game theory purports to deal mainly with unique events, the usual notion of probability in terms of long-run frequencies must be modified. Finally, bringing in timing elements and admitting long sequences of moves may quickly render it impracticable to find all of a game's pure strategies; the amount of computation becomes overwhelming.[7] But economists in their behavioral models have never demanded that every contingency should be part of the structure. Luce and Raiffa therefore may rightfully ask their readers to examine patiently the attitudes of game theory, make any revisions which seem required for the purpose of advancing its development, and withhold final judgment until the theory has had sufficient chance to come to fruition.[8]

To keep the analysis within limits, they restrict themselves to games in which each player has a finite number of pure strategies.[9] In the two-person case, this assumption allows them to represent the conflict by the familiar game matrix (*i.e.*, by a table or rectangular array) in which the rows correspond to one player, the columns to the other, and the entry at the intersection of the ith row and jth column shows the outcome resulting from one player selecting his ith pure strategy and the other his jth.[10] In addition to these axioms dealing with the structure of the game, each player is postulated to have a utility function representing his ordering of the various outcomes and to know every other player's utility function.[11]

We feel that this extreme assumption is what makes the problem of selecting a strategy so difficult.[12] Furthermore, the assumption proves vexing to the

[6] J. Marschak, "Rational Behavior, Uncertain Prospects, and Measurable Utility," *Econometrica*, Apr. 1950, XVIII, 111-41. If we define bluffing as a move that results in a willful foregoing of utility in order potentially to "make a killing" later on, then bluffing may be a part of one's action *within* the scope of a strategy in a supergame. But bluffing with respect to the selection of the over-all strategy is inconsistent with the notion of selecting a strategy which attempts to maximize *ex ante* utility over the period of play.

[7] It is well known, for example, that chess is a "trivial game." The rules assure that any play will not last more than a certain finite number of moves. Hence *conceptually* one could list every strategy (*i.e.*, prescription of how to move for each configuration of the board) and then select the winning (or nonlosing) course of action. Obviously such a complete listing is not forthcoming in the foreseeable future.

[8] Pp. 10-11. They draw an analogy to developments in theoretical physics which have often taken decades to make an impact.

[9] With the exception of Appendix 7, which is devoted to games with infinite pure strategy sets, and Appendix 8 which surveys sequential compounding of two-person games.

[10] Recall that if Nature is a dummy player, the "outcome" is really a lottery or a gamble of prizes, each occurring with known probability.

[11] Luce and Raiffa only touch upon the provocative idea of a game in which each opponent has a misconceived notion of his adversaries' utility functions (pp. 269-74).

[12] In contrast, Cournot duopolists, who adopt a naive attitude toward their plight, merely have to solve a straightforward maximization problem to determine their strategies. Once educated to the same extent as game theory opponents, they readily realize that a reappraisal of their former strategies is due.

analyst because even though he can exploit the property that the players enjoy mutual omniscience as to preferences for gambles, he must be cautious not to make interpersonal utility comparisons (the origin and scale of the utility functions are arbitrary). In our view, this warranted reluctance to make such comparisons is undoubtedly an important reason why a mathematical theory of conflict must inevitably leave many vital questions unanswered.

The definition of rational behavior in conflict situations has been an incessant cause of controversy between the followers and disbelievers of the theory. As is so often the case, a good part of the dispute has been terminological rather than substantive. To set the record straight, we see no disagreement between economists and game theorists on what a player *should* do (in the sense of furthering his own well-being, as he sees it) when he believes he knows how his opponents will play. Under such circumstances the gaming aspect disappears. In duopoly theory discussions, economists have called such knowledge "conjectural variation," and the economist's duopolist accordingly maximizes profits. The game theorist refers to "a priori" information, and his player in an exactly analogous manner selects a "Bayes strategy" defined by these a priori beliefs.[13]

In a particular class of conflicts, viz., two-person zero-sum games (to be precisely defined below), believers in the theory unreservedly offer a prescription for an "optimal" strategy. We think it is regrettable that most dilettantes in game theory never become aware that game analysis has been extended beyond the two-person zero-sum case. No wonder they are disillusioned with the theory, for the usual conflicts in economics are not in this simple category; and what is more, the "optimal" strategy applied to nonzero-sum games is often a nonsensical mode of behavior. Game theorists are anything but in agreement as to what should be defined as "rational action" or an "optimal" strategy in the nonzero-sum case. Why there is agreement at least in one instance can best be appreciated after we have discussed the game theorist's view of utility constructs.

As a prelude to Luce and Raiffa's exposition, we have constructed the following simple two-person game to emphasize the difficulties which must be faced in the analysis of conflicts:

1. Each player privately selects his strategy, *i.e.*, neither makes his choice known nor attempts to influence by bluffing, threatening, or similar tactics. An umpire receives the selected strategies from each participant and then announces the results.

2. Once each player has chosen his strategy, the final outcome is certain; this outcome may be a transfer of money, of goods, or a score, etc. Nature is not a party to the game, and neither player is sophisticated enough to envisage the possibility of randomizing (or using a probability device) in the selection of strategies.

[13] Loosely, a Bayes strategy is the selection of an action that maximizes "expected satisfaction," a concept which takes into account both the respective values of the possible outcomes and their alleged probability of occurrence. A more precise definition is given in Section II after we have defined a player's utility function for risky situations.

3. Each player has an ordinal utility function giving a consistent ranking of all definite outcomes which may arise in the game. As usual, such a utility function depends on each player's tastes.

4. If we list all possible outcomes (resulting from each pair of strategies) from best to worst as defined by the first player's utility function, then the same ordered list gives the outcomes from worst to best as defined by the second player's utility function.

5. When we describe the game by a pair of matrices whose entries are respectively player one's and player two's utilities of the outcomes, there is at least one saddle point in *both* matrices corresponding to some given pair of strategies.[14] The saddle point represents the pair of minimax strategies for the players.[15] Figure 1 illustrates the assumption. There may be more than one such saddle point. Note that the existence of a saddle point is not

Player One's Utilities   *Player Two's Utilities*

Player Two's Strategies   Player Two's Strategies

		T_1	T_2	T_2
	S_1	10	4	9
Player One's Strategies	S_2	0	1	6
	S_3	9	0	3

		T_1	T_2	T_2
	S_1	0	5	2
Player One's Strategies	S_2	10	8	3
	S_3	2	10	7

Saddle Point: S_1, T_2.

FIGURE 1

changed if one makes a strictly monotonic transformation of either individual's utility function; thus a saddle point is independent of the particular form of the utility function employed.

6. It is known that the opponent is using a minimax strategy.

The saddle point, or equivalently the player's own minimax strategy, is easily seen to yield the best outcome under the given conditions, and consequently it seems reasonable to define the minimax selection as the optimal strategy in this case. The assumptions are clearly restrictive and only a relatively few games meet the hypotheses. But we may no longer unequivocally prescribe the minimax strategy if any one of our assumptions is significantly

[14] Given player one's utility matrix where his strategies correspond to the rows and player two's strategies correspond to the columns, a saddlepoint occurs where a numerical utility is both the minimum in its row and maximum in its column. Similarly, in terms of player two's utilities, the saddle point occurs where a numerical utility is both the maximum in its row and the minimum in its column.

[15] In order not to confuse our exposition by having to distinguish between one of the player's minimaxing, and the other's maximining, we shall use the term minimax to denote a general strategy: a player considers for each and every one of his strategies the utility from the worst consequence of his using that strategy; a minimax strategy is one which yields the maximum of these utilities of the unfavorable events. In this section, we restrict ourselves to the use of pure strategies only; we relax this condition below.

relaxed. Let us assume that five out of the six restrictions hold, but one of the restrictions is violated for a particular game.

1. If communication between the two players is allowed, then either one might very well attempt to influence the other by threatening, promising, or deceiving.

2. If probabilistic uncertainty, either in the outcome or in the selection of strategies, is introduced, we need to extend our utility function to include rankings for risky events.

3. If an individual does not have a complete and consistent (*i.e.*, transitive) ordering represented by a utility function, the notion of an optimal solution which maximizes utility is ill-defined.

4. If there is not a clear and definite conflict of interests in terms of utility of the outcomes for each player, it cannot be demonstrated that player one, say, should use his saddle-point strategy, even though player two chooses his minimax strategy.

5. If the game does not have a saddle point and player two, say, uses his minimax (pure) strategy, it is not generally advantageous for player one to use his minimax strategy.

6. Finally, if player one, say, knows that player two is *not* going to use his minimax strategy, it is usually not advantageous for player one to choose his own minimax strategy.

Hence, if a single restriction is removed, we shall need some further assumptions in order to state once more that minimax is an optimal procedure in a two-person conflict. When we extend our scope to more than two persons, further serious conceptual difficulties arise. In reviewing what progress has been made in analyzing these problems, Luce and Raiffa logically start with the underpinnings of the entire theory—viz., probability notions and utility theory.

II. *Utility Axioms for Stochastic Events*

The stochastic or probability element enters game theory in the three places. First, given each opponent's pure strategy, the final outcome itself may be a random variable due to Nature's intervention. For example, the outcome of a pair of strategies might be to throw a die and to transfer an amount of money from one party to the other, the result depending on the particular random outcome of the throw. For another pair of strategies, a coin may be tossed, and some other monetary payoff is prescribed. Thus the entry in a game matrix can represent a "lottery" (a "gamble" or a "prospect" of random outcomes). It is erroneous to think of such an entry as always indicating a single nonrandom outcome. We can postulate extending the concept of a utility function to include an ordering of probabilistic situations. Then an entry in the actual game matrix for a player becomes the utility he attaches to getting the opportunity of playing the lottery. In the matrix characterization of a game, known as the *normal form*, two elements may have the same numerical utility value but may not imply that the same underlying event (a certainty or a lottery) will occur. The second place that probability may enter is in the case of a player choosing a randomized or mixed strategy. In other words,

the participant selects from his pure strategies according to some predetermined probability distribution. And finally, uncertainty may arise when the opponent's strategy is randomized according to a probability mixture which may be unknown to the player.[16]

As we have mentioned, long-run probability notions are not in accord with the single-play assumption. The "answer" to this conceptual problem lies mainly in the treatment of subjective probability. It is not a trivial problem to build a logical system on subjective probability, and here certainly is one of the controversial and difficult-to-comprehend aspects of game theory.[17] Most treatments of subjective probability are compatible with the usual numerical operations of probability theory. Thus, if the subjective probability of a head appearing upon the toss of a coin is $\frac{1}{2}$, the subjective probability of two heads appearing in two independent tosses is $\frac{1}{4}$.[18] Further, if an honest-to-goodness probability device is part of the mechanism of the game (e.g., numbered chips drawn from a bowl), subjective probability is assumed to agree with the usual objective definition of probability. The innovation of subjective probability is that it also applies to uncertain events which occur only once, and which perhaps are not even stochastic in orientation. For example, we sometimes speak of the probability of our taking a vacation within the next month, or of our getting a letter in the mail, or of our getting a raise in salary; it is unlikely in any of these illustrations that some human being is drawing a random number to determine the final outcome. Even in the case of coin-tossing we find it natural to say the *ex ante* probability of a given face is one-half, although we know that a single toss results *ex post* in either a head or a tail.

Adopting a humorous remark often made among statisticians in reference to the universal assumption of a normal distribution, we can say that mathematicians believe that economists have empirically verified the universal existence of a Bernoulli utility function,[19] and economists believe that mathe-

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[16] A special case is when the opponent picks a single pure strategy, with probability equal to one, but the player does not know which pure strategy the opponent will select.

[17] Pp. 36, 300-3, Appendix 1. Several papers on this subject are contained in *Proceedings of the Second and Third Berkeley Symposia on Mathematical Statistics and Probability,* J. Neyman, editor, Berkeley 1951, 1956; F. O. Ramsey, *The Foundations of Mathematics and Other Logical Essays,* New York 1950; L. J. Savage, *The Foundations of Statistics,* New York 1954; G. L. S. Shackle, *Expectation in Economics,* Cambridge 1949.

One important assumption which is made throughout game theory is that whenever stochastic situations enter, every possible event has been enumerated, and that the events themselves are mutually exclusive. Therefore we can be sure that one and only one of the events considered will occur. Usually a finite set of possibilities is presented. Mathematically, it is possible to solve some game theory problems for an infinite set of outcomes; needless to say, the mathematical complexities increase.

[18] Shackle's axiomatics (*loc. cit.*) do not fulfill the usual "laws" of elementary probability.

[19] A (Daniel) Bernoulli utility function for uncertain events is defined as follows: Given the utilities for a set of nonrandom events, the utility for a lottery with these outcomes, each occurring according to a given probability distribution, is numerically equal to the expected value (or probability-weighted average) of the utilities for the certain events. Therefore Bernoulli utility over uncertain events implies a linearly additive function of the component utilities for each possible lottery "prize."

maticians have rigorously proved the function's universal existence. Luce
and Raiffa give an excellent presentation of the von Neumann and Mor-
genstern type of approach to utility theory which yields a Bernoulli function
for stochastic events.[20] We point out that perhaps one of the most misunder-
stood facets of game theory is the essentiality of assuming such a function.
This is the tool enabling us to incorporate in the utility function a simple and
precise method for evaluating *any* alternative stochastic situation of the sort
just mentioned. The axioms needed are:[21]

Axiom 1. The individual has a preference ordering over all possible sto-
chastic situations (nonrandom events being a special case). Given any two
lotteries, the individual either strictly prefers one prospect to the other or is
indifferent between the two.

Axiom 2. The individual's preference ordering is consistent. If, according
to the tastes of the individual, lottery A is at least as good as lottery B, and
lottery B is at least as good as lottery C, then the consistency condition asserts
that lottery A must be at least as good as lottery C.

Axiom 3. If there are three lotteries, A, B, and C, such that the individual
strictly prefers A to B, and B to C, then there is some "superlottery," or prob-
ability prospect of *both* A and C, with A heavily weighted, that is strictly
preferred to B; and B is strictly preferred to some superlottery of *both* A
and C, with C heavily weighted.[22]

Axiom 4. If lottery A is at least as good as lottery B, then *every* super-
lottery (or probability prospect) of A and C, where C can be *any* lottery, is
at least as good as the *same* probability prospect of B and C.[23]

In recent years there have been various criticisms of the usual axioms of
preference leading to a utility function, most of the debates centering around
whether or not an individual can be assumed to possess a consistent ordering
of all certain situations.[24] As Luce and Raiffa note, such criticisms, of course,
can be made to apply to the generalized utility system for stochastic events,
specifically to Axioms 1 and 2.

In addition to establishing a type of continuity of preference by using the
intensity in a probability mixture as the independent variable, Axiom 3 denies

[20] Chapter 2. This chapter can be read independently of forerunning material, and in
this reviewer's opinion it is one of the highlights of the book. Also see I. N. Herstein and
J. Milnor, "An Axiomatic Approach to Measurable Utility," *Econometrica*, July 1953,
XXI, 291-97; J. Marschak, *op. cit.;* D. Blackwell and M. A. Girshick, *op. cit.*, pp. 102-20.

[21] Our presentation differs insignificantly from that of Luce and Raiffa; to permit ease
in exposition, we have sacrificed a slight amount of mathematical precision.

[22] An implication of the entire set of axioms is that there is some unique lottery of A
and C such that the individual is indifferent to this prospect and to lottery B.

[23] This axiom has been named by Samuelson "the strong independence property" and
by Savage "the sure-thing principle." P. A. Samuelson, "Probability, Utility and the
Independence Axiom," *Econometrica*, Oct. 1952, XX, 670-78; L. J. Savage, *op. cit.*, p. 21.

[24] K. O. May, "Intransitivity, Utility and the Aggregation of Preference Patterns,"
Econometrica, Jan. 1945, XXII, 1-13; N. Georgescu-Roegen, "Choice, Expectations and
Measurability," *Quart. Jour. Econ.*, Nov. 1954, XLVIII, 503-34; H. M. Wagner, "An
Eclectic Approach to the Pure Theory of Consumer Behavior," *Econometrica*, Oct. 1956,
XXIV, 451-66, and "Testing the Transitivity Axiom," *So. Econ. Jour.*, Apr. 1956,
XXII, 493-94.

the existence of prospects offering Heavenly Bliss or Eternal Damnation. The individual's utility function is bounded. For example[25], if the event C is to end in hell, and A and B are prizes of \$2.00 and \$1.00, respectively, then (contrary to the axiom) we might not be surprised to find an individual who prefers a dollar for sure, than any gamble between \$2.00 and hell, no matter how unlikely the latter possibility.

Some objections to the strong independence axiom have turned out to be due to a misunderstanding of its assertion.[26] To illustrate the meaning of the postulate, assume that prospect A is a three-to-one chance of getting two pounds of chocolates versus one quart of ice cream; prospect B, which by assumption is not better than A, is the four-to-one chance of receiving two quarts of ice cream or one pound of chocolates; and finally prospect C, which in fact may be any lottery we wish to consider, is assumed to be a one-to-one chance of getting two pounds of chocolates versus two quarts of ice cream. The axiom states that any lottery, say a five-to-one chance of getting A versus C, is at least as good as the lottery promising a five-to-one chance of B versus C. Why? The plausibility of the assumption is argued by recalling that in any case a lottery is to be played only once. For one superlottery, either prospect A will result *or* prospect C, but not both; and for the other super-lottery, either prospect B will result or prospect C. Furthermore, whether A, B, or C results, an amount of chocolates *or* ice cream, but not both, will be the final outcome. Since the odds for getting C in either superlottery are the same, and the component events are mutually exclusive, the individual should be indifferent in this respect toward both lotteries. Since the probability of getting A or B is also the same for each superlottery, the fact that A is at least as good as B should imply that the individual finds the superlottery with A at least as good as the superlottery with B.[27]

The axiom states nothing about an individual's behavior for repeated choices of the lotteries, where he could presumably store and redistribute his consumption of the prizes over a period of time. Therefore the axiom does not apply to situations where intertemporal "contamination" or any other actual mixing of the prizes can take place. Only a single play is envisaged and only a single lottery prize results.

Wold[28] has raised the question that if we restrict ourselves to a single play of a game over a "relevant" horizon, such a formulation may render the axiom incapable of ever being refuted by observable behavior. According to this line of argument, any single decision of a person can be made rational upon properly defining the "relevant" horizon. Thus the complete acceptance of

[25] We have slightly altered an illustration given by R. M. Thrall, "Applications of Multi-Dimensional Utility Theory," in *Decision Processes*, Thrall, Coombs, and Davis, editors, New York 1954, p. 185.

[26] H. Wold, G. L. S. Shackle, and L. J. Savage, "Ordinal Preferences or Cardinal Utility," A. S. Manne, "The Strong Independence Assumption, Gasoline Blends and Probability Mixtures," A. Charnes, "Note," P. A. Samuelson, *op. cit.*, and E. Malinvaud, "Note on von Neumann-Morgenstern's Strong Independence Axiom," *Econometrica*, Oct. 1952, XX, 661-79.

[27] We use the term "should" with the meaning of "it seems reasonable to suppose."

[28] H. Wold, *op. cit.*, p. 664.

the sure-thing postulate, even when correctly defined, remains an issue among critics of game theory.

A further result can be derived from Axiom 4 if we consider A indifferent to B, and B and C being (in every sense) identical lotteries. Then the individual is indifferent between B and any probability mixture of A and B, thus disallowing for a " love of gambling" motive.

If we agree to the four axioms as a suitable formulation for behavior, and a fortiori the relevance of a Bernoulli utility indicator, we can easily handle the three types of uncertainty which we specified above. The fundamental utility theorem states that our hypothesized rational individual behaves *as if* he maximizes expected utility for the prizes treated as certainties. Hence, once a pair of strategies is determined, if the individual faces the gamble of tossing a fair coin, the *ex ante* utility of this prospect is numerically equal to $\frac{1}{2}$ the *ex ante* utility of the outcome if a head turns up plus $\frac{1}{2}$ the *ex ante* utility of the outcome if a tail turns up. The full implication of the words "numerically equal" in the previous sentence must not be overlooked. The axiom system implies that if the individual through introspection and serious meditation contemplates the value of his utility function for the fair-coin gamble, he will arrive at a number which identically equals the expected value calculation. Therefore the Bernoulli theorem is no more nor less than a computing rule determining the *value* of various lotteries given the *values* of the component parts. The theorem does not state that the individual focuses any attention on the conceptual meaning of the "expected value of utility."

Letting each entry U_{ij} in the *normal form* game matrix for a particular player be the *ex ante* value of his utility for the corresponding pair of strategies, if the player randomizes his strategies, picking each with probability p_i, then for each of his opponent's strategies j, the player enjoys the *ex ante* utility $\sum_i p_i U_{ij}$. If his opponent randomizes his strategies with probabilities q_j, then his *ex ante* utility for the game is $\sum_j \sum_i p_i U_{ij} q_j$. Furthermore, given the opponent's randomized strategy q_j, the player's own Bayes strategy is defined as picking an action which maximizes $\sum_j U_{ij} q_j$. It follows that *given* the opponent's strategy, the player always has at least one pure strategy i which is optimal. Under such an assumption, the game problem collapses to a problem of simple utility maximization.

An important corollary of the theorem that our utility function is additive is that any positive linear transformation of an individual's Bernoulli utility function $aU + b$, $a > 0$, is permissible; in other words, the Bernoulli utility function is determined uniquely only up to origin and scale.

III. *Utility and Money*

Some of the remarkable confusions regarding game theory stem from the identifying of utility with money transfers, and Luce and Raiffa throughout are careful to maintain the distinction. If we find it to our liking to *assume* that money and utility are related by a linear function,[29] we of course are

[29] *I.e.*, $aU + b =$ monetary payoff.

at liberty to do so. But the stimulating articles of Friedman and Savage, and Markowitz[30] have led many economists to argue against such an assumption, except perhaps in expository presentations for beginners, or for a game with insignificant payoffs. It seems to us that much of an economist's potential interest in game theory might very well stem from the fact that the players do not have linear utility functions for money.[31] Although in particular instances, some games using linear approximations for the utility functions in the relevant range of monetary outcomes may be permissible (in any case, such an assumption should be made explicit), in general the identifying of money and utility contradicts the theorem that the utility function is bounded[32] (*i.e.*, Axiom 3).

It occurs to us that the argument against a linear relationship between money and utility provides additional insight for considering only a single play of the game. Imagine a lottery where a coin is tossed for nonnegligible money stakes for both players. At one level of income an individual's utility function may indicate that he should play the game, i.e., his *ex ante* utility for the game exceeds his utility for not playing the game. After a single play of the game, the individual is at another point on his income-utility function. Although the money outcomes and probabilities remain the same for the game, the utilities of the final outcomes may have changed sufficiently to cause the game to be a poor gamble. In short, the player moves from an income at which he is prone to gambling to an income at which he is prone to insurance. Therefore, what is a good strategy for the first play of the game may not remain desirable if the game is played a second time, somewhere beyond the current planning horizon of the individual.

We may draw a second important conclusion from the distinction of money and utility. We have stated that the individual's behavior is unchanged if we make any positive linear transformation of his Bernoulli utility function. If money and utility were linearly related, then an individual who finds a fair bet desirable at $1 stakes would also find attractive a fair bet at $1000 stakes, at a $1,000,000 stake, etc. We reject the hypothesis that such is generally true, and therefore, as Luce and Raiffa warn, we should be on guard against arguments aimed at game theory which are based on a confusion between utility of money and money itself.

IV. *Two-Person Games*

Slightly altering Luce and Raiffa's sequence of presentation, we begin our discussion of two-person games by citing a fundamental fact about all two-

[30] M. Friedman and L. J. Savage, "The Utility Analysis of Choices Involving Risk," *Jour. Pol. Econ.*, Aug. 1948, LVI, 279-304; H. Markowitz, "The Utility of Wealth," *Jour. Pol. Econ.*, May 1952, LX, 151-58.

[31] If both players' utility for money functions are linear, then there is a linear relationship between the two utility functions.

[32] The well-known St. Petersburg Paradox can be derived if monetary payoffs, which are assumed unbounded, are erroneously averaged rather than the bounded utilities of such payoffs. The St. Petersburg lottery is the chance of winning 2^n dollars (or more accurately, rubles) with probability 2^{-n}, $n = 1, 2, \ldots, \infty$; the expected monetary payoff is infinite, although the gamble is not too attractive (even in dollars).

person (and indeed n-person) games in normal form. Nash proved that there is at least one pair of strategies such that, if each player uses his strategy in this pair, the other player is not induced to change his own strategy in maximizing the expected value of his utility. Such a pair of strategies is said to define an equilibrium point.[33] An equilibrium strategy may entail the use of probability mixtures of the pure strategies, and assures each player of at least the utility he can expect from his minimax strategy (which also may involve the use of probability mixtures).

What are the ramifications of Nash's theorem? Just as an equilibrium set of prices in a Walrasian system need not be unique, there may be more than one equilibrium point in a game; furthermore, the expected utilities of the outcomes for the individual players vary with different equilibrium points. For some game situations (e.g., duopoly) it may turn out that there is a pair of strategies which does not define an equilibrium point but which assures each player a value of *ex ante* utility greater than that for any of the equilibrium pairs. Finally, if each player chooses his minimax strategy, the resulting pair of strategies need not define an equilibrium point.

Von Neumann and Morgenstern investigated in some detail a special case of a two-person game, which is most often presented in elementary expositions of the theory. Luce and Raiffa have termed such games as strictly competitive (Ch. 4; Append. 1 through 7). Let U_1 and U_2 be player one's and player two's utility functions, respectively, where the U functions are defined according to the axioms in Section II. Further, suppose there exists a positive linear transformation $aU_1 + b = U_1^*$, $a > 0$, such that $U_1^* = -U_2$; in other words, we postulate that it is possible to transform U_1 into U_1^* by a linear operation such that $U_1^* + U_2 = 0$. Then von Neumann proved in effect that (a) all pairs of minimax strategies are equilibrium points, and (b) every equilibrium point yields the same magnitude of utility to each player (*i.e.*, the same absolute value of utility).[34] Thus, in this special case, minimax and equilibrium strategies are equivalent.

Luce and Raiffa note that one of the most significant results of von Neumann and Morgenstern's study is that probability mixed strategies provide the key to the existence of equilibrium positions in conflict situations (pp. 170-171). In models where the opponents do not utilize mixed strategies, there is no way of guaranteeing that an equilibrium situation always exists. If there is to be a *theory* of games, and if it is inadmissible to assume an asymmetry between players which is ever present to resolve any conflict, then it is essential to establish that some strategies are available for *both* players which do not involve some sort of mutual contradiction.

When each individual's utility function for money is linear, a game in which the sum of the money payoffs is constant (*e.g.*, zero) falls under the assump-

[33] Pp. 106-9. J. F. Nash, "Equilibrium Points in n-Person Games," *Proc. of Nat. Acad. of Science*, 36, 1950, 48-49; "Non-Cooperative Games," *Annals of Math.*, Sept. 1951, LIV, 286-95.

[34] Von Neumann proved the magnificent theorem that under the above assumptions, not only do (a) and (b) above hold, but at least one equilibrium point exists in every such game; Nash, following von Neumann's keen insight, demonstrated the universal existence of equilibrium strategies for any n-person game in normal form.

tions of von Neumann and Morgenstern's analysis. But we are not required to assume that the money utility function is linear, and in any case it is the utility of money which is at the heart of the matter. Therefore we see no reason to suppose in general that the two players in a game need be *reluctant* to participate, for as we realize in the usual case of consumer behavior analysis, the utility function gives an ordinal scale and not one which indicates absolute measurements of pleasure and pain.[35]

When game theorists such as Luce and Raiffa attach the term "optimal" strategy to the minimax action in a zero-sum two-person game, they only mean by this that (a) there is no other strategy which can assure the player of doing better regardless of what his opponent does, and (b) if the opponent uses his minimax strategy, then the player can do no better than by using his own minimax strategy. Perhaps such criteria do not warrant the use of the word "optimal"; this is a matter of taste.[36] But in any case, there is no implication that if a player has a priori information as to what his opponent is going to do, he nevertheless should blindly play the "optimal" strategy. Consequently, in our belief the arguments of game theory critics that the minimax theory completely overlooks the chance of an opponent making a mistake are pretty much beside the point. What such arguments usually amount to is that a player has some a priori information or supposition that his opponent may choose a nonminimax strategy; if this is so, there is no question that the player should select his corresponding Bayes strategy. Furthermore, there are occasions when more than one minimax strategy exists, in which case it is possible to select that minimax strategy which will take advantage of mistakes.[37]

It seems to us that another empty refutation of the optimality of a minimax strategy is one which confuses *ex ante* with *ex post* utility. Needless to say, after the game is over, each player may reprehend himself for not having

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[35] D. Ellsberg, "Theory of the Reluctant Duelist," *Am. Econ. Rev.*, Dec. 1956, XLVI, 909-23.

[36] Although we have taken great pains to stress that the fundamental concepts of game theory are defined for a single play, a quasidynamic approach to the minimax theorem has been offered by Brown and Robinson in a framework which is related to Samuelson's Correspondence Principle. By postulating certain dynamic behavioral reactions, we are led to each player receiving his minimax value of utility in the "long run." The behavioral assumptions are identical in spirit to those made by Cournot for the duopoly problem. Each player assumes his opponent's strategy will remain the same (*i.e.*, as he has observed in the previous plays of the game), and then he chooses a Bayes strategy accordingly. If the pair of minimax strategies is unique, Robinson has shown that the process of repeated plays will produce long-run strategies converging to the equilibrium point; in any case there is a convergence to the minimax value of utility. The familiar arguments against Cournot behavior can similarly be applied here to shed doubt on the hypothesis that the behavioral assumptions are realistic, and a fortiori that the minimax strategy is ideal for a single play of a game. See pp. 442-46; J. Robinson, "An Iterative Method of Solving a Game," *Annals Math.*, LIV, 1951, 296-301; G. W. Brown, "Iterative Solution of Games by Fictitious Play," *Activity Analysis of Production and Allocation*, T. C. Koopmans, editor, New York 1951, Ch. 24, pp. 374-76. P. A. Samuelson, *Foundations of Economic Analysis*, Cambridge 1947, p. 284.

[37] Pp. 77-81. M. Dresher, *Theory and Applications of Games of Strategy*, RAND Corp., R-216, Dec. 1951, pp. 69-71.

acted differently. If player two uses a minimax strategy which randomizes among several pure strategies, player one *may* do better in the end by selecting a nonminimax strategy. But the ground rules of the game are that he must select his strategy before the play begins and with or without information about his opponent. Consequently, our utility axiomatization of the manner in which he views risky situations is an *ex ante* approach, which has the effect of weighting *every* possibility with its associated probability (subjective and/or objective).

A more subtle point lurks in the criticism that if an opponent plays his minimax strategy in a two-person zero-sum game, the other participant does not always need to play his own minimax strategy to obtain the minimax level of utility. We can find a parallel controversy in debates concerning revealed preference in standard consumer theory.[38] The point simply is whether it makes sense to distinguish "higher order" preferences among alternatives on the same indifference locus.[39] For example, in the game of Figure 2, each opponent's minimax strategy is to play the choices with fifty-fifty probabilities, assuring a utility valuation of 0 regardless of whether or not the other player

Player One's Utilities

Player Two's Strategies

		T_1	T_2
Player One's Strategies	S_1	−1	1
	S_2	1	−1

FIGURE 2

utilizes minimax. But if player one realizes that player two is going to randomize as stated, he may prefer the implied superlottery associated with his first strategy over that implied for his second strategy, according to a higher order differentiation of the two possibilities; and analogously for player two.[40] But a revision of each player's strategy destroys the equilibrium associated with the minimax selections. Thus, if *in fact* individuals do have a hierarchy of choice mechanisms among alternatives, then optimality criterion (b) above (p. 380) no longer holds (*i.e.*, if the opponent uses his minimax strategy, the player may do better by using something other than minimax on the basis of higher order criteria). Once the equilibrium property of the minimax pair of strategies is lost, the adjective "optimal" would be inappropriately associated

[38] I. M. D. Little, "A Reformulation of the Theory of Consumer's Behavior," *Oxford Econ. Papers,* Jan. 1949, I, 90-99. Also see references in footnote 24.

[39] In mathematical parlance, the question is whether the individual has a "lexicographic" ordering pattern. See R. M. Thrall, *op. cit.*, pp. 181-86, and M. Hausner, "Multidimensional Utilities," in the same volume, pp. 167-80.

[40] Note that we are still assuming the utility functions at *every* gradation of preference admit a zero-sum formulation. In cases where this premise is false, we would need to consider a complicated noncooperative game (defined below).

with the minimax action, for a player may be able to do better if his opponent plays "optimally" and he does not. We note that with few exceptions, econo-mists and game theorists alike have tended to avoid such issues.[41]

Once we leave the realm of strictly competitive games, problems multiply rapidly and every author feels entitled to his own concept of rational behavior. The situation is regrettable, although not surprising, for herein lie most games of interest in economic analysis. Luce and Raiffa (Ch. 5 and 6) follow Nash's subclassification of conflicts into noncooperative games, in which there is no preplay communication between opponents before the selection of strate-gies, and cooperative games, in which preplay activities are admissible. Two of their examples readily point up the difficulties in noncooperative games.

In Figure 3, both (S_1, T_1) and (S_2, T_2) are equilibrium strategies; that is, if either player were informed as to his opponent's choice, he would not be induced to revise his own selection.[42] But player one prefers the first equi-librium pair to the second, and vice versa for player two. The minimax strate-gies are $(\frac{2}{5}, \frac{3}{5})$ and $(\frac{3}{5}, \frac{2}{5})$ for players one and two, respectively, assur-

Player One's Utilities   Player Two's Utilities

Player Two's Strategies   Player Two's Strategies

		T_1	T_2
Player	S_1	3	0
One's			
Strategies	S_2	0	2

		T_1	T_2
Player	S_1	2	0
One's			
Strategies	S_2	0	3

FIGURE 3

ing each a utility of $\frac{6}{5}$. Notice that both players benefit more from any equilibrium pair than from their minimax strategies. Furthermore, the pair of minimax strategies are not in equilibrium; given that player one (two) chooses minimax, player two (one) is better off to select his second (first) strategy.

If a player could issue an ultimatum before the other did, then the in-determinacy as to which equilibrium pair would result might be resolved, pro-vided that such an act does not change the other player's utility function in such a way as to induce him spitefully to choose the "wrong" response.[43] Luce and Raiffa point out (p. 94) that even if rapport exists between the play-ers to the extent that without any physical communication they should agree that a "fair" solution would be to randomize between the two equilibrium points on a fifty-fifty basis, they are nevertheless thwarted in their goal be-

[41] Hausner and Thrall, *op. cit.*, discuss the possibility of a series of higher-order criteria, but in this reviewer's opinion they do not really face up to the basic problem.

[42] P. 90. The probability mixtures (3/5, 2/5) and (2/5, 3/5) for players one and two, respectively, also yield an equilibrium point.

[43] T. C. Schelling, "An Essay on Bargaining," *Am. Econ. Rev.*, June 1956, XLVI, 281-306. H. M. Wagner, "A Unified Treatment of Bargaining Theory," *So. Econ. Jour.*, Apr. 1957, XXIII, 380-97.

cause there is no randomizing probability mechanism over their own strategies that they can use noncooperatively to yield the desired effect.

Another type of difficulty associated with the equilibrium-pair notion (p. 95) is exhibited in Figure 4. In this case, each player considering his own utility function selects his second strategy, jointly yielding a utility of 1 for each player. These choices in fact comprise the only equilibrium pair. Yet it is mutually advantageous for them *both* to change to their first strategy, if they could be guaranteed against a "double cross."

Player One's Utilities   *Player Two's Utilities*

Player Two's Strategies   Player Two's Strategies

		T_1	T_2
Player One's Strategies	S_1	9	0
	S_2	10	1

		T_1	T_2
Player One's Strategies	S_1	9	10
	S_2	0	1

FIGURE 4

Cooperative games are built on the assumption that preplay messages are possible; any agreement arising at this stage is binding (*i.e.*, enforceable), and that each player's utility function is both known by the opponent and not altered in the course of negotiations. The first step in analyzing such conflicts is to delineate all possible outcomes including both those which are a result of noncooperative behavior (akin to the development of normal form pure strategies) and those which can be reached by agreements. Part of this characterization hinges on whether there exists a commodity commensurate with utility which is transferable by means of side payments. If transfers are allowable, game theorists seem to agree that the two players should settle on a solution which maximizes the total joint payoff and afterward resolve the thorny problem of dividing the spoils. Otherwise the consensus is that the settlement of the conflict should narrow to the Pareto optimal outcomes, where once again the particular point on the utility frontier is the problem at issue. In both cases the arguments mainly rest on normative considerations.

There is no need for us to repeat here the familiar reservations regarding the limiting of conflict solutions to Pareto optimal points. We also mention that game theorists apparently have overlooked that even if there are no legal prohibitions against side payments, the agreement to use such should be part of the game itself.[44] The noteworthy "advance" that more recent game theory has made over previous economic analysis is to suggest a single point on the Pareto locus which should be called *the* solution.[45] Luce and Raiffa describe in nice detail the theories of Nash, J. C. Harsanyi, L. Shapley, R. B. Braith-

[44] The reviewer is indebted to R. L. Bishop for this criticism.

[45] Of course, one notable exception to this statement is the commonly cited point of intersection of the players' offer curves. Another is F. Zeuthen's analysis in *Problems of Monopoly and Economic Warfare*, London 1930.

waite, and Raiffa (Ch. 6).[46] The various treatments purport either to characterize how people react dynamically in conflict situations, or to suggest a system of fair and reasonable principles by which the conflict should be arbitrated. With a sufficient number of additional axioms or behavioral postulates, the authors are able to find unique solutions to any conflict problems. As might be anticipated, the solutions themselves differ from theorist to theorist, and consequently even if one is wedded to the notion that economists should adopt a theory of conflict resolution, there is still a wide variety of theories from which to select. Luce and Raiffa briefly hint how such a selection might be made (pp. 121-124).

In addition to the ground rules adopted for cooperative games, the crucial aspects of all of the proposed solutions are the assumptions as to interpersonal utility comparisons and egalitarian principles, threat possibilities during negotiations, the range of alternatives to be considered pertinent, and the definition of a "no agreement" situation. As any economist who has tried to fit a model into game-theory terms knows, it is no simple matter to construct a game of anything but the crudest of economic conflicts, and even these present complications if a numerical solution is sought.[47] Nevertheless, it is our view that game theorists have made a notable contribution to the methodology of economics by demonstrating the possibility of resolving situations which seemingly have a multitude of outcomes by means of postulating the desirable characteristics of a solution. Like any axiomatic approach, the fundamental assumptions may always be criticized. But in our opinion game theorists have suggested a number of axiom systems which are sufficiently reasonable to warrant our concluding that the technique is legitimate as well as stimulating.

V. *n-Person Games*

In addition to all the difficulties which arise in two-person games, an entirely new realm of issues appears when a third person is added to the conflict. In the two-person cooperative game, the players may either agree or act independently. As soon as another player appears, there are the additional possibilities of each opponent coalescing with the new entrant, as well as a tripartite agreement. As more players are added, the number of potential combinations enlarges substantially. Once the coalition question is settled, there remains the problem of dividing the gains among the members of the team (provided transfers are permissible).

Luce and Raiffa devote one-fourth of their analysis to an up-to-date taxonomic survey of the *n*-person game literature, and these chapters comprise the least satisfactory section of their book (Ch. 7-12). Some of the fault lies in the lack of a persuasive theory for *n*-person games. The rest is due to Luce and Raiffa's departing from a style which they used so successfully in their other chapters. Very little motivation accompanies the various theories, simple examples are scarce, and the criticisms are often obscure. The authors discuss von Neumann and Morgenstern's characteristic function notion, ψ-stability,

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[46] Also see Wagner, "A Unified Treatment," *op. cit.*

[47] J. P. Mayberry, J. F. Nash, and M. Shubik, "A Comparison of Treatments of a Duopoly Situation," *Econometrica*, Jan. 1953, XXI, 141-54.

Milnor's and Shapley's axiomatic approaches, and in addition, a game-theory determination of voting power in legislatures, and the testing of n-person theory by means of laboratory experiments. In spite of the obvious importance of n-person games, relatively little research has been done in this area, and applications are few and far between.

A simple three-person example of Luce and Raiffa serves to illustrate the crux of the conceptual problems (pp. 199-201). Suppose that if each of the players, X, Y, and Z, act independently, they will receive a utility of zero; if any two or all three cooperate, the coalition will receive a total utility of one, and any player outside the coalition receives zero utility (assume there is a transferable commodity commensurate with utility). If, say, X and Y tentatively form a coalition in which each receives a utility of ½, then player Z can tempt X, say, into a countercoalition by promising him a utility of ¾ (Z would then get ¼ instead of his present zero). But once the new coalition is tentatively formed, Y can tempt Z into an agreement by promising him a utility of ½, etc. Even the possibility of a three-way agreement in which each opponent gets a utility of ⅓ does not produce a stable outcome. The various proposed "solutions" to such vexing examples as these comprise either a listing of all possible outcomes which are alleged to be reasonable (analogous to the narrowing of solutions to the Pareto locus), or a selection of an outcome which is uniquely determined by a set of axioms.

Two conclusions are readily apparent from Luce and Raiffa's summary. First, we feel that aside from illustrating the extreme difficulty of analyzing n-person games, the present literature is only of limited value to economists. Second, we conjecture future solutions will probably rely more on the dynamics of conflict situations. It is perhaps notable that fourteen years after the publication of von Neumann and Morgenstern's work, Hurwicz' remark is still pertinent: "The potentialities of von Neumann's and Morgenstern's new approach seem tremendous and may, one hopes, lead to revamping and enriching in realism a good deal of economic theory. But to a large extent they are only potentialities; results are largely a matter of future development."[48]

VI. Decision Theory

In their final two chapters, (13 and 14) Luce and Raiffa survey the fundamentals of modern statistical decision theory and K. J. Arrow's approach to group decision-making. Their thorough and clearly written presentation of these topics is sufficient reason to justify their treatment in a book mainly devoted to game theory. But in addition there is a significant parallelism in the methodology of game theory and of decision processes.

Modern statistical decision theory and game theory overlap at two points, viz., the statistician is assumed to enjoy a Bernoulli utility function, and many statistical problems are characterized by a normal-form game matrix. But differences between the two subjects are at least as important as the similarities. It is misleading to consider Nature, the second player in most statistical

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[48] L. Hurwicz, "The Theory of Economic Behavior," *Am. Econ. Rev.*, Dec. 1945, XXXV, 909-25.

games, as a willful opponent of the statistician, and certainly the zero-sum formulation is meaningless. Furthermore, it is possible for the statistician to "spy" on Nature's strategy selection by taking sample observations. In a manner clearly reminiscent of the axiomatic solutions to two-person nonzerosum games, various authors, including A. Wald, Savage, H. Chernoff, Hurwicz, Milnor, J. L. Hodges and E. L. Lehmann, have explored sets of postulates which lead to a unique decision rule for the statistician (pp. 278-309). The uncomfortable conclusion reached by Chernoff and Milnor is that there is no rule which will satisfy all of a certain set of presumably reasonable criteria. The resolution of this paradox is obvious, although disheartening; at least one of the allegedly reasonable properties must be foregone.

Luce and Raiffa make clear the fundamental controversy raging between the modern and neoclassical approaches to statistical inference (pp. 318-24). Decision theorists argue that the crux of any problem of inference is the final action to be taken, and this action carries with it a level of utility (or loss) depending on the true state of Nature. Once the statistician's utility function over final outcomes and a decision rule for uncertain situations are specified, the problem is essentially solved. Such an approach is sufficiently general to incorporate most of the neoclassical results as special cases. The counterargument to this approach is that in effect the generality is only a diversion; that in most real situations where the tools of statistical inference are used, it is not very helpful to speak of a statistician's Bernoulli-type function indicating a numerical loss for different mistakes. We believe that as with game theory proper, the future ascendency of statistical decision theory will depend largely on whether the approach suggests more meaningful ways to solve problems of empirical inference.

The connection between game theory and Arrow's *Social Choice and Individual Values*[49] is that group choice (in Arrow's context) might be viewed as an *n*-person game where the final outcome is selected by some aggregative scheme, such as voting. Arrow also uses the technique of suggesting a set of presumably reasonable properties which a method of aggregation should satisfy, and then demonstrates that there is no such scheme which obeys all the postulates. In addition to summarizing Arrow's arguments, Luce and Raiffa also survey the principal contributions which have followed Arrow's book, including those by L. A. Goodman and Markowitz, C. Hildreth, D. Black, C. H. Coombs, and May (pp. 345-57).

VII. *The Future of Game Theory*

Although Luce and Raiffa do not devote a particular section to the likely new developments and future influence of game theory on the social sciences, certain indications seem clear. The Bernoulli utility function will continue to provide the mainstay for the analysis of decision problems in stochastic situations. Although the minimax strategy may not be offered as "the optimal" rule for the selection of a strategy in the face of uncertainty, it will remain a procedure worthy of serious consideration in such circumstances. Develop-

[49] New York 1951.

ments in n-person theory are likely to be of a dynamic and behavioristic nature, perhaps embodying Marschak's notion of a "theory of teams."[50] Consequently, there will be a tendency to drop the normal-form abstraction of a game (append. 8). Of some interest to economists will be forthcoming research on games of economic ruin and market equilibrium.[51]

In our opinion, Luce and Raiffa (if not most game theorists) have up to now committed an error of judgment by overlooking the important insights from models of competitive economic behavior which have evolved over the last 120 years.[52] Luce and Raiffa have chosen to remain silent about any contributions falling outside the confines of mathematicians' analyses published after 1944. Nevertheless, the economist will find their book an excellent means of learning of the current status of game theory.

[50] J. Marschak, "Elements for a Theory of Teams," *Manag. Sci.*, 1955, I, 127-37. "Report of the Third Conference on Games," P. Wolfe editor, Princeton University, March 1957. O. Helmer, "The Prospects of a Unified Theory of Organizations," *Manag. Sci.*, 1958, IV, 172-76.

[51] Pp. 483-484. M. Shubik, *Competition, Oligopoly, and the Theory of Games*, forthcoming. L. Shapley, "A Symmetric Market Game," RAND Corp., RM-1533, 1955.

[52] Luce and Raiffa even describe Zeuthen's theory of warfare by means of a secondhand report, pp. 135-37.

THE DOLLAR SHORTAGE RE-REVISITED

A Review Article[1]

By C. P. KINDLEBERGER*

The year 1957 will go down in the history of literature on dollar shortage as a vintage one. The crop of books on the subject is a bumper one, including the four volumes under review and at least two others bearing on the subject, which are reviewed separately in this journal.[2]

The present books are diverse. Their authorship is preponderantly British (3:1), but in form they include a book of lectures, a monograph, a textbook and a thesis. More important, they represent a variety of analytical points of view and recommendations for remedy. For better or worse, there is no long study representative of the viewpoint of Hazlitt's *Will Dollars Save the World?*[3] or the preconversion Harrod of *Are These Hardships Necessary?*[4] For this position, if not for the polemics, one must go to the first chapter of Triffin or to Mikesell's testimony before the Boggs Committee with its quotable statement: "Dollar shortage should be regarded as a myth; it is only a cloak used to cover up the fallacies of governmental policymakers."[5] But if the spectrum is not so wide as to include this viewpoint, it is broad enough.

I. *Analysis of the Shortage*

MacDougall's monograph, which has been awaited for years, is a major work. Its subtitle, "A Study of International Economics," emphasizes that it is addressed to wider issues than persistent disequilibrium. Its structure is complex, difficult to follow in a straight reading: a text of 17 chapters, with footnotes on text; 47 appendices, with footnotes on the appendices. These

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[1] G. Crowther, *Balances and Imbalances of Payments*. (Boston: Grad. School Bus. Admin., Harvard University. 1957. Pp. vii, 70. $2.00.)

D. MacDougall, *The World Dollar Problem*. (New York: St. Martin's Press. London: Macmillan. 1957. Pp. xvii, 622. $12.50; 50 s.)

W. M. Scammell, *International Monetary Policy*. (New York: St. Martin's Press. London: Macmillan. 1957. Pp. xiv, 402. $9.00.)

E. Zupnick, *Britain's Postwar Dollar Problem*. (New York: Columbia University Press. 1957. Pp. xv, 256. $5.50.)

[2] S. E. Harris, *International and Interregional Economics,* New York 1957, reviewed on page 485, this journal. R. Triffin, *Europe and the Money Muddle,* New Haven 1957, reviewed this journal, March 1958, XLVIII, 188.

[3] New York 1947.

[4] London 1947.

[5] Subcommittee on Foreign Trade Policy of the House Committee on Ways and Means, *Foreign Trade Policy,* Washington 1957, p. 460.

last, to be sure, are minimal, but a reader, finding himself in an appendix footnote, having left the text for a text footnote where he is referred to an appendix, may be forgiven for pausing a moment to remember whence he started. But even if it does not recommend itself to him who runs as he reads, it remains a major contribution: analytical, empirical, judicious and restrained —rich in double negatives. It summarizes much of the earlier literature and fits it into place; it reviews statistical findings not only on balances of payments, elasticities, income elasticities and output, but also on productivity. Students will be mining its rich veins of fact and idea for years to come.

Discussing it with me, a friend objected that the chapter on remedies for the dollar problem was independent of the preceding analysis. But that is almost always the case. My own misgivings turn on the nature of the analysis.

MacDougall starts out with essentially the "absorption" approach. He examines the United States for possible deflationary tendencies, the rest of the world for signs of inflation-mindedness, and compares productivity in the two halves of the world. Having found no basis for a dollar problem in these relations, he proceeds to analyze what he calls the "structural changes," saying:

> Any of these [monetary and productivity] tendencies could cause a deficit but, provided they were not too pronounced, they might be offset by other changes, which we shall call "structural," favorable to the rest of the world's balance of payments. Equally, even if all the dangers mentioned were avoided, unfavorable "structural" changes could still cause imbalance (p. 145).

He then projects ranges of the possible variation in the significant items in the United States balance of payments for 1975, reaching a deficit of $4 billion on optimistic assumptions, but a surplus of $16 billion on pessimistic.

This partial-equilibrium technique of analysis is unsatisfactory. J. E. Meade was reprimanded by Ragnar Nurkse[6] for using domestic policies to get full employment, and flexible exchange rates to correct the balance of payments, without allowing for effects of changes in the balance of payments back on income and savings. Whether it is a national trait or not, MacDougall's ap̶p̶r̶o̶a̶c̶h̶ is similar to Meade's. How could the United States have an *ex ante* ̶ ̶b̶i̶l̶l̶i̶o̶n̶ from structural causes without savings outrunning in̶ ̶U̶n̶i̶t̶e̶d̶ States and investment in excess of savings abroad? ̶ ̶s̶h̶a̶r̶p̶ an economist to remain consistently on this ̶p̶a̶g̶e̶s̶ 346-47 he points out that a tendency to ̶a̶s̶ Germany or Switzerland, is of lesser ̶t̶h̶e̶s̶e̶ countries would have to save ̶a̶b̶o̶v̶e̶ domestic investment ̶n̶o̶t̶e̶s̶ that, if the rest of

of gold, he states (p. 402) that there would be "no guarantee that the increase in output would not be sold to the United States . . . so that the next crisis would merely be postponed."

MacDougall recognizes a number of asymmetries. Lowering prices abroad improves the rest of the world's balance of payments less than raising them worsens it (p. 333). This is partly a function of the assumed readiness of the United States to protect imports (which is not, by the way, examined with the scepticism applied to the world's easy generalizations about productivity differentials); and partly a consequence of this country's more plentiful supply of capital and technology which makes it easier for the United States to replace imports than for foreign countries (p. 334). Supply elasticities in the United States are high for foodstuffs. But in estimating supply elasticities in manufactures—very broadly, to be sure, and with proper humility because of the weakness of the data—MacDougall guesses that the elasticities in the United States and the rest of the world are each 5 (p. 572). I should have thought that another asymmetry existed here, as in import-competing industry, and that it was fundamentally related to the dollar problem: a higher supply elasticity with respect to price for United States exports than for those of the rest of the world. This has certainly been the case in the late 1930's, during the war and the immediate postwar period. It may not be specifically true of 1958, but it still seems to be a better generalization than that the supply elasticities are equal.

United States exports of manufactures are seen to rise from $7.7 billion average in 1953-55 to $17.7 billion on optimistic assumptions (for the rest of the world) and to $21.7 billion on pessimistic assumptions. This expansion is largely the result of the structural fact of the United States' ability to supply the "latest thing." In this context (pp. 202, 204) creeps back the "demonstration effect" which had been disposed of under the discussion of "living beyond one's means" (pp. 60-64). Moreover, the demand for these products abroad is inelastic with respect to price in the short run (pp. 211ff), and unlikely to be affected by devaluation.

The reviewer is disposed to ask whether a disequilibrium arising from the production of new goods by the United States and their purchase by, say, Europe is structural or monetary. Suppose the United States and Europe buys a new good. Equilibrium can be restored by income in the United States which is expended into imports; or a contraction of income in E the production of other exports or impo ourselves to Europe, resources do n into other occupations, or if th tic employment by an equilibrium? Is th

book is not mainly about the dollar problem but a spirited and well-written defense of the Keynes plan against the White plan. He is interested in expanding international reserves through a central-banking mechanism for central banks. The nature of disequilibrium is less interesting to him than that of the adjustment mechanism—the gold standard, flexible exchange rates, and the Bretton Woods and regional systems which have grown up. The exposition of these institutions is beautifully done in clear, straightforward prose with an occasional neat phrase (mostly jibes at the United States?— "carefully prepared and served to suit the delicate digestion of Congress," p. 139; "further pressure . . . served only to thicken the fog of abstract nouns surrounding the American explanation," p. 143). But his analysis of the dollar disequilibrium is elusive. There were three main reasons why prior to 1914 there was no sterling shortage: dependence on overseas commodities, large-scale investment overseas, and a structure of income-inelastic imports (which held up in depression) and income-elastic exports (which declined) (pp. 312-13). These conditions have not been duplicated by the United States. Beyond this, Scammell furnishes short-run explanations for particular United States surpluses: recession and restrictive import policy in the 1930's (p. 318), favorable terms of trade postwar (p. 322), and a gold price policy which "robbed the non-dollar world of one means of dealing with the American current account surplus" (p. 323), etc. He then returns to broad "structural" explanations—the constant rise in American productivity which means that "whether full employment is maintained in the United States or not, there is little hope of Europe competing effectively with the United States" (p. 339); the "tides of world demand" which for "at least twenty years . . . have been flowing towards the United States" (p. 346). One of his most interesting suggestions, tossed off in a footnote, but worth extended analysis, is that a significant difference between the United States and Britain during the periods of their economic supremacy was that the latter was being overtaken, as the United States is not, by faster rates of growth of productivity abroad (p. 356).

Scammell's main theme is the necessity for enlarging international reserves by amending the International Monetary Fund, which he attacks as a "mere mouthpiece by American economic power" (p. 394). (His book, it is important to say, was written before the 1956 revival of activity of the Fund in connection with the Suez monetary crisis, and he would possibly want to modify some of his remarks about its inactivity in the light of the record which he failed to anticipate.) He recognizes, moreover, that this action might not enable the scarcity of the dollar to be cured, and would supplement it with other lines of action. But we recur to questions of policy.

Zupnick's thesis suffers many of the faults of its genus. It summarizes material which has been summarized before; it is poorly written, with much jargon, a surfeit of adjectives and adverbs, and some solecism. It is studded with exasperating quasiquantitative remarks, such as:

> In the light of the recent empirical studies which show that American price elasticity of demand for British exports in the interwar period was extremely high, it is reasonable to assume that a sufficiently large tariff reduction, applied in a nonselective manner, would ultimately result in a

rather substantial increase in the volume of American imports derived
from the United Kingdom (pp. 116-117).

and

. . . the extremely large capital outflow imposed a rather heavy burden on
the United Kingdom (p. 151).

But the main difficulty is that Zupnick indulges, with MacDougall and Scam-
mell, in partial-equilibrium reasoning. He gives pride of place in causing the
British postwar dollar problem to the rise in the United States price level
and the dominant position of the United States oil companies. There are six
reasons in all; the other four include the inadequacy of primary production
abroad; the increased tempo of industrialization abroad; the failure of the
gold price to rise; and the failure of the United Kingdom to adapt to the
basic changes which occurred in the structure of the world economy. But
the first two stand out for him. "It is difficult to exaggerate the importance
of this conclusion" (p. 120).

It is easy to exaggerate the importance of the rise in the United States prices
and the dominant position of the United States oil companies as causes of the
British dollar problem. One could as readily argue that American prices were
too low if they let the British buy all those imports they could not pay for;
or, more reasonably, that the European demand caused the rise in prices rather
than the increase in prices the large expenditure. But even if the rise in prices
in the immediate postwar period had been an autonomous variable caused
by supply conditions, the essential problem is why Britain did not adjust
to it, and whether, if prices had been lower, all other items in the British
balance of payments would have been unchanged, to produce a *pro tanto* im-
provement in the balance. Zupnick has misgivings on this score when he
discusses the price of gold. On two occasions (pp. 130, 220ff), he recognizes
that a higher price for newly produced gold would mean more exports, but it
also might mean more spending. In the final analysis, improvement in the
British balance of payments requires an increase in income larger than the
associated increase in spending, and this would not be guaranteed by any
change in prices or in ownership of oil companies.

Geoffrey Crowther's book has all the virtues of a set of lectures by an Eng-
lishman to an American audience. It is polished and witty. Moreover, the
analysis rests at basis, although at times it seems to get away from it, on the
fundamental fact that balance-of-payments surpluses and deficits are the
mirror image of deficits and surpluses of domestic spending over income.

The first lecture sets out the relation of the evolution of the balance of pay-
ments to various stages of national growth, paralleling Shakespeare's seven
stages of man and running in terms of the difference between spending and
income. This analysis follows that apparently developed earlier in Crowther's
Outline of Money, first published in 1941. The third lecture on dollar scarcity,
however, tends to depart from absorption and to deal in terms of "natural
resources and the structure of different national economies" (p. 45):

It is difficult to believe that there can ever have been another case of a
country where the demand of the rest of the world for its products was

so urgent, and its demand for the products of the rest of the world so in-
different—where rises in price would choke off so few sales on the one
side and falls in price stimulate so few purchases on the other—as is the
case with the United States today (p. 48).

In industry after industry, it is not simply that the American machine
is better or cheaper, it is the only one obtainable that will do the job
(p. 46). There are so many American goods that the world wants, what-
ever they cost (p. 51).

But even if structure produces United States import demands of zero price
elasticity for price decreases and zero income elasticity for income increases,
and rest-of-world import demands for United States goods of zero price
elasticity for price increase and zero income elasticity for income decreases
—which MacDougall's careful investigation shows to be far from the
case—it is still true that a surplus on the part of the United States means
absorption less than income, and a deficit in the rest of the world absorption
in excess of income. Why does the job, which only the American machine can
do, have to be done? If foreign countries buy United States goods whatever
they cost, the explanation must be tied back into the impact on the propensi-
ties to spend as a whole, and not merely on imports.

II. *Policy Recommendations*

Zupnick is not particularly concerned with therapy. The three Britons are.
MacDougall is opposed to flexible exchange rates, in contrast with Scammell,
who opts for them. Along with Scammell, however, he favors raising the dollar
price of gold and additionally enlarging international reserves. But his main
reliance is on the maintenance of the machinery of controls and the readiness
to discriminate against United States goods if need be. Devaluation may be
inevitable, and presumably the appropriate medicine for deep-seated trends
against Europe. But it would be a mistake to dispense with the equipment
needed for exchange control designed to discriminate against the dollar. Until
reserves are built up to absolutely safe amounts, it may be necessary to apply
it while trying to decide whether an adverse movement is secular or merely a
fluctuation about a neutral trend.

Crowther's solution is two (non-Communist) worlds. He thinks it will be
possible to limit ourselves to two, since the adjustment mechanisms of income
and flexible exchange rates which will not work between the Eastern Hemi-
sphere and the dollar area, function effectively within Europe and between
Europe and the sterling area (for different reasons in the two cases, to be
sure). Two worlds, in his judgment, will be tolerable: the United States has
come a long way since Undersecretary of State William L. Clayton's and
Ambassador Lewis Douglas' insistence on freer trade to its present support
of the Common Market, the Free Trade Area and the Coal and Steel Com-
munity.

Scammell, finally, is not very clear whether his main policy recommendation
—the enlargement of international reserves—will in fact enable the dollar
problem to be cured (p. 357). Like Thorp in *Trade, Aid or What?* (Balti-

more 1954) he is for Trade, Aid and Everything. Recognizing that it is easier
to shoot down five (*sic*) separate assaults than a mass attack, he would go
after dollar shortage in Europe with a program of expanding exports to the
United States, earning dollars in multilateral exchange, reducing imports from
the United States, adoption of flexible exchange rates, higher United States
overseas investment, and a higher dollar price of gold. But his main interest,
as indicated, is to enlarge the volume of international reserves (apart from
the increased gold price). He passes over the basic difficulty of the Interna-
tional Monetary Fund which has been not its small size but its incapacity to
deal with trends, as opposed to fluctuations about trends. Scammell's remedy
is the same as Triffin's; but, while he recognizes the existence of a dollar
shortage, and the necessity to deal with it by other means, Triffin in his
enthusiasm for multilateral clearing is under no obligation to deal with a
trend because he does not believe it to exist.[7]

Serious objection may be raised against a point accepted by MacDougall
(pp. 263ff) and Scammell (p. 351) relating to capital movements as a
means for offsetting excesses and deficiencies of absorption. Following an
analysis developed by Walter Gardner, Randall Hinshaw, Walter Salant and
Evsey Domar, they worry lest foreign lending add to the dollar problem
rather than solve it by requiring a flow of dollars from the borrowing coun-
tries to the United States for interest and amortization. But this reasoning is
in error since it neglects increases in productivity in the borrowing country
and increases in income in the lending. Crowther is on stronger ground in
admitting that the theoretical case for lending is a sound one, but that the
practice bristles with difficulties (pp. 54ff).

Crowther also has the better of the argument, in my judgment, on the
relevance of tariff policy to the disequilibrium. Scammell wants tariff reduc-
tions as part of his broad attack on disequilibrium (pp. 340-41); MacDougall
asserts that the United States could "help most by a progressive liberalization
of commercial policy" (p. 379). A useful illustration of the superiority of the
Crowther position of irrelevance is furnished by the lack of impact of German
tariff reductions on her balance.

III. *Concluding Remarks*

How far have we come? Certainly a long way on the details: on the meas-
urement of the elasticities, on productivity in general (though not on the
introduction of new goods), and on the general theory of balance-of-payments
disequilibrium. But there is still much to be done to unify the theories of
income and price in international economics, to bridge the gaps in Crowther's
analysis leading from absorption to structure, and to amend the partial-
equilibrium approach used by MacDougall for the purpose.

I am still disposed to argue against Mikesell that there is no asymmetry
in the distribution of wisdom among governmental policymakers (at least

[7] Having mentioned Seymour Harris' book earlier, it may be appropriate to place it
among the others by stating that its analysis is eclectic and classificatory, and that it
eschews recommendations for coping with the dollar shortage (which it understands to
exist).

in so far as the United States is concerned, on the one hand, and the rest of the world ex-Germany and ex-Switzerland, on the other). I imagine, too, that he, Haberler, Furth, Triffin—to bring in more names mentioned in Harris' eclectic summary—would be prepared to bet with MacDougall and Crowther that there was greater likelihood that a given future disequilibrium in the balance of payments of the United States would involve dollar shortage than dollar surfeit. At the same time, however, it behooves MacDougall and Crowther to make a more general case for persistent disequilibrium than new goods, urgent demands, United States quickness to impose tariffs, natural resources, or a more mystical "economic strength." None of these considerations apply to Germany and Switzerland, except possibly tariff in the latter country. And in none can the connection between structure and the balance of payments be made unless one goes through and explains the relationship between spending and income.

I am persuaded that the disequilibrium is systematic. But interesting, provocative, and full of insight as the 1957 crop of books is, there is still room, in my view, for further work.

THE PERMANENT INCOME HYPOTHESIS[1]

A Review Article

By H. S. HOUTHAKKER*

The consumption function, once as controversial as any Keynesian innova-tion, is now almost universally accepted as a tool of economic analysis. It would be difficult to find a recent cycle or growth model of which it is not a cornerstone. Uncontroversial though the *notion* of a consumption function has become, about its mathematical *form* there is much less agreement. Yet the study of macroeconomic models has shown that the exact shape of the consumption function has a considerable influence on the stability properties of the model.

Two questions about the consumption function are of particular impor-tance. The first is whether consumption is proportional to income, so that the average and marginal propensities to consume coincide; Keynes himself had suggested that the average propensity would normally exceed the marginal propensity. The second question is whether income should be regarded as past, current or expected, or as some combination of the three.

Much empirical research has been devoted to these questions, especially to the first one. At first family budget surveys were the chief source of informa-tion, and the consumption functions derived from them seemed to bear out Keynes' postulate of disproportionality. The short time series available until 1940 led to much the same conclusion. Then, however, Simon Kuznets' estimates of savings going back to the end of the 19th century became avail-able and they presented quite a different picture, which was subsequently confirmed by R. W. Goldsmith's more elaborate calculations. If Keynes had been right the ratio of consumption to income should be higher 50 years ago than it is now. Yet the long-term estimates revealed no significant change in that ratio. This clear contradiction between the evidence from time series and from cross-section data cannot be resolved by casting doubt on the statistics; it calls for a more comprehensive theory which accounts for each of the two conclusions.

The proposed solutions to this contradiction have all taken the form of a reinterpretation of the independent variable in the consumption function. Closest to the original Keynesian formulation is Tobin's approach, which

introduces assets as additional explanatory variables.[2] A more radical departure, which took "income" to mean the relative income position of a household in its community, had already been defended by Dorothy S. Brady and Rose Friedman, J. S. Duesenberry and Franco Modigliani.[3] Of immediate relevance to the book under review was the point of view taken by Hamburger who, in an unpublished doctoral thesis (Chicago 1952) emphasized the relations between consumption and wealth.[4]

There is some empirical support for each of those approaches, but it can hardly be said that they have been definitely established. There is still room for another hypothesis, and the purpose of this most stimulating and closely reasoned book is to present one. Although the application to consumption is new, the principal ideas go back to some earlier work in which the author participated.[5]

I. *The Underlying Theory*

Milton Friedman's hypothesis is based on a division of both consumption and income into a permanent and a transitory component. By permanent income is meant the annual equivalent of the revenues which a person expects to receive during a long period of time (presumably several years, though not necessarily his entire life). Later in the book (p. 93) we are told that permanent income "is to be interpreted as the mean income regarded as permanent by the consumer unit in question, which in turn depends on its horizon and foresightedness." It will accordingly depend on his accumulated or inherited capital, occupation, environment, etc. The transitory component consists of unforeseen additions or subtractions to income, which are supposed to cancel out over the period considered and to be uncorrelated with permanent income.

Friedman's fundamental relation is between permanent consumption and permanent income; in fact the *ratio* between permanent consumption and permanent income is supposed to be dependent only on the rate of interest, the ratio of "nonhuman" wealth to income, and individuals' tastes and to be independent of permanent income. The sum of the permanent and the transitory component, which is the only magnitude we can observe from cross-section data for a single year, is called "measured" income (or measured consumption as the case may be). Moreover (and this is the crux of the hypothesis) the transitory component of consumption is assumed to be uncorrelated with the transitory component of income; measured consumption depends consequently on permanent income (with an error term), but not on measured income as such.

What this amounts to in less technical language is that the household plans

[2] See James Tobin, "Relative Income, Absolute Income, and Savings" in *Money, Trade and Economic Growth, Essays in Honor of John Henry Williams*, New York 1951, p. 135 ff.

[3] See particularly J. S. Duesenberry, *Income, Saving and the Theory of Consumer Behavior*, Cambridge Mass. 1949.

[4] See also William Hamburger "The Relation of Consumption to Wealth and the Wage Rate," *Econometrica*, Jan. 1955, XXIII, 1 ff.

[5] See Milton Friedman and Simon Kuznets, *Income From Independent Professional Practice*, New York 1945.

consumption over a fairly long period, on the basis of expected income during that period and that it will not diverge from its consumption plans because income in a particular year falls short of or exceeds expectations. The marginal propensity to consume out of transitory income would therefore be zero. The man who has a lucky day at the races does not buy his friends a drink, and the poor fellow whose wallet is stolen does not postpone the purchase of a new overcoat. In other words, the availability of liquid assets has no influence on consumption decisions.

This startling conclusion is derived in Chapter 2, principally from an analysis of the case of complete certainty. Friedman apparently does not share the doubts frequently expressed concerning the relevance, and indeed the logical consistency, of the assumption of certainty. Even so, some rather strange arguments have to be invoked before the permanent income hypothesis can be deduced. Thus on page 13 it is argued that a dollar spent in year one must be equivalent to a dollar spent in year two because "the things being compared are of the same stuff, differing only in dating," and the author finds it "hard to see any reason why this difference in dating should have an asymmetrical effect."

Other authors have not found it so hard to see why time introduces asymmetry. On page 167 Duesenberry is chided for maintaining the classical view that the higher an individual's income, the more attention he can afford to pay to future needs. Friedman comments that: "as is shown in Chapter 2, this analysis is, to say the least, most unsatisfactory on a purely theoretical level." In fact, Chapter 2 shows nothing of the kind; it simply evades the issue. The theoretical level, incidentally, is so pure that consumers are assumed to be not merely omniscient but immortal (p. 12, fn. 8). Friedman's argument implies also that the rate of interest on loans is zero, for the sum borrowed and the sum repaid are "of the same stuff, different only in dating."

The case of complete certainty is so remote from reality that the author's views on the matter might be thought unimportant. Unfortunately, this is not so, for Friedman's brief discussion of uncertainty amounts to little more than an assertion that uncertainty makes no difference. The discussion is unsatisfactory chiefly because it pays little or no attention to liquidity constraints. It is a matter of common observation that people are often unable to borrow in order to improve the time-shape of their consumption, even though their prospects might justify borrowing. This imperfection of the capital market is perhaps the principal reason why consumption can frequently not be adjusted to permanent income. Later in the book (p. 93, fn. 51) Friedman himself mentions this imperfection as one reason why permanent income might vary with age, but he ignores the point in his theoretical chapter.

The theoretical foundations of Friedman's approach, it will be seen, are anything but firm. Let us not forget, however, that in his well-known methodological essay[6] Friedman has argued that a theory should be judged

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[6] See his *Essays in Positive Economics*, Chicago 1953, Part I.

not by its assumptions, but by its conclusions. However this may be, we must now look at his empirical evidence.

II. *The Statistical Analyses*

(There are no data on permanent income, and to test the hypothesis it is necessary to translate it into observable terms. More particularly, the postulated relation between permanent income and permanent consumption has to be translated into a relation between measured income and measured consumption. This is done in Chapter 3, where it is shown that the elasticity of measured consumption with respect to measured income is equal to the fraction of the variance of measured income contributed by the permanent component of income. Thus if permanent income could be measured directly (in which case its variance would equal the variance of total income) the elasticity just mentioned would be equal to 1, hence consumption would be proportional to income. The greater the contribution of transitory components, the smaller the elasticity.

Although empirical analyses occupy most of the book, I will not devote much space to them in this review. Friedman's argument is mostly so intricate and subtle that a brief discussion could not do justice to it; but I will have more to say about the empirical evidence in Part III. It would be unjust, however, to omit an expression of admiration for the skill and insight which he displays throughout those chapters. Much of what he has to say is debatable, but all of it is thought-provoking and intelligent. In his ability to relate observations to hypotheses Friedman is without peer.

Chapter 4, perhaps the best in the book, presents the evidence in favor of the permanent-income hypothesis that can be derived from cross-section data. Chapter 5 performs a similar task with reference to time-series data. Together these two chapters make out a good prima facie case for the permanent-income hypothesis. On the whole that hypothesis does what it is intended to do: it agrees both with the disproportionality between measured income observed in the cross-section data, and with the proportionality observed in the time series data. In Chapter 6 the permanent-income hypothesis is compared to the relative-income hypothesis; it is shown that the two lead to broadly similar conclusions. Despite Friedman's evident sympathy for the relativists, however, he rejects the sociological contentions that are basic to their approach.)

I feel the more justified in refraining from detailed criticism of this empirical evidence because it has only an indirect bearing on the validity of the permanent-income hypothesis. The data are shown to be consistent with that hypothesis, but since they are also consistent with a number of other hypotheses, they do not confirm Friedman's hypothesis. To put it differently, the relation asserted between permanent income and permanent consumption is not identified in the data he considers.

In this respect Chapter 7, which is devoted to an estimation of the marginal propensity to consume from income data only, is of greater interest. As mentioned before, the elasticity of measured consumption with respect to measured

income is equal to the proportion of the variance in total income attributable to the permanent component. The latter proportion can be estimated if income data for a number of years are available for the same spending units. Such data are relatively spotty, but for what they are worth they indicate that the proportion for nonfarm families is between .7 and .85. Since regression analyses of consumption on income lead to rather similar coefficients Friedman regards this coincidence as strong evidence for the permanent-income hypothesis. Actually the possible range of the proportion is so wide, and the households to which the two sets of evidence refer are so different, that it is hard to be impressed.) In the one case where the two analyses are made on the same set of data (Table 20) the correspondence is anything but close, and it is useless to attribute this to deficiencies in the data.

III. *Additional Testing*

A more searching test is therefore needed. Such a test has in fact been proposed, but not performed, by Friedman himself (pp. 215-16). Actually he does not consider this test, or the others suggested in the same Chapter 10, to be necessary for confirmation of the permanent-income hypothesis, for in his opinion the evidence he has examined already provides sufficient confirmation. It will be clear by now that I do not share this view, and I am all the more grateful to Friedman for indicating a further testable implication of his hypothesis. Interestingly enough, his confidence in the favorable outcome of this test was premature, as we shall see.

The test now to be discussed is based on cross-section data, in which the participating households are classified into subgroups so as to reduce the variability of permanent income. To take a simple example, the households are classified according to the occupation of the head, and within each occupation group, according to income. It is then possible to calculate a regression of measured consumption on measured income for each of the occupations separately, and also for all occupations combined. According to Friedman the marginal propensity to consume out of transitory income is zero, and hence the elasticity of measured consumption with respect to measured income is equal to the proportion of the variance of total income that is accounted for by permanent income. Thus if we compute a regression for steelworkers only, the elasticity should be less than for the population as a whole, because there is less variation in permanent income amongst steelworkers than there is amongst people from all occupations combined. Since therefore the variability of measured income observed in the case of steelworkers must be due more to transitory components, the elasticity should be smaller than in the case of all occupations taken together. Extending this reasoning to multiple classifications Friedman points out that his hypothesis implies that the elasticity should be smaller the more narrowly defined the group of households for which it is computed.

The large-scale budget survey carried out by the U. S. Bureau of Labor Statistics in 1950 and published jointly with the Wharton School of Finance

TABLE I.—MARGINAL PROPENSITIES TO CONSUME (UPPER FIGURE) AND INCOME ELASTICITIES (LOWER FIGURE) BY CITY CLASS, OCCUPATION AND AGE OF HEAD, U.S. URBAN CONSUMERS, 1950

Occupation	North			South			West			All	Age of Head			
	Large	Suburban	Small	Large	Suburban	Small	Large	Suburban	Small		25-34	35-44	45-54	55-64
Self-employed	.438 / .527	.648 / .692	.562 / .463	.512 / .565	.535 / .576	.822 / .537	.582 / .464	.424 / .386	.437 / .469	.505 / .523	.391 / .304	.595 / .588	.421 / .436	.549 / .645
Salaried professionals, officials, etc.	.591 / .744	.863 / .852	.622 / .757	.743 / .797	.507 / .643	.795 / .836	.623 / .741	.587 / .727	.678 / .812	.650 / .770	.703 / .756	.631 / .733	.742 / .796	.580 / .800
Clerical and sales workers	.724 / .754	.818 / .813	.812 / .922	.773 / .779	.875 / .749	.430 / .439	.790 / .743	.701 / .249	.418 / .489	.758 / .726	.808 / .836	.714 / .698	.778 / .665	.732 / .723
Skilled wage earners	.708 / .740	.613 / .598	.639 / .692	.919 / .875	.661 / .738	.787 / .603	.710 / .572	.891 / .877	.909 / .895	.762 / .740	.780 / .788	.794 / .714	.747 / .809	.746 / .640
Semiskilled wage earners	.834 / .833	.796 / .704	.983 / .888	.808 / .840	.942 / .928	.612 / .783	.876 / .796	.760 / .755	.754 / .752	.827 / .815	.713 / .753	.883 / .868	.822 / .822	.895 / .792
Unskilled wage earners	.841 / .721	.828 / .846	1.083 / 1.049	.890 / .911	1.020 / .903	.869 / .890	.817 / .805	.914 / .591	.943 / .796	.874 / .816	.914 / .960	.870 / .814	.854 / .787	.869 / .784
Not gainfully employed	.834 / .571	.686 / .546	.905 / .457	.827 / .659	.738 / .845	1.059 / .964	.639 / .541	.816 / .465	.630 / .288	.773 / .530	.981 / .651	.713 / .765	.767 / .503	.761 / .503
All	.596 / .679	.732 / .738	.667 / .628	.759 / .792	.637 / .775	.824 / .801	.696 / .682	.600 / .553	.557 / .557	.658 / .696	.713 / .740	.669 / .744	.617 / .669	.667 / .656
AGE 25-34	.695 / .720	.634 / .675	.576 / .689	.762 / .859	.817 / .869	.644 / .830	.797 / .615	.878 / .683	.570 / .717	.713 / .740				
35-44	.647 / .791	.725 / .811	.780 / .841	.792 / .817	.607 / .748	.941 / .575	.689 / .640	.481 / .591	.470 / .599	.669 / .744				
45-54	.506 / .585	.847 / .816	.693 / .723	.690 / .728	.568 / .762	.896 / .885	.745 / .689	.629 / .493	.651 / .539	.617 / .669				
55-64	.715 / .677	.634 / .648	.540 / .484	.847 / .797	.698 / .706	.695 / .856	.006 / .699	.780 / .469	.544 / .494	.667 / .656				

and Commerce[7] provides great scope for testing this implication, thanks to the many cross-tabulations presented. The particular classification chosen was by city class, occupation, and age of the head of the household. The groups contained under each of those headings can be seen in Table I. In order to avoid meaningless results based on very small numbers of households, all those where the head was over 65 or under 25 were excluded, and so were all remaining subcells in which fewer than 5 income groups were represented.[8] This left 215 subcells containing 9923 households. Moreover these subcells were combined in various ways, for instance by lumping all cells in the same occupation.

For each of the 363 cells so created two simple regressions of consumption on income were computed. One was linear (constant marginal propensity to consume) and the other double-logarithmic (constant elasticity of consumption with respect to income). Friedman uses both shapes of the consumption function, though on the whole (and I agree) he prefers the latter.[9]

Reproduction of the full results of those analyses would occupy too much space. The marginal propensities and the elasticities for the larger cells (those where the households were classified by two characteristics, by one characteristic, or not at all) can be found in Table I. This table contains three cross-classifications, one by occupation and city class, one by occupation and age of the head, and one by age of the head and city class. The entries in the row and column marked "All" are common to pairs of cross-classifications, which made it efficient to combine everything in one table.

A quick glance at Table I shows that Friedman's assertion, according to which the regression coefficients for more narrowly defined subgroups should be smaller than for broader subgroups, is by no means borne out. Looking, for instance, down the "All" column for various occupations, we see that only one occupation (the self-employed) has an MPC smaller than the .658 found for the sample as a whole. A similar check for the elasticities shows that only two occupations have a smaller elasticity than the sample as a whole has. Similarly, of the 36 MPC's and elasticities in the cross-classification by age and city class, only 15 are smaller than the corresponding MPC's and elasticities in their city-class group (that is to say, smaller than the item in the "All" row at the top of this cross-classification).

A more systematic comparison of this type is summarized in Table II. The "minor" cells are those which are defined by more characteristics than the "major" cells. Separate comparisons are made for linear and double-logarithmic regressions. Cases are called "favorable" if they are in accordance with Friedman's assertion, that is if the estimate for the minor subgroup is smaller than for the major subgroup; otherwise they are called "unfavorable." The comparisons made in the previous paragraph for the cross-classification by age

[7] *Study of Consumer Expenditures, Incomes and Savings,* Vol. II, Philadelphia: University of Pennsylvania, 1956.

[8] By "subcell" is meant a group of households belonging to the same city-class group, occupation group and age group.

[9] Within each cell the averages for each income range were weighted by the number of households in that range. The informed reader will realize that access to the IBM 704 was a distinct advantage.

TABLE II.—COMPARISON OF REGRESSION COEFFICIENTS
IN CELLS OF DIFFERENT CONTENT

Defining Characteristics		No. of Cells		No. of Cases			
				Linear		Double-log	
Minor Cells	Major Cells	Minor	Major	Favor-able	Unfavor-able	Favor-able	Unfavor-able
1. Age of head	All households	4	1	1	3	2	2
2. Occupation	All households	7	1	1	6	2	5
3. City class	All households	9	1	4	5	5	4
4. Age & occupa-tion	Age	28	4	8	21	12	16
5. Age & occupa-tion	Occupation	28	7	16	12	16	12
6. Age & city class	Age	36	4	15	21	17	19
7. Age & city class	City class	36	9	16	20	16	20
8. Occupation & city class	Occupation	63	7	29	34	28	35
9. Occupation & city class	City class	63	9	24	39	28	35
10. Age, occupation & city class	Age and occu-pation	215	28	102	113	94	121
11. Age, occupation & city class	Age & city class	215	36	82	133	96	119
12. Age, occupation & city class	Occupation & city class	215ᵃ	63	101	110	98	113
13. Age, occupation & city class	All households	215	1	67	148	86	129

ᵃ Four major cells contained only one minor cell each, so that no comparison was possible.

and city class, for instance, can be found in line 7 under "linear" and "double-log."

Table II shows that for almost every comparison the number of unfavorable cases exceeds that of the favorable cases. Even though most of the comparisons are not statistically significant taken separately, the combined impression is very damaging to the permanent-income hypothesis. Perhaps the most devastating evidence is in line 13: (of the marginal propensities to consume in the 215 smallest subcells less than one-third agree with Friedman's theory, and only 40 per cent of the corresponding elasticities agree. Even if the coefficients for the minor cells had a fifty-fifty chance of being smaller than the coefficients for all households, the observed result would have a probability of less than one per cent. A fortiori we are compelled to reject the hypothesis that the coefficients in the minor subcells are on the whole smaller than those for all households.)

IV. *Evaluation*

The negative conclusion, which applies equally well to the linear and to the double-logarithmic form of the consumption function, clearly reflects adversely on the permanent-income hypothesis of which it is the most direct

test available so far.[10] The fact that Friedman himself had confidently suggested it only adds to its significance.[11] Nor car there be any doubt that if the concept of permanent income has any meaning at all, its numerical value must be more uniform in the minor than in the major subcells, so that the conditions for the test are indeed satisfied.

It is more difficult to say, however, what the negative result of the test actually implies. One obvious though tentative interpretation is that the MPC out of transitory income, far from being zero as Friedman maintains, is actually greater than the MPC out of permanent income. Thus, to come back to our previous example, the lucky winner does not run to the savings bank but to the tavern, and the victim of theft does cut his coat according to his cloth. It is perhaps relevant here that the definition of consumption used in the test is not the one that Friedman prefers. He would like to exclude the purchase price of durables from current consumption and include their use value. I have much sympathy for this idea, which was first put into practice by Goldsmith in his savings study referred to above. Unfortunately durable purchases are not stated separately in the tabulations of the BLS Survey of 1950, nor in many of the sources which Friedman uses in his book. In any case, I doubt whether this conceptual difference can account for the negative result of the test.

The main thing that needs to be done, it would seem, is to make explicit allowance for the factor that is so conspicuously absent in Friedman's theory, namely liquid assets. Here again, the BLS-Wharton data are deficient. If it could in fact be shown that liquid assets do not influence current consumption, the permanent income hypothesis may yet become a useful tool of economic analysis. As of now it must be regarded as a novel idea whose interest lies neither in its theoretical plausibility nor in its empirical validity, in both of which respects it is unsatisfactory, but in the further research which it is likely to stimulate.

[10] I understand that Harold Watts of Yale University has independently performed a similar test using data from the Survey of Consumer Finances and has reached similar conclusions.

[11] I should point out, however, that I have diverged from Friedman's suggestion in one minor respect. Friedman's conclusion is that a weighted average of the coefficients in the minor cells should be less than the coefficients for the major cells. He does not say, however, what weights should be used. Any plausible set of weights would no doubt lead to the same negative conclusion as was reached above. Thus if total income is used as a weight, the mean of the MPCs in the 215 smallest sub-cells is .734, as against .658 for all households combined.

COMMUNICATIONS

The Mutual Influence of Mitchell and Commons

Professor Boulding in his address, "A New Look at Institutionalism," at the 1956 annual meeting of the American Economic Association, observed: "Of the three [best representatives of the school], Commons is the isolate— and to my mind probably the most important and influential of the three in the long run. It is curious that neither Veblen nor Mitchell seem to have known Commons, though it is hard to believe that they did not have at least a casual contact."[1]

It is true that Veblen and Commons seem to have had little or no personal contact. But the case of Mitchell and Commons is far different. They not only knew each other but corresponded and functioned together professionally, in a variety of significant ways, for some twenty-five years. The evidence is both published and unpublished. The published material includes the following: Mitchell and Commons were in a small group that met in 1917 to promote what became the National Bureau of Economic Research.[2] On its formal organization in 1920, Mitchell became director of research and Commons a member of the board of directors. While Commons was president of the Stable Money League and its successor organization, The National Monetary Association, Mitchell was a member of its Research Council.[3] On becoming president of the American Economic Association in 1924, Mitchell appointed Commons to membership on the Nominating Committee. Finally, in the preface of *Legal Foundations of Capitalism* (1924), Commons lists Mitchell's name as first of those from whom he had "important assistance and criticism."

Then there exists an extensive correspondence between the two men which is in the Mitchell Papers, available in the division of Special Collections of the Columbia University Library. It is by no means complete; if Commons had not destroyed his own correspondence files, it is probable that more letters would be available. But the letters we do have cover the last two decades of Commons' life, a period in which Commons published his major contributions to economic theory.

The Commons-Mitchell correspondence includes discussions and criticisms of theory as well as matters of public policy, preparation of manuscripts and publication strategy, and other professional matters. Mitchell read, criticized, and made suggestions on some of Commons' most important theoretical works, and Commons acknowledged not only the assistance, but the intellectual stimulation of his colleague. They met to discuss research projects and articles.

Here are some interesting passages from the correspondence: It begins with a letter from Commons on December 20, 1921:

[1] *Am. Econ. Rev. Proc.*, May 1957, XLVII, 6.

[2] Herbert Heaton, *A Scholar in Action: Edwin F. Gay*, Cambridge 1952, p. 94.

[3] Irving Fisher, *Stable Money*, New York 1934, pp. 204-5.

Dear Mr. Mitchell,

Received copy of the [National Bureau] volume on *Income* [*in the United States*]. I am greatly pleased with it. Shall see you at [the American Economic Association meetings at] Pittsburgh. I hope you can let me have a copy of your paper to be read at that time, as I have a seminar this year on Business Cycles and Unemployment.[4] Also I want to talk over with you about sources of materials. ...

After the meetings, Commons wrote on January 4, 1922:

Dear Mr. Mitchell,

Referring to our conversation on theory of value in Pittsburgh, I am sending herewith an article on Correlation of Law and Economics which I have in course of preparation for a law magazine. I am also sending the first chapter of my latest revision of valuation theories ... I hope to talk matters over again when I see you in February in New York.[5] ...

On October 30, 1922, Commons sent to Mitchell the completed manuscript entitled, *Reasonable Value:*

My dear Mitchell,

I am taking the liberty of sending you by express the manuscript of my latest revision of my chapters on value theories based on the court decisions. ... I shall greatly appreciate whatever comments and criticisms of the chapters you may have. ...

Mitchell replied on March 29, 1923:

Dear Commons:

... Your work will have a profound influence on economic theory and perhaps on jurisprudence, but ... the development of this influence is likely to be rather slow. ... The book is too original to have a rapid success. It is also too large a volume to be read by many people. Have you considered the possibility of publishing chapters 1-9 first, giving people a little time to digest that volume, and then coming out with chapters 10-12, perhaps under an independent title? ...

Commons answered (April 9, 1923):

Dear Mitchell:

... I am particularly thankful for your suggestion of issuing it [*Reasonable Value*] as two volumes, making a division as you suggest. I am starting in at once to get the first volume ready and hope to have it completed by next fall. That will give me two or three years to work on the review of the different cycles of economic theory. ...[6]

The next letter (August 2, 1923), reveals that Commons was so impressed

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[4] The Mitchell paper is "The Crisis of 1920 and the Problem of Controlling Business Cycles," *Am. Econ. Rev.*, suppl. March 1922, XII, 20-32.

[5] The reference to New York is apparently to the annual meeting of the board of directors of the National Bureau of Economic Research.

[6] The first part became *Legal Foundations of Capitalism*, New York 1924, and the remainder, *Institutional Economics*, New York 1934.

with Mitchell that he sought, though unsuccessfully, to have Mitchell prepare a brief on "Pittsburgh Plus":

Dear Mitchell:
I am writing you in order to find out whether you could . . . serve the four states of Illinois, Iowa, Minnesota and Wisconsin in presenting the economic facts and arguments before the Federal Trade Commission in the so-called "Pittsburgh Plus" case. . . . These states want to have an outside economist, not a resident of these four states, and have delegated me and my colleagues [from the other state universities to make the choice] and we have agreed on ycu. . . .

In sending Mitchell chapters of his new theoretical treatise, *Investigational Economics,* Commons wrote on March 30, 1937:

Dear Wesley:
Heartily appreciate your book [*The Backward Art of Spending Money and Other Essays*]. . . . It is just what I was hoping to get—your Presitial address on *Quantitative* etc., bearing on comparison I am making of Lionel Robbins and my *Institutional,* analysis. . . . I am rapping him on his criticism of you & wanted to know just how you would have disposed of him. . . .

Mitchell replied on May 7, 1937:

Dear John:
. . . The manuscripts you sent me . . . succeed admirably in showing the importance of the issues that your analysis raises. . . . I am winding up my year's course on Types of Economic Theory by discussing with the class your Institutional Economics. I think a large proportion of them at least feel more drawn toward your kind of work than toward that of any of the other masters whom they have studied. . . .

In answer, Commons wrote, on May 29, 1937:

Dear Wesley:
Enclosed is concluding chapters of my *Investigational Economics* for beginners, along with *Contents* showing the setting. I wonder if my references to you are the kind of reply you would make to Robbins. At my age I keep thinking I may be daffy without knowing it, and reiterating the obvious, or hitting a straw man. Your comments on preceding chapters reassure me. Please comment on this at your convenience. . . .

Mitchell's strong support of Commons' theoretical work both in reviews and correspondence came at a time when dominant opinion as expressed in the great majority of reviews, showed, to put it mildly, little appreciation of its importance.[7]

This correspondence, and the evidence of the published works of these

[7] It should be noted that the economists' skepticism of Commons as a theorist did not diminish their admiration of him as the leading student of American labor. This latter reputation was doubtless responsible for his election to the presidency of the American Economic Association in 1917.

men, suggests that there were many points cf agreement and similarity between Commons and Mitchell and that these transcend their differences. But a detailed discussion of this larger point would take us·beyond the limits of a note.

<div align="right">JOSEPH DORFMAN*</div>

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The Union and Wages in Basic Steel: A Comment

Editor's Note: Footnotes 2a, 3a, 7a, 8a and 20a were added by the author after he had read Rees' *Reply;* but Ulman wishes it to be clear that agreement with other aspects of the *Reply* is not necessarily implied.

The purpose of this paper is to challenge the interesting conclusion by Albert Rees[1] that the steelworkers' union did not raise wages appreciably above the levels which would have been reached in its absence during the war-and-postwar inflationary period. This conclusion is based primarily on inferences drawn from: (1) a comparison between wage changes in basic steel and wage changes in certain less well-organized occupations; (2) a comparison between wage changes in basic steel in the two world-war inflationary periods; and (3) reports of shortages in both the product and the labor markets of basic steel in the inflationary period following the second world war.

I will consider each of these alleged foundations in turn and will point out (a) that in each case the criterion is not a satisfactory one for determining whether or not the steelworkers' union made an independent contribution to the observed increases in the earnings of its members in basic steel, and (b) that in certain instances the empirical evidence presented by Rees, when supplemented by additional relevant evidence, makes the contrary conclusion at least as plausible as the conclusion that it did not do so. In the latter connection, it should be noted that Rees at one point merely claimed that "it is equally easy to argue that collective bargaining in the basic steel industry during this period kept wages below the level which they would otherwise have reached, or to argue that it raised wages above such a level." However, it is elsewhere made abundantly clear that he regarded the first inference as more equal than the second. This paper might therefore be regarded as an effort to redress this disparity.

I. *The Interindustry Wage Comparisons*

From a comparison between percentage increases in average hourly earnings in basic steel and in selected industries characterized by much less complete unionization than prevailed in steel, Rees concludes that, for the periods 1939-1948 and 1945-1948, "the efforts cf the United Steelworkers were not enough to offset the forces working toward relatively larger increases

[1] Albert Rees, "Postwar Wage Determination in the Basic Steel Industry," *Am. Econ. Rev.,* June 1951, XLI, 389-404.

for other groups" (p. 401). This much is true for the majority of the industries in question, but of course it does not imply that, in the absence of a well-organized union, wages in basic steel would have risen as much as they actually did. The latter inference is implied in the conclusion quoted above only if one could have confidence that, for the purpose at hand, the only significant difference between basic steel and the comparison industries consists in their respective degrees of unionization. But no attempt was made to control for other differences between basic steel and the other industries to which it was compared—such as differential changes in employment and productivity and differences in market structure or in original level of earnings. Other writers believed such differences to be significantly associated with variations in relative wages, and some of them attempted to make allowance for them in assessing the impact of unionism per se.[2]

For example, apart from the difference in the degree of unionization, basic steel differed from most of Rees' comparison industries with respect to certain characteristics which tended to hold wages in basic steel down relative to wages in the comparison group in the early wartime subperiod, and with respect to other characteristics which tended to exert the same effect in the subperiod between the suspension of controls and the onset of recession in late 1948.

In the first place, the original level of earnings in basic steel greatly exceeded that in all other categories except crude petroleum. (See Table II below.) Since, in wartime the labor markets in essential industries were characterized by widespread substitution of less skilled and lower-paid labor for more highly skilled and higher-paid workers, one would expect greater percentage, as well as absolute, increases in earnings in industries which originally employed relatively high proportions of low-paid workers and which sought subsequently to retain them.[2a] This point is related to another element of difference between basic steel and most of the highly organized industries with which it was compared. Over the decade 1940-1950 (which includes the period under discussion), women comprised a high and in some cases an increasing proportion of the work force in certain occupational groupings which are relevant to 7 of Rees' 12 comparison industries: textile-mill products, in

[2] See especially J. T. Dunlop, "Productivity and the Wage Structure," in *Income, Employment, and Public Policy,* New York 1948, pp. 341-62; A. M. Ross, *Trade Union Wage Policy,* Berkeley 1948, pp. 113-33; A. M. Ross and William Goldner, "Forces Affecting the Interindustry Wage Structure"; and J. W. Garbarino, "A Theory of Interindustry Wage Structure Variation," both in *Quart. Jour. Econ.,* May 1950, LXIV, 254-305.

[2a] Another methodological issue is related to the failure of Rees' paper to control for differences in original levels of earnings. Levinson's study does compare groups of highly organized industries with groups of lightly organized industries in the same classes of original earnings; and the author finds that the former did not secure greater wage increases than the latter during the period 1941-47. But a comparison among large groups at once raises the possibility that wage movements in the nonunion sector might be affected by wage movements in the unionized sector. Levinson calls attention to the fact that part of the wage increases in the union sector might have been due to "union pressure" and that "this greater increase might then have been transmitted to the nonunion workers through sympathetic pressure." H. M. Levinson, *Unionism, Wage Trends, and Income Distribution, 1914-1947,* Michigan 1951, pp. 67 and 74.

which 53.3 per cent of all employees were women in 1950 as against 49.9 per cent in 1940; confectionery and related products—65.4 and 67.1 per cent, respectively; laundry and dry-cleaning—67.1 and 66.8 per cent; and wholesale and retail trade—37.9 and 34.1 per cent. In all of these categories, the number of women employed increased despite the fact that, during the wartime period, employment of women in higher-paid defense industries expanded greatly. At the same time, the proportion of women workers to the entire female population 14 years of age and over increased from 26 per cent in 1940 to a peak of 37 per cent in 1945 and then declined to a level of 29 per cent in 1950.[3] But whether or not increased wages were required to attract these additional female workers to the labor force, it is reasonable to presume that these lower-paid trades were under especially strong market pressure to raise wages in order to retain and increase their own female work forces.[3a]

A further element of difference between basic steel and most of the comparison industries consists in the fact that steel was far more "essential" to the wartime economy; and it is a matter of common knowledge that wartime controls on wages were much more effectively imposed on such essential industries than they were on more purely "civilian" trades—such as confectionery, laundries, cleaning and dyeing, retail trade, ice cream, etc.

Moreover, in the subperiod following the removal of wage controls, basic steel's pricing policy tended to retard the increase in its demand for labor. Rees pointed this out and attributed this phenomenon in part to the existence of widespread collective bargaining in basic steel. In Part III below, however, we question this explanation and suggest that the existence of another set of characteristics, its oligopolistic structure and political sensitivity, sufficed to account for this industry's price behavior—in which respects basic steel differed notably from all of the other comparison industries with the exception of crude petroleum.

In addition, Rees' method carries the implication that in none of the industries which were lightly organized did unionism exert an impact upon the level of money wages. This method makes no allowance for the possibility that nonunion firms might have raised wages in order to prevent themselves from becoming organized.[4]

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[3] U. S. Dept. of Labor, Women's Bureau, *Changes in Women's Occupations, 1940-1950*, Bull. 253, Washington 1954, pp. 90-97; and *1956 Handbook on Women Workers*, Bull. 261, Washington 1957, p. 2.

[3a] Nor can the movements in wages of male wage earners be regarded as independent of the special influences which affected the wages of their female co-workers. If employers were under especially strong market pressure to raise the wages of women in those industries which had heavy concentrations of female labor, considerations of tradition, equity, and morale alone, in addition to interchangeability in some occupations, would have induced them to raise the wages of their male employees to a certain extent. (Men and women employees in such industries are frequently in the same family!) Slichter notes some ("not pronounced") tendency for the hourly earnings of male common labor to be low where the percentage of women among wage earners is high. He attributes this in part to the fact that employers in those industries might wish to hire inferior male workers, some of whom could not transfer to "heavier" industries where wages are higher. S. H. Slichter, "Notes on the Structure of Wages," *Rev. Econ. Stat.*, Feb 1950, XXXII, 85.

[4] S. H. Slichter, "Do the Wage Fixing Arrangements in the American Labor Market Have an Inflationary Bias?" *Am. Econ. Rev.*, May 1954, XLIV, 338.

Thus Rees' procedure can support a conclusion no broader than the statement quoted in the introduction—but this conclusion is not directly responsive to the question which he raises at the outset: "Has large-scale bargaining during the postwar period raised wages above the levels which would have prevailed in its absence?" It is clearly possible that, even if—contrary to fact —steel wages had risen more rapidly, in percentage terms, than nonsteel wages during the periods studied by Rees, they might have risen even more rapidly in the absence of widespread collective bargaining. And it is clearly possible that, in the absence of widespread collective bargaining, steel wages might have lagged more, in percentage terms, relative to the other wages than they did in fact.

II. *Comparisons Between Two War Periods*

Rees presents data which reveal "that steelworkers made larger percentage gains in money and real hourly earnings during the period 1914 to 1920 than during the period 1939 to 1948. In the earlier period steelworkers' money earnings increased relative to earnings in all manufacturing, while in the more recent period they decreased" (p. 400).[5] However, during the postwar period 1945-1948 average hourly earnings in steel rose as rapidly as average hourly earnings in all manufacturing, and money hourly earnings in both just about kept pace with the cost of living. (The annual increases were 33, 32, and 34 per cent respectively.)[6]

Now it is evident that interpretation of all these findings must be subject to the same class of restrictions which apply in the case of the interindustry comparisons discussed in the preceding section, since no control was explicitly established for all relevant differences between the two periods apart from the difference in the degree of unionization in steel. Comparing basic steel wages relative to wages in all manufacturing in the two periods might be construed as an attempt to control for other differences; however, it also eliminates the differences with respect to unionization. For, unionization in manufacturing increased along with unionization in steel during the second period; by 1946, 69 per cent of the production workers in manufacturing were covered by collective bargaining.[7] Manufacturing wages rose less rapidly in 1939-1948 than in 1914-1920 (see footnote 14), but this does not warrant the conclusion that unionism in all manufacturing had no effect upon the movement of hourly earnings in manufacturing and upon the cost of living during the second period. Hence, the fact that steel wages fell relative to the former and failed to rise relative to the latter by as much as in 1914-1920

[5] The statement concerning the movements in money earnings in basic steel relative to those in manufacturing is supported by computations from data of the National Industrial Conference Board for 1914-1920 and from data of the Bureau of Labor Statistics for 1939-1948. Computations from BLS data for the period 1914-1920 indicate that steel wages rose no more rapidly than did manufacturing wages in the early period. See footnote 14 below.

[6] Average money hourly earnings in basic steel rose from $1.188 in 1945 to $1.580 in 1948; in all manufacturing they rose from $1.023 to $1.350. See BLS series published in *Mo. Lab. Rev.*, Table C-1. The cost-of-living movement is calculated on both the "old" BLS Index of Consumers' Prices and the revised index.

[7] *Mo. Lab. Rev.*, May 1947, LXX, 765.

might merely indicate that the United Steelworkers were unable to keep up with the Joneses over the period 1939-1948 and specifically in the subperiod 1939-1945.

In any event Rees claims only that his evidence "does show . . . that the efforts of the union were not sufficient to offset apparent differences in other factors, probably including the smaller increase in steel prices . . ." (p. 400), and, as we have seen, unionization in manufacturing in the second period. But we might ask whether one might find other evidence to support the more relevant hypothesis that, but for the existence of widespread collective bargaining in basic steel, the percentage increase in hourly earnings which did occur over the period 1939-1948 would have been smaller than it was— despite the fact that it was much smaller than the increase in 1914-1920.

The latter hypothesis, I believe, acquires greater plausibility than does Rees' view to the contrary when the following additional evidence is arrayed against the wartime wage comparisons: (1) a comparison of changes in employment in basic steel in the two wartime periods, taken in conjunction with (2) the changed nature of the jobs and the work force in this industry.

1. *The Two Wartime Increases in Employment.* According to Table I employment in basic steel increased by much more in 1914-20 than it did in 1939-48, not only in percentage terms but absolutely as well, and relative to total civilian employment and labor force. Thus while steel wages rose more rapidly in 1914-20 than in 1939-48, so did employment in basic steel. Moreover, while the per cent increase in average hourly earnings in 1939-48 was about .6 as great as that in 1914-20, the per cent increase in basic steel employment in 1939-48 was only about .45 as great as that in 1914-20.

Suppose, for illustrative purposes, that one wished to estimate the percentage increase in hourly earnings in basic steel for 1939-48 on the assumption that it bore the same relationship to the percentage increase in employment during that period as the percentage increase in hourly earnings in 1914-20 bore to the percentage increase in employment in 1914-20. Then, given the latter ratio and the observed increase in employment in 1939-48, one would have predicted that hourly earnings would have increased about 71, 66, or 62 per cent in 1939-48, depending upon which estimate of employment for 1920 is used. This is well below the calculated increase of 87 per cent. Moreover, we have not taken into account the fact that, while the increase in steel employment was much smaller in the second period than in the first, the increases in total employment and labor force were much smaller in the first period. Thus, in 1914-20, basic steel claimed between 149 and 172 thousand of the 4,315,000 new jobs created in the economy—roughly 3½-4 per cent; whereas in 1939-48, it claimed only about 100 thousand of the 13,628,000 new jobs created—under 1 per cent.[7a] In addition, as the table

[7a] The example in the text might be altered to make some allowance for differential movements in the labor force as well as in purely inflationary components of the wage increases by comparing increases in real wage rates (which, according to Rees' data, amounted to 27 per cent in 1914-1920 and 13 per cent in 1939-1948) with increases in steel employment relative to civilian labor force. In any event, the example is intended merely to illustrate the possibility that the observed increase in wages during the second period might well have

TABLE I.—HOURLY EARNINGS, EMPLOYMENT, AND WEEKLY HOURS IN BLAST FURNACES, STEEL WORKS, AND ROLLING MILLS, 1914–1920 AND 1939–1948

(1) Year or Change	(2) Average Hourly Earn- ings	(3) Estimates of Employment in Basic Steel			(4) Civilian Labor Force (in thousands)	(5) Civilian Employ- ment (in thousands)	(6) Weekly Hours Basic Steel
		A	B	C			
1920	.745	426,700ᵃ	436,295ᵇ	450,100ᶜ	41,750ᵉ	40,080ᵉ	63.1
1914	.301	278,072	278,072	278,072	38,875ᵉ	35,765ᵉ	64.9
Absolute change	.444	148,628	158,223	172,028	2,875	4,315	−1.8
Per cent change	147.5	53.5	56.9	61.9	7.4	12.1	−2.8
1948	1.580		488,500ᵈ		61,442	59,378	39.5
1939	.845-		383,441		55,230	45,750	35.3
Absolute change	.735		100,059		6,212	13,628	4.2
Per cent change	87.0		25.8		11.2	29.8	11.9

ᵃ The low estimate (A) for 1920 implies, before rounding, an increase in employment of only 2.4 per cent between 1919 and 1920. It is taken from an index of employment compiled by the National Research Project of the Works Progress Administration in *Production, Employment and Productivity in 59 Manufacturing Industries*, Report No. S-1, Pt. 2, Philadelphia, May 1939, pp. 97–99, and forms the basis of most subsequent productivity studies of this industry for the period 1919–29. It also uses month-to-month link relatives, but is based on the year 1929 and extends the BLS index for 1923–36 back to 1919. Presumably the difference between the low and the high estimates for 1920 reflects the fact that the sample of firms reporting year-to-year changes in employment each month for 1919–20 was different from the sample reporting for the year 1920–21.

ᵇ The middle estimate (B) for 1920 was obtained by applying an estimated increase of 156.9 per cent to the Census figure of 1914. This estimate was taken from an "adjusted employment index" constructed by the BLS in 1926 (*Mo. Lab. Rev.*, July 1926, p. 9) which reflects changes in hours of work as well as in the number of employees. However, since the number of hours worked declined during this period, this estimate understates the increase in the employment of workers. Like our high estimate, this estimate of the increase in worker employment for the period 1919–20 takes 1914 as a base and uses the Census figures for 1919 and the monthly reports on changes in employment compiled by the BLS in deriving the estimate of employment for 1920.

ᶜ Estimate (C) for 1920 was obtained by applying to monthly Census data on employment in steel works, blast furnaces, and rolling mills percentage changes in employment reported to the BLS by a sample of employers in "iron and steel" for the 9 months, January through September. The monthly changes in employment for October through December were omitted in this calculation because employment in 1920 was depressed due to the A.F. of L.'s organizing strike and the reported year-to-year increases averaged greatly in excess of the increases reported for the other months. Inclusion of all twelve year-to-year changes would yield an average total of 459.5 thousand production workers in basic steel in 1920.

ᵈ The estimate for 1948 was obtained by multiplying the BLS figure of 536.8 thousand for 1948 by the ratio of the 1947 item in the Census series to the 1947 item in the BLS series. As the following data show, these proportions show some tendency to decline over the period 1947–50; hence applying the 1947 ratio tends to overstate 1948 employment in the census series.

suggests, the existence of the basic 12-hour day in 1914-20 made it difficult for the industry to absorb part of its increased man-hour requirements by lengthening the work week, whereas it was able to do this in the period 1939-48.

Nevertheless, the foregoing could be quite consistent with Rees' hypothesis that unionism was not an independent influence in raising money wages in 1939-48 if it is assumed that it was considerably more difficult for basic steel to attract and retain workers in the second period than in the first. Note, however, that this assumed change in the steel labor-market situation would have had to have been of sufficient magnitude to offset the relatively larger increase in labor supply over the second period. But is this assumption plausible? I believe not.

2. *The Changed Nature of the Job and the Work Force.* Historical evi-

been less than it was in fact were it not for the presence of a strong union. The evidence presented in this section is consistent with the view that, in the second period relative to the first, the industry's increase in demand for labor was smaller and the increase in the supply of labor available to it was greater. In the next section, further evidence (on immigration) is presented in support of the latter proposition; other evidence will be put forth to support the view that industry's labor supply elasticity was greater in the second period.

(Continuation of Table I)

Year	(1) BLS Series	(2) Census Series	Ratio of (2) to (1)
1947	517.6	470.8	.910
1948	536.8	488.5 estimated	
1949	476.7	427.9	.898
1950	535.6	475.9	.889

For a general explanation of differences between the estimates provided by the two agencies see *Handbook of Labor Statistics*, 1950 ed., p. 3. The estimated increase of about 26 per cent in production employment in blast furnaces, steel works, and rolling mills between 1939 and 1948 may be checked against the estimate provided by the American Iron and Steel Institute series on number of wage earners in the iron and steel industry. According to the latter series, iron and steel employment rose from 396 thousand in 1939 to 503 thousand in 1948, an increase of 27 per cent (See *The Economic Almanac, 1953-1954*, New York 1954, p. 383. This series begins in 1935.)

e Estimate from unemployment data presented by Stanley Lebergott, "Annual Estimates of Unemployment in the United States, 1900-1954", in Nat. Bur. Econ. Research, *The Measurement and Behavior of Unemployment*, Princeton 1957, p. 216. National Industrial Conference Board data on total labor force (based on the gainful-worker concept) and total employment yield increases of about 5 and 10 per cent respectively for the period 1914-20.

Sources: Average hourly earnings: 1920 and 1914, U. S. Dept. Commerce *et al.*, *Historical Statistics of the United States*, Ser. D164-171, p. 69; 1948, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1950 ed., Table C-1, p. 69; 1939, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1947 ed., Table C-1, p. 54. Estimates of employment in basic steel: 1914, U. S. Bur. Census, *Fourteenth Census of the United States*, Vol. X, Washington 1923, p. 310; 1939, U.S. Bur. Census, *Statistical Abstract of the United States, 1955*, Table No. 1040, p. 837. Civilian labor force and civilian employment: 1948 and 1939, U. S. Bur. Census, *Statistical Abstract of the United States, 1955*, Table No. 220, p. 187. Weekly hours, basic steel: 1920 and 1914, U. S. Dept. Commerce *et al.*, *Historical Statistics of the United States*, Ser. D164-171, p. 69; 1948, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1950 ed., Table C-1, p. 69; 1939, U. S. Bur. Lab. Stat., *Handbook of Labor Statistics*, 1947 ed., Table C-1, p. 54.

dence suggests that it was more rather than less difficult for basic steel to attract and retain workers in 1914-20 than in 1939-48. Largely because basic steel was the only major industry in the economy which adhered to the 12-hour day for its production employees, it experienced the highest turnover rate of any of the large manufacturing industries in 1909; and it was reported that ". . . the ablest and best-trained men in the steel works were leaving the steel industry when opportunity offered, to avoid the 12-hour day, the frequent overtime, and the great irregularity of work."[8, 8a] And as a result of its unpopular working conditions, basic steel was obliged to recruit a very large and growing proportion of its work force from the nation's supply of inexperienced and docile Slavic immigrants—many of whom, having left their wives abroad, were more willing to sacrifice leisure time for extra income than were native-born workers.[9] However, after the first world war began, the industry's supply of new cheap foreign labor was abruptly shut off—just when the industry had to embark upon a very large expansion of its work force. Steel employers greatly increased their employment of Negroes from the South during the first world war—as they also did during the second—but the large majority of the new workers had to be recruited from the ranks of white adults already in this country.[10] And the latter had already been exhibiting a marked tendency to leave basic steel. That the wartime increases in steel wages which did occur were in part required by the necessity of overcoming the industry's persisting relative disadvantage in hours of work and other conditions of employment is reflected in the following excerpt from an editorial in *The Iron Age* in 1919.

> Labor was more contented ten years, 15 years, 20 years ago than it has been in the past few months. The flow of immigrants to whom steel industry rates and steel industry hours of service were attractive has been shut off completely for five years and seems altogether unlikely to be resumed, yet the steel industry has increased its capacity by 40% dur-

[8] U.S. Senate Doc. No. 110, 62nd Cong., 1st Sess., *Report on Conditions of Employment in the Iron and Steel Industry,* Washington 1911, Vol. III, p. 384; also pp. 15, 20, 21, 162, 165-166, 175, 380; also Vol. I, pp. xiv- xvi.

[8a] Between 1909 and 1914 the work week was reduced, although to what extent the scheduled levels of hours in 1914 reflected the existence of depression in the industry could not be determined, as the 1914 Census of Manufactures noted. The *Reply* calls attention to this reduction and also to the fact that 20 per cent of all workers in basic steel worked 72 hours or over; in all manufacturing—including basic steel—however, only about 3.5 per cent worked that long in the course of a week.

[9] *Ibid.,* I, xvi, xli-xlii.

[10] According to admittedly inaccurate Census data, 5 per cent of the persons attached to "blast furnaces and steel rolling mills" in 1910 were Negro; by 1920, Negro employment had increased to 16 per cent of the total. Applied to the estimates of basic steel employment in Table I for 1914 and 1920 (and arbitrarily assuming 5 per cent Negro employment in 1914), this would roughly indicate an increase of less than 59,000 Negroes in the work force out of a total increment of 148,000-172,000. See U. S. Bur. Census, *Thirteenth Census of the United States,* Washington 1914, Vol. IV, pp. 338-39; and *Fourteenth Census of the United States,* Washington 1923, Vol. IV, pp. 346-47, 15. For the increase in Negro employment in iron and steel as a whole during the second world war period, see Robert C. Weaver, *Negro Labor, A National Problem,* New York 1946, pp. 297-98.

ing that time. Obviously it is necessary to have more attractive conditions than formerly in order to keep the industry continually manned. The demand has been increased, the supply has been diminished.[11]

But almost immediately after U. S. Steel finally inaugurated the basic eight-hour day in 1923, industry sources reported that the labor shortages with which they had been continually plagued had come to an end; and the secretary of the A. F. of L. committee for steel organization wrote that the 8-hour day "has checked the rush for organization that otherwise would have taken place . . . [because] in the steel mills operating on an 8-hour basis employment therein will be found as desirable as anywhere else. . . ."[12] There can be little doubt that he was substantially correct and that the industry entered the second world war better prepared to compete for production labor than it had been at the outset of the first world war.[13] While the industry had eliminated its disadvantage with respect to hours of work, its wage position vis à vis manufacturing industry in general in 1939 was virtually the same as it had been in 1914 and in 1920.[14] More important, whereas the quit rate in basic steel had been extremely high during the earlier period, it was well below the rate in all manufacturing in 1940; and although the quit rate in basic steel rose more rapidly than the quit rate in manufacturing from 1940-48, the former was still below the latter near the end of the second inflationary period. In April 1940, the quit rate in steel was 0.4 per 100 employees and the quit rate in manufacturing was 0.7; in April 1948, the rates were 2.1 and 3.0, respectively.[15]

[11] *The Iron Age,* August 21, 1919, p. 522. An editorial in 1918 (April 11, p. 956) noted that, "The American born . . . do not gravitate to the blast furnace and steel mill. If the mountain will not come to Mahomet, it is perfectly clear what it behooves Mahomet to do. Wages and conditions that have been satisfactory to one class must be made satisfactory to another class if the other class is to be attracted . . ."

[12] *Ibid.,* Oct. 18, 1923, p. 1,030.

[13] Of course, at the beginning of the two wartime periods, there was no immediate stringency in the labor markets, since both 1914 and 1939 were years of high unemployment. Lebergott estimates that 8.0 per cent of the civil an labor force was unemployed in 1914 and 9.7 per cent in 1915; while in 1939, 17.2 per cent were unemployed. See Stanley Lebergott, "Annual Estimates of Unemployment in the United States, 1900-1954," in Nat. Bur. Econ. Research, *The Measurement and Behavior of Unemployment,* Princeton 1957, pp. 1-6.

[14] *Ibid.,* p. 215.

Average Hourly Earnings

(1)	(2)	(3)	(4)
Year	Basic Steel	All Manufacturing	Steel/All Mfg.
1914	.301	.223	1.35
1920	.745	.555	1.34
1939	.845	.633	1.33
1945	1.188	1.023	1.16
1948	1.580	1.350	1.17
1948, September	1.675	1.386	1.21

Source: Historical Statistics of the United States, Ser. D 164-71, p. 69; *Handbook of Labor Statistics,* 1947 ed., Table C-1, p. 54; *Mo. Lab. Rev.; Handbook of Lab. Stat.,* 1950 ed., pp. 58-59, 69.

[15] *Mo. Lab. Rev.,* Table B-2.

We believe that the assumption that it was more difficult for basic steel to retain and recruit the marginal worker in the second period than it had been in the first is difficult to maintain in the face of the historical evidence cited above. Indeed, the contrary assumption appears easier to defend, although it is not necessary to do so. Let one merely assume equal difficulty in both periods and then compare the two periods with respect to changes in steel employment—whether in percentage terms or relative to changes in the labor force—as well as changes in wages. Such a comparison might well lead one to wonder why the 1939-48 wage change was not smaller than in fact it was.

Needless to say, the rough calculations in the preceding subsection cannot be used to show by how much the steel union was responsible for the increase in money wages in 1939-48. For one thing, if we accept the hypothesis that unionism was an independent influence on wages, we must also reckon with the possibility that the increase in basic steel employment would have been greater in the absence of the union than it actually was during that period (and that this would have reduced the difference between the predicted and the observed wage increases).

III. *Shortages and Expectations*

Rees would presumably agree that in the absence of the union the increase in steel employment would have been greater than it actually was before the inflationary peak had been passed in September 1948, but for a quite different reason; he believes that the union helped to choke back the demand for steel labor and thus to prevent both employment and wages from rising as rapidly as they would have in the absence of the union. And he refers to reports of local labor shortages during the postwar period as evidence that wages would have risen more than they did had it not been for collective bargaining. Let us now test the third leg of this tripod.

Two alleged causes of local labor shortage are considered. On the one hand, "wage increases were undoubtedly delayed by fixed-term collective agreements" (p. 402). However, such fixed-term agreements need not have resulted in shortages if the periodic negotiations had resulted in wage rates sufficiently high to compensate in advance, as it were, for subsequent increases in demand. Since it is noted in another connection that "shortages existed soon after wage increases were negotiated, indicating that negotiated rates at these times were probably below those which would have prevailed in the absence of collective bargaining" (p. 393), one might infer that the existence of fixed-term agreements is not regarded as the most important agent of retardation.

The more important cause of local labor shortage is alleged to have been the union's contribution to the industry's decision to underprice its product, thereby permitting product shortages to develop and also making it unprofitable to raise wages to levels which would have obtained in the absence of price restraint. In part, the union's publicity campaign against price increases helped to hold down steel prices, according to this argument, but more important was the alleged fact that employers feared to raise prices and wages sufficiently to overcome the "temporary" shortages in steel and steel labor

because they believed that "the union could successfully maintain the higher level of wages after the demand for steel had declined." Hence in the absence of the union, it is alleged, wages would have risen to higher levels during this period.

Can we presume that, in the absence of the union, the existence of local labor shortages would have sufficed to induce employers in basic steel to raise wages above the levels actually reached under collective bargaining? Were any alternatives to raising the basic hourly rate available to basic steel employers; and might they have availed themselves of these alternatives either in the presence or in the absence of collective bargaining?

The Assumption of Perfect Competition. If we assume perfect competition in the labor markets for basic steel, we can presume that "a shortage would cause the bidding up of wages in the absence of collective bargaining. . . ." However, the assumption of the existence of conditions which would ensure perfect competition in the labor markets for production workers in basic steel in the absence of unionism is unwarranted. One writer noted that "a large company . . . adopt[s] and follow[s] a uniform policy for all its plants," and that at least the major companies tend to engage in "uniform behavior on labor conditions."[16] In addition to the policies of the union and the relatively large size of U. S. Steel and its consequent ability to assume its historic role of wage leader, he attributes these phenomena to the fact that steel mills typically employ a relatively large proportion of the labor of the types they require in the local labor markets involved. Where there is only one steel company in a town, "they are either the one company in a one-company town or one of a handful of large employers in a somewhat bigger community." Most of the industry's tonnage is produced in the large steel centers of the country; however: "When two steel plants are part of the same labor market, a wage increase by one certainly affects the other " And while steel employers compete with other industries for skilled maintenance workers and for common labor, the majority of the skilled and semiskilled jobs are unique to the industry[17] and are usually staffed by promotion.

This writer is describing monopsonistic or oligopsonistic labor markets. Now if a labor shortage exists in such a market it cannot be presumed that removal of an institution (in this case, the trade union) responsible for holding the wage rate at the level with which the shortage is associated would be followed by a "bidding up of wages." The new wage (and the new level of employment) would be determined by the relative positions of the demand curve for labor and the marginal-cost-of-labor and labor-supply functions; and the new wage might well fall below the union wage, even if at the latter wage there existed excess demand due presumably to an upward shift in the demand curve. Moreover, even immediately following a subsequent rise in the negotiated wage, the higher union wage could still be associated with some excess demand; this, however, does not appear to have been the case. Thus if "shortages existed soon after wage increases were negotiated," this

[16] Robert Tilove, *Collective Bargaining in the Steel Industry*, Philadelphia 1948, p. 31.

[17] *Ibid.*, pp. 31-32.

should not be taken as necessarily "indicating that negotiated rates at these times were probably below those which would have prevailed in the absence of bargaining." They might have been, but we cannot assess the probability involved solely from the evidence (a) that increases in employment occurred, (b) that price increases failed to eliminate product rationing, and (c) that intermittent local labor shortages were reported, primarily in the Chicago and Youngstown areas (although to what extent such shortages occurred among production workers in basic steel specifically is not known). In this context, the reporting of "labor shortage" might have signified only that employers wished to hire at the union rate more labor than was available at that rate. It did not necessarily signify that multiplant companies would have been willing to raise the wages of the existing forces in all their mills in an attempt to employ additional workers in some mills in those localities where labor was tight at the time, or even that they would have been willing to raise the local basic rate alone.

Some Dynamic Considerations. Even if the wage level at which the local market could be cleared is assumed to have been above the union wage, it need not follow that, in the absence of collective bargaining, a labor shortage would have resulted in a wage increase. When the dominant employing units expect that the short-run supply curve of labor in the local labor market will shift to the right or that the demand for labor will be reduced in the near future, it is unlikely that what is regarded as a temporary shortage would touch off a wage increase. The period July 1946 to at least August 1947 was cited by Rees as having been characterized by serious local labor shortages in the Chicago-Gary and Youngstown areas. Now some of these reported labor shortages were due to shortages in housing facilities near the plants; in one instance, it was specifically reported that, "The number of workers recruited [from the Pittsburgh region] was limited, apparently, only by the availability of housing at the plant site."[18] And, during the same postwar period, some reported shortages were attributed to the availability of unemployment compensation benefits to war-weary civilian workers and returning servicemen; while the Chicago steel mills were trying to fill about 3,300 jobs (mostly unskilled), 135,000 individuals in the area were reported as unemployed—and over 50,000 returning servicemen were expected.[19]

If the steel employers had had no union to contend with in 1946-47, would they really have responded to such shortages by raising wages sufficiently to have eliminated them? If not, then the reported existence of shortages under collective bargaining is not sufficient to create the presumption that collective bargaining held wages down. Nor can it be held to imply that unionized employers were not paying wages higher than they would have been willing to pay in the absence of the union but with all other conditions remaining the same. If we admit that the employer, under nonunion conditions, might tolerate some excess demand which he regards as temporary, then we cannot tell whether the shortage actually observed under collective bargaining is

[18] *The Iron Age,* Nov. 21, 1946, p. 136.

[19] *Ibid.,* Aug. 12, 1946, p. 44; Aug. 29, 1946, p. 124.

larger or smaller than the shortage which he would find tolerable under non-union conditions.

Relevant to the latter consideration is Hicks' suggestion that employers who have reason to believe that the supply curve of labor to their industry is considerably more elastic in the long run than in the short would hesitate to raise the wage rate sufficiently to cover a momentary shortage and later to reduce it. They would be restrained by their fear of "cheating the expectations" of entrants to the trade.[20] Employers in the U. S. steel industry might well have been similarly motivated in the period between August 1918 and August 1923 (when the 8-hour day was introduced). With the exception of the depression of 1920-21, this was a period of chronic labor shortage in the industry, unrelieved by the five wage increases which were either too little or too late to remove the specific shortages which had provoked them.[20a] Two months after the ineffective wage increase of April 1923, *The Iron Age* approvingly quoted an admonition from W. I. King to "Raise wages as little as possible, thus avoiding the necessity of severe wage cuts at a later date"; and it added, "Advancing wages that must afterward come down causes trouble and it is human nature that it should be so. We are dealing with men as they are, not as they would be if the Golden Rule were their sole guide to thought and conduct."[21]

Both Hicks and King presumably assumed that the employers whom they had in mind were able to act on their long view; however, the employers' ability to do so implies in turn a departure from perfect competition on their side of the labor market. This condition, as we have argued, is satisfied in the case of basic steel. But now consider the assumptions underlying the argument that employers in steel, if there were no union, would set a wage high enough to eliminate what they regard as a temporary shortage and without regard to a foreseeable future excess supply with which that wage might be associated. This argument assumes either that such employers act irrationally in terms of their long-run situation or that the degree of competition among them is so high that they (literally) cannot help themselves. Hence, as in

[20] See J. R. Hicks, "Economic Foundations of Wage Policy," *Econ. Jour.*, Sept. 1955, LXV, 404.

[20a] In 1920 local labor shortages were reported and persisted from April to September; these followed a wage increase in February and were terminated before the next rise in wages, which did not occur until May 1921. In 1922, shortages which were reported from the beginning of the year were not responded to by a wage increase until September, and this increase did not have the effect of stopping complaints of labor shortage. Furthermore, a wage increase of 11 per cent in April 1923 failed to alleviate a severe and persistent shortage of labor; as a result of this U. S. Steel inaugurated the basic 8-hour day in August with a compensating wage rise. The latter move did put an end to the persistent labor shortage, incidentally, which clearly reveals the prior effect of the industry's policies with respect to hours of work upon its ability to attract and retain native-born labor.

[21] *The Iron Age:* Apr. 15, 1920; Sept. 2, 1920; Jan. 26, 1922; May 4, 1922; June 29, 1922; Oct. 12, 1922; Nov. 9, 1922; Dec. 28, 1922; May 31, 1923; June 21, 1923. U. S. Steel also reported labor shortages during the second half of 1919; the subsequent increase occurred on Feb. 1, 1920.

the previous section, we are led to note that the view that labor shortage-cum-bargaining implies a restraining effect of unionism upon wage increases leans heavily on the assumption that perfect competition would prevail in the labor market in question in the absence of unionism.

Overtime—An Available Remedy. Presumably neither Rees nor we would deny that employers who believe it to be in their short-term interest to raise employee compensation to a given level would hesitate to do so if they could subsequently reduce compensation without fear of employee resistance. We might further agree that workers would tend to oppose an attempt to reduce their basic rate of pay and that knowledge of this would make employers unwilling to raise that basic rate in the first place. But there is more than one way to skin this cat, and it is available to union as well as to nonunion employers. By paying overtime premiums during transitional periods of worker shortage, employers could expand their employment of man-hours (a) without having to pay a higher rate for the straight-time hours throughout the enterprise, (b) without contravening either the letter or spirit of their fixed-term agreements, and (c) without courting the alleged peril of being prevented by the union from reducing average wages in response to a subsequent decrease in demand for labor.

Rees cites the specific instance of the Youngstown Sheet and Tube Company whose vice-president in charge of industrial relations testified before a Senate committee that, in August 1947, his company was short 1,000 workers of its normal complement of 24,C00 in the Youngstown and Chicago-Gary areas. But this official added, "We do make up that thousand men by employing men overtime, but at the same time the efficiency is not there."[22] Whether or not this company would have been any more willing to increase its unit labor costs by raising its straight-time wage rate and its outlays on employee selection and training sufficiently to increase its work force by 4 per cent than it was to increase them by paying an overtime premium and incurring loss of efficiency is not known. However, we do know that throughout the period 1946-1948 basic steel was not, in the aggregate, on an overtime basis. In the ten strike-free months of 1946, the average work week was 37.4 hours; in 1947, 39 hours; and in 1948, 39.5 hours. (In all manufacturing, the length of the work week was 40.4 in 1946 and 1947 and 40.1 in 1948.) During the wartime period, 1942-1945, however, the work week in basic steel exceeded 40 hours, as it did—although to a much lesser extent—in 1951 (40.9), 1953, 1955, and 1956 (all 40.5).[23] Apparently the industry was unwilling to pay premium wages for overtime—according to one report of a labor shortage at the end of 1945, "mostly because it feels that only on a straight-time basis can it meet its present costs. This has meant the elimination of overtime." Between 1945 and 1946, when the work week declined by 9.1 hours, total

[22] U. S. Congress, Senate, *Steel Supply and Distribution Problems Affecting Smaller Manufacturers and Users,* Hearings before the Special Committee to Study Problems of American Small Business, 80th Cong., 1st Sess. (1947), p. 2019.

[23] The length of the average work week in 1942 was 40.2 hours; in 1943, 44.3; in 1944, 46.3; and in 1945, 44.1 The foregoing are BLS data.

man-hours worked in the entire steel industry declined, while the number of hourly rated employees rose by over 4 per cent.[24]

Thus an alternative to raising the base rate existed, but it was not significantly used, apparently because it cost too much. How, then, do we interpret those reports of local labor shortage? It is possible that they signified merely that, while employers would have been willing to hire more labor than was available to them at the going rate, they would not have been willing to raise the average rate sufficiently to pay overtime. Indeed, in this context, it is even possible that reports of local shortages might have signified in some cases that the employers involved wanted to hire more labor, but only at a lower wage rate. For that is what would be implied if their unwillingness to pay overtime meant that they would also have been unwilling to raise their total hourly payroll costs by the amount necessary to defray the additional costs of recruitment, selection, training, etc. involved in the expansion of their work forces.

In any event, the phenomenon of reported local labor shortages combined with employer unwillingness to raise the average rate of pay by as much as the offer of time-and-a-half would necessitate is consistent with another hypothesis advanced by Rees as applicable to this problem.[25] This hypothesis is that the industry's policy of setting steel prices below the levels at which the product markets would be cleared depressed the level of demand for its production labor. But now one might question his further claim that the union was in part responsible for this price policy.

Holding Down Prices and Wages. We have suggested that, in the pre-union era of the basic steel industry, the fear of future unionism (or, as it used to be called, "labor unrest") led employers to exercise restraint in granting wage increases in response to labor shortages which they believed to be temporary. Now, in the postwar period under consideration, the expectation of a future tapering off in demand was widely held by the industry's leadership.[26] Therefore, even if the steel union had been nonexistent in 1946-48, it appears more likely that the industry would have exercised some moral restraint in its wage-price policy than that it would have thrown caution to the winds. Hence one cannot easily accept the view that the substance of unionism contributed independently to the restrained behavior of the industry in 1945-48 if the same type of behavior would have been elicited by its shadow.

Of course one might maintain that the threat of unionism would exert a lesser degree of influence than the presence of unionism, but it would be at

[24] *New York Times*, Dec. 15, 1945, p. 12; *The Iron Age*, Aug. 15, 1946, p. 92. The *Times* report also noted that, despite the incomplete success of its recruitment campaign at the time, U. S. Steel was apparently making no effort to hire or retain "any considerable number of women, who held down 20 percent of the jobs at the Irvin works during the war."

[25] This theory of union accountability for price-wage inhibition in administered price industries was originally set forth by W. A. Morton, "Trade Unionism, Full Employment and Inflation," *Am. Econ. Rev.*, March 1950, XL, 18.

[26] U. S. Senate, *Steel Supply and Distribution Problems,* op. cit., pp. 2002-3, 2005, 2007-8, 2010-11, 2016.

least as easy to argue to the contrary. In the first place, employers unionized at the outset would have to reckon with a higher probability of a strike for higher wages during the period of labor shortage than would nonunion employers. Thus, if unionized employers wished to secure a future wage rate with which no excess supply of labor would be associated, they would presumably have to choose between incurring a strike in good times and in the near future in order to hold wages below the levels at which markets could currently be cleared, and incurring a strike to reduce wages in some possible period of future labor surplus. Since the loss of business from a strike would be less and the prospects of their winning would be greater under the latter alternative, that alternative would presumably be preferred. Moreover, a firm like U. S. Steel, to which a policy of present restraint probably commended itself with particular force under nonunion conditions, found that unionism eliminated certain exceptional circumstances which had prevailed in the past. Since U. S. Steel had traditionally sought to play the role of wage and price stabilizer within the industry under nonunion conditions, it lagged behind other firms in reducing wages during the downswings of 1920-21 and 1929-32. A policy of wage restraint during periods of labor shortage might well have appealed to such a "leader" under nonunion conditions since it tended to minimize its differential losses during ensuing downswings. But once the union was able to inherit the stabilizer's mantle, the dominant firm no longer had such a strong private stake in holding wages below the levels at which they would have cleared the labor markets.

Of course, one need not rely upon the theory of unionism as a price depressant to account for the industry's conservative pricing policies during this period. In the light of the industry leaders' conviction (mentioned above) that postwar operating rates were abnormally high, their consequent reluctance to expand capacity in the face of strong public pressure to do so, their fear of price controls and their frequent desire to ration in the interests of their traditional customers, wage-price restraint is understandable enough.[27]

Now price restraint, whatever its cause, tended to hold down the rate of increase in the demand for production labor in basic steel; thus one need not abandon Rees' hypothesis that "the smaller increase in steel prices" in 1939-48, as against 1914-20, was a factor which helped to account for the smaller increase in money wages during the latter period. But if we refuse to agree that the 1939-48 price increase would have been greater in the absence of collective bargaining, we are certainly not obliged to conclude that the 1939-48 increase in money wages would also have been greater in the absence of collective bargaining. Rees' data (p. 398) reveal that the rise in steel wages relative to steel prices was much greater in 1939-48 than in 1914-20; in the earlier period base prices of finished steel rose by 144 per cent while wages rose 148 per cent, whereas in 1939-48, a price rise of 61 per cent was accompanied by a wage increase of 99 per cent. For reasons presented in this section, we regard it as improbable that the union was instrumental in holding prices down during the latter period; for reasons presented in Section

[27] *Loc. cit.; The Iron Age:* Jan. 29, 1948; June 10, 1948; Sept. 21, 1948; Oct. 7, 1948.

II above, we regard it as more likely that the union was a factor in forcing up the ratio of wage increase to product price increase in the second period.

IV. Recent Developments: 1948-1956

It is of interest to determine how the organized workers in basic steel fared since September 1948, which is the terminal date of Rees' study and the month in which their average hourly earnings reached their peak in the immediate postwar period. Limitations of space make it impossible to catalogue the changes in the conditions of employment secured in the six separate negotiations in the period 1948-56.[28] It should be noted, however, that changes in the geographical and occupational wage structures in the industry and in so-called "fringe benefits" (including pensions and insurance, paid holidays, shift differentials, and the Supplementary Unemployment Benefit Plan), as well as in basic wage rates were negotiated under collective bargaining during this period. This brief account, however, will consider only changes in the level of average hourly earnings, which, of course, fail to reflect some of the changes in the terms of employment other than basic rates. On the other hand, average hourly earnings have tended to increase more rapidly than contractual rates in this period; this is due in great part to the inclusion of an increasing proportion of the work force under incentive pay plans—a process, incidentally, in which the local unions involved have played a significant role.

Table II compares increases in average hourly earnings in basic steel with wage increases in some of the lightly organized industrial categories identical with or corresponding closely to those employed by Rees (those for which data have been continuously available) and in all manufacturing. The table reveals that basic steel's standing in the group including itself and the 9 lightly organized (in 1946) trades improved markedly between 1948 and 1956. Between 1939 and 1948 and between 1945 and 1948, 4 of these trades had obtained increases in average hourly earnings greater than those secured in basic steel. (Some of the factors contributing to this result were offered in Section I above.) But between 1948 and 1956 only the ice-cream workers received greater increases than the steel workers—a phenomenon most appropriate to the democracy's cold-war economy.

We need not repeat our early caveats concerning the interpretation of the results of such comparisons. They are consistent with the hypothesis that unionism exerted an independent impact on the level of steel wages in any or all or none of the periods considered. However, certain additional information makes plausible the hypothesis that the union probably did exert an independent influence on the level of money wages in basic steel after September 1948. In the first place, while the increase in basic steel production (33 per cent) over the period 1948-55 was about the same as the increase in industrial and manufacturing production as well as in gross national product in 1956 prices (34, 36, and 33 per cent respectively), employment in basic

TABLE II.—ORIGINAL LEVELS AND RELATIVE CHANGES IN AVERAGE HOURLY EARNINGS, BASIC STEEL AND OTHER SELECTED CATEGORIES

	(1) Average Hourly Earnings 1939	(2) 1948 to 1939	(3) Ratio 1948 to 1945	(4) 1956 to 1948	(5) 1956 to 1939
Basic Steel	.845	198	139	151	299
Crude Petroleum	.873	196	146	145	284
Nonmetallic Mining	.550	232	145	150	349
Confectionery	.492	214	137	142	305
Nonalcoholic Beverages	.556	192	131	147	282
Ice Cream	.626	187	142	157	294
Power Laundries	.417	198	124	127	252
Cleaning and Dyeing	.490	201	128	128	257
Wholesale Trade	.715	191	134	147	281
Retail Trade	.536	203	142	145	293
Total Manufacturing	.633	219	133	143	313

Source: Original data in *Monthly Labor Review.*

steel increased considerably less than employment in manufacturing and total civilian and nonagricultural employment—the increases being about 1.4 per cent in steel, 8.1 per cent in manufacturing, 6.4 per cent in civilian employment, and 12.4 per cent in nonagricultural employment. This suggests that increases in productivity and/or prices enabled the industry to expand output without placing its labor markets under heavy stress. Between 1948 and 1955 output per production-worker man-hour increased by slightly under 28 per cent. Since a recent BLS bulletin notes that factors making for retardation in the rate of increase in physical productivity were especially in evidence in the early years following the second world war,[29] it might be presumed that physical productivity increased somewhat more rapidly in 1948-55.

Moreover, the industry overcame its postwar reluctance to expand capacity; while total ingot and castings capacity decreased slightly between 1945 and 1948, capacity increased by over one-third between 1948 and 1955-56, although operating rates remained within the 90-95 per cent range.[30] Nor is the industry's post-1948 price history inconsistent with the view that it abandoned its postwar pessimism. Finished steel prices rose almost three times as

[29] U. S. Dept. of Labor, BLS Bull. No. 1200, *Man-Hours per Unit of Output in the Basic Steel Industry, 1939-1955,* Washington 1956, pp. 3-4, 8.

[30] Am. Iron and Steel Inst., *Annual Statistical Report, 1955,* New York 1956, p. 53.

rapidly as wholesale prices other than farm products and food between September 1948 and December 1956; in contrast, steel prices had risen very little faster than these wholesale prices between August 1945 and September 1948 and thus lagged considerably behind the latter over the entire period 1939-September 1948.[31]

Taken together, the increases in productivity, capacity, and prices might support the view that the industry was more able and would have been more willing (in terms of its expectations) to grant wage increases after 1948 than before, provided that it was obliged to do so. The relatively modest increases in employment combined with the relatively low quit rate in basic steel suggest that the industry was not under heavy market pressure to grant the wage increases which it did put into effect. On the other hand, the fact that—in addition to the installation of the pension and insurance plans in 1949—two of the five wage increases granted after 1948 were associated with strikes and that the 1955 settlement followed a stoppage of a few hours suggest that the industry was under real pressure from the union during this period.

LLOYD ULMAN*

Reply

I am pleased that after seven years Professor Ulman still considers my article of sufficient interest to warrant his careful analysis of it. However, I cannot agree that he has weakened my conclusions. In this reply, I shall examine in turn his discussion of the three main strands of my argument. Before doing so, however, I should like to restate the conclusion of my article. I concluded that wages in basic steel during the period 1945-48 were about what they would have been in the absence of collective bargaining. It seems somewhat more likely that collective agreements held wages down slightly in some localities than that they raised wages, but such effects if present would have been small. If Ulman feels that I implied that collective bargaining depressed wages substantially—say 10 per cent—he has misread me. If his own conclusion is that it raised wages substantially—say 10 per cent—I do not think the evidence supports him.

I. *The Interindustry Wage Comparison*

For the purpose of estimating union effects, wages in the steel industry should ideally be compared with wages in industries that are like steel in every important respect except that they are unorganized. There are, of course,

no such industries. But despite their limitations, interindustry comparisons are of some value. Ulman suggests that this value might be increased by the explicit control of differences among industries in such variables as productivity, initial levels of earnings, and the sex composition of employment. Since his stated purpose is to challenge my conclusions, and not merely to criticize my methodology, he has not pursued these issues far enough. One must ask in which direction and by how much the failure to control for these variables affects the results. To throw light on this question, I shall use such additional data as I have been able to assemble on short notice.

Productivity was considered in my original article; I constructed an index of output per man-hour in the steel industry before the BLS index for this period was available. However, data for all manufacturing were not then available for comparison.

A comparison of changes in output per man-hour in steel with available data of other industries is shown in Table I.

TABLE I.—INDEXES OF OUTPUT PER MAN-HOUR, SELECTED INDUSTRIES, 1948

(1939 = 100)

Basic steel	
Original estimate	130[a]
BLS series	126[b]
All manufacturing	111[c]
Confectionary	115[d]
Ice cream	139[e]

[a] A. Rees, "Postwar Wage Determination in the Basic Steel Industry," *Am. Econ. Rev.*, June 1951, XLI, 394.

[b] Computed from Joint Economic Committee, *Productivity, Prices, and Incomes*, 85th Cong. 1st Sess., p. 220.

[c] Computed from *ibid.*, p. 148.

[d] *Productivity Trends in Selected Industries*, BLS Bull. 1046, Washington 1951, p. 8.

[e] *Ibid.*, p. 11.

It is unfortunate that BLS productivity indexes have been published for only two of the nonunion industries in my wage comparison, one of which (ice cream) is an exception to the general wage pattern. However, the large difference between the productivity indexes for basic steel and all manufacturing suggests that output per man-hour rose more in steel than in most other industries during this period. Previous studies have shown, as one would expect, a positive relation among industries between changes in output per man-hour and changes in average hourly earnings.[1] It thus seems clear that productivity differences among industries were a factor tending to raise relative wages in steel, and that explicit control for differences in productivity would have strengthened my conclusions.

It was not possible for me to use the technique used by Levinson and by Ross of making wage comparisons among industries grouped by initial wage

[1] See, for example, J. W. Garbarino, "A Theory of Interindustry Wage Structure Variation," *Quart. Jour. Econ.*, May 1950, LXIV, 298-99.

level. As Ulman points out, only one weakly organized industry had an initial wage as high as that of steel. However, Levinson, who applied this technique to all unionized industries, reached conclusions similar to mine. Of the period 1941-47 he writes "All comparable wage-earner groups in the manufacturing, extractive, public utility, and construction industries obtained approximately equal wage increases, regardless of union strength."[2]

Ulman makes a good point about the sex composition of employment in the industries I compared. Steel is almost exclusively male, while some of the comparison industries employ many women. Moreover, sex differentials narrowed to varying extents within almost all industries between 1939 and 1948.[3] Thus I tended to overstate the rise in earnings of males in other industries relative to earnings of males in steel. However, allowing for this effect does not seem to alter my results materially, as shown in Table II.

Table II is an expansion of Table IV of my original article. A column has been added showing the per cent of female employment in each industry in 1939. Estimates of wages increases for males only have been made for the three industries for which data are available on average hourly earnings by sex for 1939 and 1947 or 1948. The wage increase figures have been separated into two groups: Column 4 shows the wage increases in the seven industries with more than 20 per cent female employment in 1939. Column 5 shows the new estimates for males only in three industries, and the original data for industries that were more than 80 per cent male in 1939. A comparison of the wage increase in steel with the other increases shown in column 5 does not lead to conclusions different from those of the original table.

Whether wage controls were more effective in steel than in the comparison industries does not seem to me to be a relevant issue. The terminal date of my comparisons is long after the end of wage control and there was ample time for wages to return to levels determined by other forces.

I agree that wage increases won by unions often influence the wages of nonunion employers. It is this that requires that the union and nonunion earnings considered be for establishments that are not too similar. For example, one obviously could not compare union and nonunion steel plants. However, in the industries selected as weakly organized, the threat of union expansion during this period seems weak enough so that it could not have appreciably affected average earnings in the industry.

Ulman's major criticisms of the interindustry comparisons work in opposite directions. When they are all taken into account as far as possible, the comparisons still suggest that wages in basic steel from 1939 to 1948 rose by approximately as much as they would have risen in the absence of a union.

II. Comparisons Between Two War Periods

Ulman argues that the conclusions I draw from comparing the two war periods are invalid in part because the increase in steel employment was larger

[2] H. M. Levinson, *Unionism, Wage Trends, and Income Distribution 1914-1947*, Michigan Bus. Stud. Vol. X, No. 4, Ann Arbor 1951, p. 67.

[3] See the data on average hourly earnings by industry and sex in various issues of the National Industrial Conference Board *Manag. Record*.

TABLE II.—COMPARISONS OF CHANGES IN AVERAGE HOURLY EARNINGS
SELECTED INDUSTRIES, 1939–1948

Industry and Sex	Per Cent Female 1939ᵃ	Per Cent Union 1946ᵇ	Average Hourly Earnings September, 1948 as Per Cent of 1939 Average	
			More than 20% Female	Less than 20% Female
(1)	(2)	(3)	(4)	(5)
Basic steel	0.9	80 to 100		199
Crude petroleum	3.2ᶜ	20 to 39		196
Nonmetallic mining	1.7ᶜ	20 to 39		233
Cotton textiles	38.4	20 to 39	289	
Males	—	—		279ᵈ
Silk & rayon textiles	49.7	20 to 39	281′	
Males	—	—		270ᵉ
Confectionary	66.5	20 to 39	221	
Males	—	—		204ᶠ
Nonalcoholic beverages	2.8	20 to 39		193
Butter	14.4	20 to 39		223
Ice cream	86.2	20 to 39	187	
Power laundries	64.5ᵍ	20 to 39	199	
Cleaning and dyeing	30.3ᵍ	20 to 39	197	
Wholesale trade	19.3ʰ	1 to 19		193
Retail trade	34.1ⁱ	1 to 19	203	

ᵃ Computed from data for wage earners, *Census of Manufactures*, 1939, Vol. I, pp. 70–85 except as noted.

ᵇ For sources and notes, see Rees, *op. cit.*, p. 399.

ᶜ Data are for April 1940 and include salaried workers, computed from *Census of Population, 1940*, "Labor Force, Sample Statistics, Industrial Characteristics."

ᵈ Male earnings in 1939 were estimated from (a) ratios of male to female average hourly earnings in the industry "cotton-North" in June 1939, computed from the data of the National Industrial Conference Board; (b) the ratio of female to male employment shown in column 2; and (c) BLS average hourly earnings in the industry "broad-woven cotton textiles" for all of 1939. Male earnings for 1948 were similarly estimated, using NICB data for June 1948; BLS data for September 1948; and the 1939 employment weights by sex.

ᵉ Same method as in cotton, using the NICB industry "silk" in 1939 and "silk and rayon" in 1948.

ᶠ Same general method as in cotton. Ratios of male to female earnings for 1939 were computed from *Mo. Lab. Rev.*, Mar. 1940, L, 629, based on BLS data for twelve states. The ratio applied to the 1948 wage for both sexes is for "candy and chocolate" for January 1947, from BLS Industry Wage Studies Bull., Wage Structure Ser., No. 55. A very similar ratio for the confectionary industry in Illinois is given in *Illinois Labor Bull.*, Jan. 1948.

ᵍ *Census of Business*, 1939, Vol. III, "Service Establishments," p. 59.

ʰ *Ibid.*, Vol. II, "Wholesale Trade," p. 15.

ⁱ *Ibid.*, Vol. I, "Retail Trade," p. 21.

in the first than in the second world war. For "illustrative purposes" he estimates the percentage increase in wages for 1939-48 on the assumption that it bore the same relation to the percentage increase in employment as in 1914-20. This yields the three estimates of the 1939-48 wage increase of 71, 66, and 62 per cent. The actual increase exceeds the middle estimate of this set by 32 per cent. The illustration seems to suggest that without the

union wages might have increased only by the estimated percentage, and
that the union won increases one-third larger than would have been obtained
in its absence. Such power would equal that of strong craft unions under
highly favorable circumstances. Later Ulman warns that the illustration can-
not be used in this way. One is left to wonder whether it can be used at all.

The same technique could be applied to estimate the increase in earnings
in all manufacturing. Employment of wage earners in manufacturing rose
28 per cent from 1914 to 1919[4] and average hourly earnings rose 114 per
cent.[5] From 1939 to 1948 production-worker employment in manufacturing
as reported by the Bureau of Labor Statistics increased 52 per cent. On the
assumption that the increase in earnings bore the same relation to the in-
crease in employment from 1939 to 1948 as it did from 1914 to 1919, we
obtain an "estimated" increase in earnings for 1939-48 of 212 per cent! The
actual increase was only 113 per cent according to BLS data. But the extent
of unionization in manufacturing was much greater in 1939-48 than in
1914-19. Does the calculation therefore suggest that unionization held down
earnings in manufacturing during the second world war? I think not.

The difficulty with Ulman's technique is that it assumes that the supply
schedule of labor to the steel industry was (a) the same in both periods
and (b) highly inelastic in both—that a wage increase of 147 per cent was
needed to expand employment 54 to 62 per cent. Ulman recognizes and dis-
cusses the first of these assumptions, but not the second. Viewing these
money-wage increases from the supply side, we must think of a rising level
of the supply schedules and not of movements to the right along steeply
sloping industry schedules. In other words, the wages an industry must pay
during an inflation depend only in small part on the extent to which that
industry expands, and in large part on the tightness of the general labor
market. Inelastic supply to an industry must result from the specialization
of labor to industries, and this must be more true of skilled than of unskilled
workers. Ulman's reasoning thus suggests that the wages of skilled workers
will increase relative to those of unskilled workers during periods of rapid
expansion of employment. What we observe, of course, is the reverse.

In comparing the ability of steel mills to attract labor during the two wars,
Ulman makes too much of the differences between the periods and not enough
of the similarities. The principal difference was in hours of work. This is
exaggerated by talking about the 12-hour day as if it were the standard work
day for all steel workers during the first world war Of the 278,000 wage earn-
ers in basic steel in 1914, only 55,000 had a scheduled work week of 72 hours
or over. The work week of 194,000 workers was 60 hours or less—the 10-hour
day then common in most other industries.[6] Considerable progress had been
made in shortening the work week since 1909, in part as a consequence of the
Senate inquiry of 1911 which Ulman cites.

[4] Computed from Solomon Fabricant, *Employment in Manufacturing, 1899-1939*, New
York 1942, p. 214.

[5] Computed from BLS data given in *Historical Statistics of the United States, 1789-
1945*, p. 67.

[6] *Census of Manufactures, 1914*, Vol. II, pp. 205 and 223.

Many jobs in steel mills during both wars had other disadvantages that Ulman does not stress. They were hot, heavy, or dirty, and often all three. These factors were mentioned in accounts of recruiting difficulties in the period 1939-48.[7]

III. *Shortages and Expectations*

Ulman seems to believe that I assumed perfect competition in the steel labor market in the absence of unions, though I stated no such assumption. The term "perfect competition" can unfortunately always be used to deride economic analysis because it is obvious that no market, and certainly no labor market, can be truly perfect. I agree fully with George Seltzer who, in a companion study to mine, concluded that there was wage leadership in the basic steel industry long before the union. But he added "It should not be inferred from the preceding sections that the USA-CIO has had no influence upon wage leadership and wage uniformity in basic steel. Quite the contrary is true."[8]

There are several reasons for believing that the wage leadership of the period 1914-20 permitted more complete adjustment of wages to labor shortages than the collective bargaining of 1939-48. First, the leadership of the earlier period applied largely to the timing of wage increases, and even there was not complete. It permitted substantial geographical differences within the North, as well as some local differences, in the amounts of increases and the levels of wages.[9] The geographical differences were well suited to distinguishing between markets where labor was scarce and those where it was not. In contrast, after May 1946 the only remaining geographical differential was that between the North and the South. Second, in the earlier period there was no barrier to increasing wages for particular occupations not specialized to steel in which there were labor shortages, though such increases announced by one producer would probably have been followed by other producers in the locality. After January 1947 all jobs were classified in a contractual system of skill differentials. A wage increase in an occupation where there were labor shortages could not be made without also increasing wages in other occupations in the same job class where labor was plentiful. Finally, the practices of the earlier period permitted more frequent adjustment of wages; wages in some mills were raised as often as three times in one year.

Ulman states that the five wage increases between August 1918 and August 1923 were "too little or too late to remove the specific shortages which had provoked them." This suggests that he has evidence of labor shortages immediately following these increases, but unfortunately this evidence is not presented. If employer collusion holds down wages in the absence of unions and unions reduce this effect, the evidence of labor shortage should be clearer for 1918-23 than for 1945-48. There is, however, clear evidence that the in-

[7] See, for example, U.S. Employment Service, "Labor Market Information," Indus. Ser. No. 33-1, Curr. Suppl., Apr. 1946 (mimeo.).

[8] "Pattern Bargaining and the United Steelworkers," *Jour. Pol. Econ.*, Aug. 1951, LIX, 324.

[9] These statements are based on unpublished tabulations of common labor rates in eight steel mills in seven localities, which were kindly furnished to me by John T. Dunlop.

dustry did not always follow the advice of W. I. King against wage increases that must later be rescinded. Wages in some mills were cut on as many as four different occasions during the recession of 1921, and these cuts were not fully restored by the increases of 1922-23. There have been no such wage cuts since the second world war, though recent recessions, of course, have been milder than that of 1921. Although these differences between these two periods have many causes other than collective bargaining, certainly collective bargaining contributes to them.

I could not include in my article all the evidence of labor shortages during 1946-48 collected in my study. All of it related specifically to production workers in basic steel, and it came from a variety of sources.[10] Evidence of unemployment in the whole labor force of the Chicago area is hardly relevant to this point. These unemployed included women, white collar workers, and others not suitable for steel employment.

Ulman emphasizes the availability of overtime as a way out of labor shortages. But this is a very expensive way out. Apart from any effect of longer hours on efficiency, overtime hours cost 50 per cent more than straight-time hours. The use of substantial amounts of overtime may thus be much more expensive than straight-time wage increases designed to meet particular shortages if these shortages are expected to persist for any considerable time. The failure to use substantial amounts of overtime work therefore does not contradict the evidence of shortages of labor at prevailing straight-time rates.

Ulman's final objection to my position concerns wage-price relations. Three main hypotheses have been advanced to explain the underpricing of steel and automobiles immediately after the second world war: (a) fear of government control or antitrust action; (b) fear that temporary price increases might help unions win permanent wage increases; and (c) the desire to build customer good will (steel) or strong dealer organizations (automobiles). Like Ulman, I believe the first of these to be the most important.[11] Since I know of no evidence that enables us to to discriminate among these hypotheses, I cannot prove that the second one has any validity. But neither has Ulman proved that it has not. Pricing decisions in oligopolistic industries are complex, and need not be attributed to single causes. Decisions to sell at prices far below those that would clear the market require a lot of explaining, and all the hypotheses together seem barely adequate.

IV. Conclusion

Ulman's section on developments since 1948 I do not regard as relevant to the main point of my article. It suggests, however, a misunderstanding of the way in which my results can properly be generalized. Ulman seems to read my conclusion as implying that the United Steelworkers has always been a union without economic power. But this inference is unwarranted. Indeed, I specif-

[10] For further details, see my unpublished doctoral dissertation, "The Effect of Collective Bargaining on Wage and Price Levels in the Basic Steel and Bituminous Coal Industries, 1945-48," University of Chicago, pp. 38-42.

[11] See my article "Wage-Price Relations in the Basic Steel Industry, 1945-48," *Indus. Lab. Rel. Rev.*, Jan. 1953, VI, 193-205.

ically attributed the 1949 pension settlement to the power of the union. I also believe that the union won wage increases in 1937 that would not have occurred without it. A thorough study of the effect of the union on steel wages since 1948 would now be of great interest, and I can only guess within broad limits what it might show.

The general conclusion that my study does support is that collective bargaining *during periods of rapid inflation* is very unlikely to make wages rise faster than they otherwise would, and is quite likely to cause them to lag. This conclusion is now supported by additional evidence drawn from other industries, including the industries that were unionized in 1914-1920.[12] The conclusion is also supported by Levinson's study. He finds no union impact on wages in 1914-20 or in 1939-47, but does find an impact in the period of price stability from 1923 to 1929.[13]

These findings suggest that collective bargaining is not among the causes of rapid inflation. Whether it is among the causes of gradual inflation must still be regarded as an open question, though the burden of proof rests on those who feel that it is.

ALBERT REES*

[12] See S. P. Sobotka, "Union Influence on Wages: The Construction Industry," *Jour. Pol. Econ.*, Apr. 1953, LXI, 137-43; and Elton Rayack, "The Effect of Unionization on Wages in the Men's Clothing Industry," unpublished doctoral dissertation, University of Chicago. Both of these studies show strong union effects on wages in periods other than 1914-20 and 1939-48.

[13] *Op. cit.*, pp. 47 and 66-67.

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Errata

The third sentence in paragraph three of the note "External and Internal Public Debt," by James M. Buchanan, in the December 1957 number of the *American Economic Review* (p. 995) should be changed to read as follows: "If a given state or community could be confronted with two alternative situations identical in all respects save that in one an internal debt service charge is present while in the other such a charge is absent, the *second* is obviously to be preferred."

The Cambridge Economic Handbooks series under the joint editorship of C. W. Guillebaud and Milton Friedman is now being published in this country by the University of Chicago Press. *The Economics of Underdeveloped Countries* by P. T. Bauer and B. S. Yamey is the first volume published under the new arrangement. The review of the book in the March 1958 number of the *American Economic Review* (pp. 170-71) did not include the University of Chicago Press as the American publisher.

BOOK REVIEWS

General Economics; Methodology

Economics in the United States of America: a Review and Interpretation of Research. By RUTLEDGE VINING. (Paris: United Nations Educational, Scientific, and Cultural Organization. 1956. Pp. 62. $1.00.)

Professor Vining's short "review and interpretation" of recent developments in economics in the United States is one of several publications by Unesco in its series *Documentation in the Social Sciences,* an international "charting of research activity in the social sciences." Although the foreword describes Vining's review as a "report," it is in fact a critical essay on methodology in economics. The principal subject of the essay is recent theoretical work on "game theory, statistical decision theory, the theory of linear-programming and the like." The text of the essay, at least the part of it that I think I understand, is that a "political economist must search with care in order to find within this work techniques or materials directly applicable to the study and analysis of economic problems of the 'classical' type."

Vining starts his review with the observation that "it is in the forms in which the problems are considered . . . that we may observe the trends of economic research." Economic problems, says Vining, are always problems of choice among alternatives. Two types of choice problems, however, must be sharply distinguished from each other. The first type involves choice among means for a well-defined end. Thus, for example, a business enterprise faces the problem of choosing among alternative rules of action that rule which will lead to a maximum of a specified kind of output from given inputs of productive services, or the rule that will minimize costs of a given output, or the rule that will maximize the net worth of the enterprise. The rule that will be chosen or "ought to be chosen" in a choice problem of this type depends, of course, not only on the well-defined objective to be attained but also on the restraints imposed by the physical universe and by the legal and regulatory institutions of the social economic organization.

Many of the recent theoretical developments—such as game theory, linear programming theory, information theory, and queueing theory—Vining states, have been helpful both in clarifying the logic of this type of problem and in solving substantive problems in the real world. Operations research, to which Vining devotes three pages in the second part of his essay, is an excellent example of a field whose subject matter has been explicitly limited by its professional practitioners to problems of this first kind: to understand and hence ultimately to modify and improve the operations of a group whose activities are directed toward a well-defined objective.

The "means-end" problem is essentially an engineering one. The tasks that have to be performed in solving such problems are accounting for alternative systems of rules of action, defining clearly the objective to be achieved, pre-

dicting the performance of alternative rules, and ranking the rules according to the goodness of their performance. "The specialization of any expert who tenders advice on problems of choice," Vining states, "lies in his ability to analyse special kinds of rules of action, and the statements which he makes as a specialist have reference to one or another of the above tasks."

Conventional economic theory, particularly those parts labeled the "theory of the household" and the "theory of the firm;" often is couched in language that makes it sound as though it were designed for the purpose of solving problems of choice of this first kind. Vining takes pains to point out, however, that those who interpret conventional theory this way grossly misunderstand it. He emphasizes that the classical economic theory of the political economist does not inquire into how a firm or a household solves its problems of choosing among means for well-defined ends, and in particular is not concerned with advising them on how they "ought" to solve problems of this type. Instead conventional theory simply assumes that firms and households do select "optimum" rules; deduces from these "solutions" certain equilibrium conditions (equalities and inequalities among marginal rates of substitution); and builds on these a theory of the market.

That the traditional tools of economic analysis were not designed for the purpose of advising firms and households on how to manage their business affairs has been said often before. But it needs to be said again. Much of the dissatisfaction among "business" and "labor" economists with economic theory undoubtedly stems from their disappointments and frustrations in trying to apply economic theory to the problems of individual business firms, unions, and the like.

If the purpose of conventional theory is not that of assisting firms and households to manage their affairs "rationally," then what is its purpose? This brings us to Vining's second type of choice problem: "the individuals constituting a . . . society of free agents jointly choose the constraints and regulations which they impose upon their individual actions. The decisions made by a legislative body in the drafting of legislative acts are examples of choices of this . . . type."

A free society is a commonwealth of free individuals engaged in a more or less continual review of the performance of the laws of the society and continually striving by discussion to reach consensus on the choice of laws. A free society is not a task-oriented group controlled by some decision-maker who seeks best rules for managing the group toward some well-defined objective. (Vining, in a four-page comment in the second part of his essay, scolds "welfare economists" for couching welfare economics in language that makes it sound as though the problems of choice facing a society of free individuals were simply means-ends problems of the first kind. It is not clear whether he intended his criticism to go to the substantive propositions of "welfare economics" or only to the language used in some expositions of welfare economics.)

Conventional economic theory, of course, is not a theory explaining how a free society chooses its laws or ought to choose them. Vining is absolutely right, however, in emphasizing that the work of the political economist, the

tools of whose trade are conventional theory, has been directed toward informing discussion among free citizens on their choices among alternative laws. The task of the political economist has been and is that of predicting the consequences—for prices, rates of consumption, and the like—of alternative laws. It is in this sense then that classical economics has been concerned with problems of choice of the second kind.

The trade of the political economist is not the trade of the management or "efficiency" expert. The tools of the first trade were not designed to serve the second one, and, indeed, they have little usefulness for the second. In the same way there is no presumption that the tools of the efficiency expert will have much usefulness for the political economist. This double-edged proposition is an important one, particularly for a document such as this one sponsored by Unesco presumably addressed at least as much to economists in the universities and ministries of poor countries as to economists in the United States.

Intellectual tools have a habit, unfortunately, of not being neatly labelled. How does Vining, or any one else for that matter, know whether a recent theoretical development in "economics" is "designed" to serve the political economist or the operations researcher? Consider, for example, the recent literature on the problem of choice under risk and uncertainty. Many of those who have written in this field state explicitly that their contributions are designed to improve upon conventional economic theory, to make it more useful to the political economist. Should we not believe them? Language is no help here, for a contribution that sounds as though it is dealing with the first type of choice problem may, like the economic theory of the firm, be recast in language that is in the mode of conventional economic theory.

At this point Vining's methodological scheme breaks down and another test must be brought to bear: do the contributions enable the political economist to perform his tasks better? In fact this is the test that Vining applies to both game theory and "simultaneous equations econometrics." I am puzzled, therefore, by his treatment of game theory as work on the first type of problem—the choice of means for a well-defined end—while treating the "econometrics" approach to analysis of the business cycle problem as an example of poorly conceived work on the second type of problem—the problem of appraising an economic system.

I am even more puzzled as to Vining's purpose in the last 19 pages of his essay. In these pages he recounts the discussions in official circles, both in the 1920's and in recent years, of the problem of economic instability; and he reviews briefly and favorably the empirical work on the business cycle by Arthur F. Burns. He emphasizes the continuing ambiguity of the concept of "economic stability" and laments the poverty of political economy as an intellectual discipline in social discussion of the stability issue. At the end of the essay he expresses the belief that a useful theory of business-cycle phenomena, based on Burns' work, eventually will be developed.

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An Economic Theory of Democracy. By ANTHONY DOWNS. (New York: Harper & Bros. 1957. Pp. viii, 310. $4.50.)

Dr. Downs applies to the study of democratic politics the methodology of model-building that is now familiar in economics. If political science is ever to evolve into a more theoretically grounded discipline, it is doubtful that Downs charts the way. Even a skeptical reader, however, will find his novel analysis stimulating and provocative. Although the theme is politics, special attention is devoted to economic applications. As an exploratory study in a central field, the volume should be of interest to social scientists generally.

Downs has a carefully restricted concept of democracy: Politics are the concern chiefly of political parties and individual citizens. A political party is a limited team of office seekers who act only to achieve the power, prestige and income that come with public office. Good deeds are not excluded, but they are only means to these private ends. The members of any one team agree on the deeds (good and bad) that are expedient. The individual citizen has consistent and fixed "political tastes." As far as available information permits, he values the results of different governmental activities in these terms. He votes periodically for one or another party on this basis. Elections are decided by majority rule. All offices go to the victorious party. If there are more than two parties and coalitions, the government is formed by any cooperating group of parties receiving a majority of the total votes.

Under these and other assumptions, Downs analyzes the functioning of democratic politics. A central thesis follows at once: democratic parties seek to achieve only one goal, the maximization of votes. Under alternative assumptions of certainty and uncertainty, further implications are explored regarding the nature, possibilities and consequences of political rationality.

A theory is never fully realistic as to assumptions, and even a very unrealistic one can be suggestive. Downs' analysis nevertheless is properly appraised in terms of assumptions as well as implications. As the author freely acknowledges, his model excludes vital aspects, but as a working hypothesis the basic postulate on party motivation is compared with the corresponding behavioral postulates of economics, from which (as the title of this volume underlines) it is adapted. Adam Smith's famous reasoning about the motives of the butcher, the brewer and the baker supposedly applies "equally well to politics." Therefore, "we accept the self-interest axiom as a cornerstone of our analysis" (p. 28).

One need not consider public officials as a breed apart in order to question this view. After all, social consequences are generally far more important and manifest in public political than in private economic choices. The butcher, the baker and the brewer can less easily afford to consider such consequences than the public official. Failure to *maximize* votes may still leave a party with a victorious majority but failure to maximize profits necessarily involves a financial sacrifice. Regarding income alone, politics are hardly very rewarding; and many persons enter this field only after they have made their fortunes. Perhaps the economist himself is often misguided about motives, if not of

bakers then of corporation executives, but this is no reason to compound his error in politics.

In appraising the remaining assumptions, it should be considered that at least under uncertainty many familiar features, such as persuasive leadership, ideologies, etc., are admitted. The analysis nevertheless still seems to exclude other no less familiar features, such as persuasion regarding ends as distinct from means, "traditional" and "neurotic" voting, falsification, etc.

Among many diverse findings, perhaps most novel is the conclusion that democracy cannot stand certainty. In a two-party system, for example, rationality on the part of an incumbent party calls for it to adopt and to promise to adopt policies on every issue that are favored by a majority of citizens, but unless there is a strong consensus, the opposition may still gain a victory by uniting passionate minorities. If the incumbent party is itself solicitous of such minorities, as can easily be shown, it still may be defeated by appropriate opposition tactics. Moreover, rationality on the part of either party is thwarted to begin with if (as is almost inevitable) there is some one issue on which the majority preferences are inconsistent, *i.e.*, the majority prefers A to B, B to C, and C to A. If (as Downs assumes) voters are guided primarily by past performance, it follows that the incumbents have no incentive to cater to voters' preferences while in office; if promises are what count, elections are decided by the extraneous fact of who commits himself first. These unhappy consequences may still occur but are not inevitable under uncertainty, since all calculations are then made difficult.

For sake of clarity, it may be well to observe first that the two situations which Downs distinguishes where rationality breaks down are in reality essentially one. As may not be stressed sufficiently, what counts ultimately is not the voters' preferences on individual issues but their preferences as between all combinations of all alternative policies on all issues. Passionate minorities cause difficulties, therefore, only if they result in an inconsistency in collective preferences as between alternative combinations of policies of the same sort as occurs if the collective preferences are inconsistent as between alternative policies on one issue. Barring such inconsistency, there is always a program that cannot be beaten. If a tie vote is taken to indicate collective indifference, the reader may readily see how impassioned minorities give rise to inconsistency in collective preferences by working out the majority preferences between alternative policy combinations in an example given by Downs: Table 2, p. 64.

Downs very possibly helps illuminate here a political phenomenon of genuine interest: how passionate minorities make for unstable majorities. One wonders, for example, whether his abstract logic may not have some sort of application to the French parliament. On the other hand, the disconcerting implication that the effective functioning of democracy requires mistaken calculations should be read in the light of the special nature of the model, including the restricted role allowed to persuasive leaders and the assumption about party motivation. If the incumbents have any other than private ends, for example, rationality still is possible (but contrary to what was said

above, given certainty, a benevolent party that fails to maximize votes faces inevitable defeat unless such benevolence is universal, *i.e.*, unscrupulous opponents may take over all of the party's program except for vote-costly benevolent features). Additionally, in Downs' model public office supposedly confers practically unlimited authority. In the real world, the powers of democratic governments are always limited, and partly to areas of relative consensus.

Under certainty, Downs apparently considers that democracy would escape difficulties if there were no impassioned minorities or inconsistent collective preferences on any one issue. Given his theoretic model, however, the incumbents could always formulate an unbeatable program. This might be very discouraging to the opposition.

Inconsistency in collective preferences is deservedly referred to in this study as the "Arrow problem." Although Arrow envisaged the inconsistency as posing a problem of welfare economics, many suspected that it was more pertinent to politics. This study happily makes this fact all the clearer.

The introduction of uncertainty inevitably blurs and complicates the model, and some may wonder whether with this the possible advantages of model building may not be largely lost, but Downs succeeds in illuminating in a striking way the pervasive role of this factor. For students of politics, I suspect one of the more valuable features of the book will be the suggestive elaboration of such aspects, for example, as the relation between uncertainty, party ideologies, and the distribution of power. In order to explain competition between different party ideologies, an ingenious adaptation is made of economic theories of spatial competition.

Under both certainty and uncertainty, government measures affecting resource allocation are examined with special reference to the attainment of a Pareto optimum, *i.e.*, there are no unexploited opportunities for some to gain without others losing. The author treads on familiar ground in stressing the great practical difficulties that are encountered in public action towards this end in respect of collective goods or otherwise. On the other hand, given his general thesis on governmental behavior, the usual arguments regarding the possible adverse effects on economic efficiency of political income redistributions necessarily gain in force. A novel, perhaps useful finding is that the Arrow problem may preclude the continuing realization of a Pareto optimum even if practical difficulties are absent.

Probably of more interest to economists than the discussion of resource allocation will be the thesis that at least in their normative theories, they erroneously assume that democratic governments seek to maximize "social welfare." In reality, it is held, governments consist of men who pursue private ends within the framework of a given political constitution. In prescribing policy, the economist must not assign the government "a social function inconsistent with those motives and that structure, unless he is deliberately advising a change in society's political constitution" (p. 291). Apparently his task rather is to advise the government "how best to please the voters" (p. 292).

Very possibly, economists do tend to assume too easily that public officials are social-minded. It is permissible to doubt nevertheless whether the alternative standpoint proposed in this volume is any more correct. In any case, a question is in order concerning methodology. Telling the government "how best to please the voters" may mean many things, including doing just what normative economics always has been doing. But perhaps understandably some may balk if (as Downs seems to mean) the economist is to take as normative data the private ends of public officials, and a "political constitution" which apparently embraces the momentary distribution of political power. Tariffs may be vote-getters, but this is not a sufficient reason for the economist to advocate them. Downs considers that the normative economist may also counsel individual voters (as distinct from public officials), but has little to say about this approach. On the supposition that public officials are to be counseled in terms of essentially the same values as citizens generally, it has seemed to the reviewer that the counseling of citizens is properly the central task of normative economics.

A recurring theme of this study concerns the need to integrate political and economic theory. For purposes of both positive and normative economics, it is argued, "economists must develop models which unify politics and economics, as we have done in this study" (p. 294). With a fully developed theory of politics, we may agree that the economist might gain in diverse ways from being able to predict political behavior. It does not deprecate Downs' achievement, however, to feel that such eventualities must lie far in the future, if they are ever to be realized at all.

The study concludes with an extensive list of "testable propositions derived from the theory." The list is convenient, but the author perhaps has done himself an injustice by including a good many propositions that clearly fail to be of the "nonobvious" sort sought; for instance: "Democratic governments tend to redistribute income from rich to poor"; "Many citizens who vote and consider voting important are nevertheless not well informed on the issues . . ."; "Democratic governments tend to favor producers more than consumers in their actions."

ABRAM BERGSON

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Economic Fictions—A Critique of Subjectivistic Economic Theory. By PAUL
    K. CROSSER. (New York: Philosophical Library. 1957. Pp. xxiii, 322.
    $4.75.)

For many students of the history of economic reasoning Crosser's book will provide irritating reading. It consists of a series of essays dealing in turn with the "most representative writings" of the "most representative authors of the subjectivistic school of economic thought," namely Thünen, Menger, Wieser, Böhm-Bawerk, J. B. Clark, W. S. Jevons, Schumpeter, Keynes and O. Spann. Crosser has dipped his pen deeply into a bottle filled with philosophical phraseology in order to demonstrate how the teachings of the "classical economists," Adam Smith and David Ricardo have been "deconceptualized, designified,"

transformed into "paradoxes," brought to "meaningless and nothingness" by
the marginalist economist, how the teachings of Schumpeter are marked by
Intuitionism, those of Keynes by Alogism, those of Spann by Mysticism.

Characteristic of Crosser's attitude towards his problems is a statement in
which he identifies the "subjectivistic form of reasoning" with the elimination
of the distinction between the imaginary and the nonimaginary, between
"fictitiousness and reality," and defines "fiction" as a "postulation which is
based on the assumption that a distinction between reality and appearance
constitutes an inconceivable proposition."

It might not be amiss to contrast with that problematic definition of "fic-
tion" another which enjoys widespread acceptance and reads that "fiction
is a statement known to diverge from the results of our observations and to
contain an element admittedly false (See the *Encyclopedia of the Social
Sciences*, Vol. VI, p. 227). Thus defined, "fictions" have been a perfectly legiti-
mate, even indispensable instrument of all patterns of thought which are not
based on the belief that "reality" can be grasped directly by human reason
and that "truth" can be established directly with the aid of appropriate
concepts. When Crosser places Newton's famous phrase *Hypotheses non fingo*
as a motto at the head of his book, he seems to overlook the fact that
Newton's "atom," the indivisible standard unit of the law of gravity, was
a fiction; when he extols the logic of the "classicists," Adam Smith and David
Ricardo, he ignores the fact that in various passages of their writings the
standard unit of labor costs is presented as something like a fiction; he obvi-
ously ignores an interesting chapter of H. Vaihinger, *Philosophy of As If*
(Transl. 1925) dealing with the role of fictions in Adam Smith's *Wealth of
Nations*.

It is obvious that an intensified use of fictions has been made by subsequent
authors who realized far more clearly than had Smith or even Ricardo, that
the human mind can, at best, arrive at coherent and consistent pictures of
reality constructed in accordance with the rules of human reasoning. It is a
moot question to what degree the introduction of such fictions into economic
doctrine might have led to erroneous interpretations of economic relationships
and events, and Crosser would have rendered a great service not only to the
history of economic thought, but also to that of Western reasoning if he had
carefully analyzed the logical background of the various fictions which have
played changing roles in economic doctrines. Instead of undertaking a study
of this type, he has considered it his main task to oppose to the "postclassical"
developments of economic reasoning the firm belief in some of the main logical
principles of Ricardian economics: The conception of value as an outcome
of "societal appraisal," the existence of a clear line between "essential" and
"nonessential" characteristics, between the "lasting and the passing," the
"real" and the "apparent," the distinction between an "intrinsic" nonmonetary
value of the goods and an "extrinsic," nonessential monetary value; the dis-
tinction between the flow of money and the flow of products (p. 260); the
explanation of the elements of the distributive process (wages, rents, profits,
interests) in terms of social, not functional relationships and so on. On closer
analysis it appears that, what Crosser is fighting for is the preservation of a

medieval heritage: the belief that the substance concept and the consequences drawn from it are indispensable elements of conomic analysis.

KARL PRIBRAM

Washington, D.C.

Economic Issues—A Financial and Economic Debate in the Critical Years 1954-57. Edited by STEPHEN FROWEN and H. C. HILLMAN. (London: Waterlow and Sons Ltd. 1957. Pp. viii, 231. 21s. 6d.)

The thirty-three essays which compose this volume appeared originally in *The Bankers Magazine* in the years 1954-56. Among their authors are such well-known economists as Roy Harrod, Colin Clark, G. L. S. Shackle, Lawrence R. Klein and R. G. Hawtrey. The individual contributions are short—from five to ten pages each—and are concerned with a wide variety of topics clustered rather loosely about various policy issues which were matters for public discussion and concern in the period covered, such as inflation, monetary controls, budget policy, and problems of convertibility and international economic cooperation. Obviously it is idle to expect either a high degree of coherence or much depth of penetration in such a collection of short pieces culled from the pages of a journal whose principal function is not to advance economic discussion but to service the needs of the banking profession. Indeed, American economists may marvel that in England professional economists should be afforded such an opportunity to discourse directly with practical businessmen through the pages of a trade journal. How the economists responded to this opportunity to present their views—the topics they picked and their manner of approach—may well be of more interest to professional economists outside the United Kingdom than the particular views expressed.

The editors have done their best to provide a modicum of unity by grouping the essays under six general headings. The first group, called "Issues and Views," consists of five assorted review articles and a short essay on inflation. It is not at all clear why the editors chose to reprint the reviews, since none of them is in any way remarkable. The essay on inflation by M. J. Bonn provides a brief description of the relation of budget surpluses and deficits to changes in the money supply and strikes the first few chords in the motif of bank rate which later swells to full orchestra in the debate on monetary policy.

In the second group of essays the authors seek to acquaint their readers with some of the concepts which underlie national income accounting and to illustrate the application of national income statistics and forecasts to policy formation. In an article on "Capital" Colin Clark deprecates the relative neglect of national wealth measures, notes some of the difficulties involved, and endorses a method of valuation for which the rationale is anything but clear since it can result in the assignment of zero capital value to fully maintained and currently used capital equipment.

Possibly because bankers were the intended audience, the essays on the channels and effectiveness of montary policy are among the more vivid and lively in the collection. Articles by Harrod, Hawtrey and W. T. Newlyn explore questions of the timing of "the credit squeeze," whether it is the cost or

availability of money that is more significant, and the relative importance of commercial bank cash versus liquid-asset holdings as fulcrums for central bank policy. Hawtrey reiterates his view that the primary mechanism through which changes in interest costs affect production and employment is via traders' willingness to hold goods inventories. An essay by Shackle, included under another grouping later in the volume, expounds the familiar view that investment, especially that in fixed plant and equipment, is insensitive to interest changes. One is struck by the absence of fresh ideas and, more important, of the dearth of concrete empirical information which characterizes these exchanges. Surely it is not too much to expect that a thorough-going empirical investigation of the behavior of banks and other lenders as well as borrowers would contribute to the clarification of some of these much-debated issues.

The next two groups of essays are concerned with taxation, fiscal policy and related matters. Among the fresher contributions are three articles by Lawrence Klein utilizing materials drawn from the Oxford Savings Surveys conducted annually since 1952. In one he discusses the implications of savings habits of individuals for budget policy intended to encourage private savings, in another the practicality of estimating personal expenditures (to serve as the base for an expenditure tax) as a residual obtained by subtracting saving from income. The third compares some aspects of the situation of British and American consumers, e.g., sources of income, behavior concerning consumer durables, channels of savings, and so on as revealed in survey research studies in the two countries. In another essay David Walker suggests that inflation reduces the share of corporate profits which goes to the stockholder, since directors can maintain or even increase the dividend rate on issued capital while reducing the percentage of profits paid out.

The final set of essays devoted to international economic relations includes a comment by Harrod on the problem of convertibility as of late 1954, a couple of somewhat technical discussions on the European Payments Union and the General Agreement on Tariffs and Trade, a diagnosis by Hawtrey of the reasons for the weakness of the pound relative to the dollar (too much purchasing power at home directed toward domestic consumption), and an attempt by H. C. Hillman to assess the probable impact on the British economy of an American recession. The last of these gains currency from recent downtrends in the American economy.

There is little of substance in these essays for the professional economist. For the nonprofessional the collection may well lack the appeal of direct relevance to present policy issues. There is also the difficulty that the principle of selecting articles from a single journal inevitably sacrifices something of the quality and balance that could be achieved by drawing on a broader base. One can applaud the original purpose of the essays included in this volume—that of developing communications between economists and an intelligent public—and even admire the grace with which this purpose has been accomplished, while remaining unconvinced of the merit of such a reprint collection.

DONALD R. HODGMAN

University of California, Berkeley

Contributo alla teoria della politica economica. By FRANCESCO PARRILLO.
    (Turin: UTET. 1957. Pp. xv, 303. L. 2,400.)

Politica economica means "economic policy" as distinguished from *economia politica,* political economy or (as Alfred Marshall taught us to say) economics. Economic policy is not to be confused with action or administration. It is, or should be, a *guide* to action, "A bridge between the oversimplifications of economic analysis and the operational plane [of governmental activity in regard to economic matters]."

Classical and neoclassical economists, says Professor Parrillo (p. 204), tended to view economic life as self-contained and self-directed; the state was regarded as a thing foreign—and by inference harmful—to the body economic. On the whole, therefore, the economic policy most suitable for the state was thought to be one based on *non-agenda* rather than *agenda* (p. 297). Although the dominant role played by government in all modern economic societies—the author might well have added, archaic ones too—has not gone unnoticed by our profession, no consensus has yet been reached on what should be the first principles of an affirmative and scientific economic policy for the guidance of government. This book is an appeal for more attention to this question. It is also an excellent compendium of present and earlier speculations on the scope (but not method) of government's intervention in economic life.

The book's seven chapters are devoted mainly to four topics. Chapters 1 through 4, about a hundred pages in all, afford an overview of the "policy" doctrines expounded by influential 18th and 19th century economists. Chapter 5 provides a hundred-page exposition and critique of more modern Italian thought on the same subject, beginning (as might be expected) with works that appeared during the Mussolini régime—a period which in its impact on Italian economic thinking marks a turning-point as sharp as that which in America ensued from the great depression and the New Deal. Chapter 6 surveys in fifty pages the "theoretical systematization of economic policy by foreign contemporary economists." Included here are not only abstracts of the ideas of the "constructive contributors"—Hicks, Frisch, and a number of others—but also of other people who are to varying degrees skeptical or agnostic in regard to the possibility of finding any universals on which to found a rational public economic policy. Among these may be included Röpke, Myrdal, and of course Lionel Robbins. Finally, Chapter 7 enumerates various goals for economic policy that seem to have wide endorsement. Among these (p. 288) are development of greater productivity; rectifying cyclical fluctuations; broadening markets so as to absorb the added abundance to be afforded by improved techniques; and a better income distribution both spatially and interpersonally.

Parrillo of course perceives that some of these goals may prove mutually inconsistent; a strategy (policy) that will bring us closer to one may by its very adoption interdict us from reaching one or more of the others. He declines to prejudge their relative importance, but says (p. 286) that in science "objectivity can never be disassociated from the power of selectivity and critical judgment"; hence social science is not prima facie inadequate to the task of ranking values as well as advising on methods for achieving goals (this

latter being the particular province of economic science as such). Moreover, the author points out quite rightly that the anguish of choice between goals is not necessarily an unavoidable consequence of greater governmental economic intervention, *i.e.*, a strong "economic policy," and such a policy need not have as a built-in feature the greater abridgement of individual liberty. A policy that can greatly expand the parameters of the economic scheme, without unduly delimiting their mutual relationships—leaving these, and the relationships of the variables as well, to determination by market forces—will surely enlarge very greatly every individual's range of choices (p. 217, 298).

Politica economica gives all the evidences of excellent scholarship one is accustomed to expect from Italian economists. It would have been, however, more complete had the author made mention of J. K. Galbraith's very interesting view that a suitable component of governmental economic policy is the fostering of countervailing power. The different structure of Italian economic society may explain this omission, which in any case scarcely diminishes the merit of Parrillo's thought-provoking book.

A. STUART HALL

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The American Economy. By ALVIN H. HANSEN. (New York: McGraw-Hill. 1957. Pp. xv, 190, $5.00.)

This book is one of the Economic Handbook Series designed to serve as brief surveys in one-semester courses, as supplementary reading for certain courses and as an aid in the growing field of adult education. The book is in large part a series of six lectures given by the author at the University of Chicago with some supplementary material.

It reviews the economic advances of recent decades, and the gradual transformation of a *laissez-faire* economy to a mixed economy. There is much material on modern techniques for stabilizing and expanding the economy and on the relevance of Keynesian economics to modern problems. The book describes the important advances in economic thinking in recent years as well as those in the areas of stability, growth and distribution—advances which have been facilitated by the development of modern national-income theory.

The author's discussions of monetary policy in an advanced economy and of recent monetary issues are among the most suggestive sections of the book. In a concise analysis of the contemporary problem of inflation in a society committed to a policy of full employment he establishes the generalization that periods of rapid growth have usually been periods of moderate price increases. He then concludes that we probably cannot achieve our real growth potential in the next twenty years without a moderate increase in the price level.

There follows a provocative discussion of the effectiveness of the rate of interest as a means of economic control. This device, he believes, is now out of date because of its adverse effect upon capital values. Since monetary policy can play only a modest role in a stabilization program, primary reliance must be placed upon fiscal policy supplemented when necessary by selective controls of real estate and consumer credit. These general conclusions on monetary policy are buttressed by a discussion of recent monetary issues including

the method of carrying out open-market operations and the relations between the Federal Reserve and Treasury policies.

There is a lengthy section dealing with the Employment Act of 1946 and its history under both Democratic and Republican administrations, together with a summary and evaluation of much of the work of the Council of Economic Advisers and the Joint Economic Committee.

In an impressive chapter dealing with the problem of values in a rich society the author raises questions traditionally not considered a part of the discipline of economics. He suggests that just as Keynes broadened the scope of economics from Marshallian cost accounting and moved it into the realm of political economy, so today economics must develop a concern not merely for full employment and maximum output but also for social priorities, that is, it must become once again a branch of moral philosophy.

The book reflects the mature thinking of one of the outstanding economists of this generation. It will be of real interest not only to the serious adult reader but to the professional economist as well because of its comments on contemporary problems and because of its bold plea for a broadening of horizons within the profession.

JAMES F. CUSICK

Dartmouth College

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Price, Cost and Output. By P. J. D. WILES. (Oxford: Basil Blackwell. 1956. Pp. xi, 302. $4.20.)

When *describing* business enterprises, orthodox marginal analysis characteristically assumes that entrepreneurs (1) know what their marginal revenues and marginal costs are, and (2) are motivated to maximize profits by setting prices and outputs indicated by the equation of marginal costs with marginal revenues. When *evaluating* the performance of business enterprises as those institutions by means of which society allocates its resources to meet its wants, welfare economics based on orthodox marginal analysis notes that when demand for the product of the firm is not always perfectly elastic (as, of course, it is not except under competitive conditions) then resources are used in "derogation from consumers' sovereignty and the best allocation of resources." In viewing this orthodox marginal approach to the business enterprise, Wiles notes that (1) the desire to reach welfare conclusions resting on marginal concepts has pushed economists to logical discoveries as to how firms would act if they knew marginal revenues and marginal costs and maximized profits rather than toward factual investigations of actual firm behavior; (2) as a consequence of failing to make factual investigations, economists have inadequate classifications and descriptions of business behavior when marginal revenues and marginal costs sometimes are not known and at other times are not knowable, while at the same time firms do not maximize profits and are subjected to all sorts of pressures ranging from spe-

cific governmental controls to subjective pressures to be "fair"; and (3) the focus on marginal costs leads economists to ignore, or at least not to emphasize, those relatively more important factors which influence long-run average costs, factors such as technological change, improvements in managerial techniques, age of the firm, and the like.

Wiles' views as to the shortcomings of orthodox marginal analysis naturally affect the organization of his book. After introducing the subject and defining terms, Wiles describes the short-run price and output behavior of business enterprises within five descriptive categories. The five categories contain firms of the following different types: (1) "primitive higglers," firms which sell goods not produced by them, or goods for which they do not know costs, to unique customers; (2) "price-takers," firms which find prices set for them in perfect markets; (3) "full-cost producers," firms which are output takers and who set price at average cost plus a chosen margin; (4) "discontinuous producers," firms which produce unique articles, or unique batches of articles, one at a time; and (5) "marginal cost producers," firms which find it necessary to discriminate among customers with respect to price either to avoid losses or to economize on fixed facilities by "evening-out" the peak demand for a perishable product or service. Description of short-run price and output behavior of firms in these five categories points to new or modified meanings for the supply curve and profit maximization. Then follows material on the shape of long-run average cost curves—which are held to be L-shaped, not U-shaped, with a consequent absence of cost limits to plant and/or firm expansion—and determinants of price and output movements in the long run. A final chapter uses the welfare criteria established in Lerner's *Economics of Control* (New York 1944) to evaluate the performance of business enterprises as Wiles describes it.

In detail, not much of Wiles' work will seem new to economists whose interests have pointed them toward an examination of business enterprises and the important niche which they occupy in the economy. To say only this, however, would be grossly unfair to Wiles and to minimize the importance of a first-rate book. The book's importance stems from four characteristics: (1) the fact that it brings together in one book for meticulous examination most of the objections which economists have specifically presented, or inarticulately felt, with respect to the marginalist theory of the firm; (2) the fact that these objections are examined within a framework that assigns each a place in which its relative importance can be assessed; (3) the fact that attempts are made to outline a new general theoretical framework to replace the old where it is found descriptively inadequate; and (4) the fact that the new picture of business performance painted is compared with the welfare ideal (a performance picture which is not, in Wiles' view, as bad as that painted by orthodox marginal analysis). These characteristics would seem to make this book required reading for students who have had a rigorous course in micro-analysis and who wish to look carefully at the business enterprise from either a management or a social point of view.

More specifically, this reviewer found particularly stimulating Wiles' examination of the relationships between accounts (and accounting) and firm

behavior, his balanced discussion of the marginal cost principle, and his admittedly preliminary analysis of the determinants of the price-output equilibrium of the firm in the long run when the long-run cost curve for the firm is L-shaped. On the other hand, this reviewer was struck by the absence of any real consideration of the problems of organization as independent determinants of firm action, an inadequate treatment of the problems of oligopoly, and, in most instances, the replacement of one a priori theoretical system—that of marginal analysis—by another. With respect to the latter point, and except for a statistical study of long-run cost curves, Wiles simply reasons from new assumptions about the nature of the firm and/or its environment to new conclusions about its behavior. True, these new assumptions strike the reviewer as being more "realistic" than the old. True, occasional support for their use is found in the statements, or writings, of entrepreneurs and/or analysts of business behavior. But, at bottom, many basic points are subject to the trap which Wiles so correctly sees as having caught the marginalists. "The indicative mood has a fatal attraction: it is so much easier to say 'the entrepreneur does' than 'if my premises hold the entrepreneur would' " (p. 2).

Finally, this excellent book might well be read with profit along with Howard Bowen's slim volume entitled *The Business Enterprise as a Subject for Research* (New York 1953) written for the Committee on Business Enterprise Research of the Social Science Research Council. Bowen's volume defines "business enterprise research" and the "theory of the firm" and suggests research areas likely to prove fruitful. It is clear that Wiles' book is squarely in the area marked out by Bowen, that he is using techniques congenial to the Bowen report, and that he has at least systematically and meaningfully approached a good many of the research areas suggested by Bowen. He does not have all of the answers, but the economist who hopes to find them might well start with Wiles' work in beginning his search.

<div style="text-align: right">CLARK C. BLOOM</div>

State University of Iowa

Income and Wealth, Series VI. Edited by MILTON GILBERT and RICHARD
    STONE. (London: Bowes & Bowes. 1957. Pp. xiv. 306. 42s.)

This volume comprises ten of the twenty-three papers originally given at the Fourth Conference of the International Association for Research in Income and Wealth held in the fall of 1956. The papers, which were presumably selected on the ground that they represent contributions of more general interest and permanent value, fall into two parts. Three papers present or evaluate explorations in model building; the remaining papers deal with several important aspects of the measurement and interpretation of size distributions of income.

Carl F. Christ's paper on econometric models of the U. S. economy is a masterpiece of exposition and condensation. In a few pages, the elements and dynamic properties of what he calls Tinbergen-type models are presented. The balance of the brief paper is built around two tables in which the characteristics of nine of the best-known models are summarized and results of ex-

trapolating four of these models for one or two postwar periods are presented. The Klein-Goldberger model, which Christ finds the best, is subjected to additional tests to ascertain its forecasting qualities.

J. Lips and B. D. J. Schouten examine the reliability of the policy model used by the Central Planning Bureau of the Netherlands. P. J. Bjerve explains a model used to forecast bank liquidity in Norway for 1950 and 1951. The model consists essentially of a double-entry table showing estimated changes in intersector claims and debts of the main sectors. Projections were arrived at in different ways: for some cells no change was assumed except for known changes in contractual payments (since there was not sufficient evidence to expect change in either direction), for some others such down-to-earth methods as making guesses on the basis of interviews with currency-control officials were used, and for still others alternative assumptions were made as to results of fiscal operations and as to policy. After following Bjerve through the intricacies of several sets of tables and equations, the reviewer finds himself in a situation somewhat similar to that of Mr. Jurdin in the *Bourgeois Gentilhomme* who suddenly discovered that he was talking prose: he simply did not know that, by having acquired the habit of analyzing the effects of government fiscal and debt operations, of bank lending and investment operations, and of changes in the holding of assets on the liquidity position of the various sectors of the economy, he was building models.

It is possible to demonstrate, as Christ does by comparing the Klein-Goldberger model with a now famous "naive" model of Friedman (which simply assumes no change from the previous year), the superiority of one model over another for a period of years. However, there do not seem to exist any objective criteria for judging the reliability of a model. This reviewer cannot but agree with Lips and Schouten (p. 27) that it is a matter of subjective judgment whether an error of 100 per cent in forecasting year-to-year changes should be regarded as serious or not. Indeed, the permissible error will in each case depend on the use to which results of projections are to be put. Whether the building of economic models will ever yield more than a discipline for the analyst, forcing him to consider a fairly complex set of interrelations within a formalized framework of past experience, is a question which the limited experience with such models is as yet incapable of answering. The three papers in this volume are more likely than not to tilt the balance in favor of the skeptic.

The seven papers on income-size distribution suggest that the interest in this important field, where the multiple and at times contradictory effects of the economic process and of government policies are molded into one, at times deceivingly simple set of figures, is gradually shifting from measuring to interpreting. Dorothy S. Brady's paper on "Measurement and Interpretation of the Income Distribution in the United States" draws our attention to the fact that the focus of at least one of the three earliest studies on income distribution in the United States was already on the "why?"; the inquiry into the "what" was meant as a stepping stone for exploring what would happen if the *status quo* was changed.

Mrs. Brady's paper is a rich sampler of problems that have arisen in recent years in trying to draw new economic insight from statistical data on size distribution in the United States. In doing so, she emphasizes questions that are particularly relevant in studying and projecting spending and saving.

Mrs. Brady keenly feels the need for a theoretical framework for analyzing size distributions, and identifies among the results and hypotheses suggested by recent studies some possible building blocks for such a framework. Impressed by the relative symmetry of the distribution of income within specific occupational groups, she explores the factors which may affect departure from symmetry in the upper part of the range; notes the stability of relative incomes within a professional group; and raises, on a more general level, the question of the "transient" and "permanent" elements in income. If her statement that "the statistical elegance of present day estimates of the income distribution in the completeness and consistency of the accounting for the total population and the total income during a given year does not guarantee their usefulness in economic analysis and projection" was intended as a warning to her international audience, which might have been overly impressed by our wealth of our income statistics, this reviewer would not know of any wiser advice to give.

The following three papers are largely descriptive, exploring changes in income distribution in Denmark (K. Bjerke), Canada (S. A. Goldberg and J. B. Podoluk) and Australia (mainly in 1942-43, by H. P. Brown). All three bring together material not available in convenient form elsewhere, explore the peculiarities and shortcomings of the original data, describe steps taken to improve their comparability and consistency, and to a varying extent summarize the main characteristics of the distributions discussed. Bjerke focuses on changes in the distribution of income as well as of wealth in Denmark between 1939 and 1952, and the article devoted to Canada does the same for wages and salaries in the longer interval between 1930-31 and 1951. Brown concludes his paper by suggesting a new measure of inequality which he calls an "inferiority index," and which is related to Gini's δ. D. Cole and J. E. G. Utting use data from the 1951 family budget study covering one single county in England (Cambridgeshire) to explore the association between family structure and income. Their explorations on the effect of multiple-earner families on the family size distribution of household incomes, on factors influencing the formation of multiple-earner households, and the life-cycle aspect of household income, even though based on material limited to a narrow geographic area, raise questions of general significance which have as yet been probed into only very superficially in the United States and elsewhere. The remainder of the paper deals with the dependence of size distribution on the unit of measurement (ranging here from individuals to households).

The brief paper by R. Bentzel explores some aspects of the economic interpretation of changes in the inequality of income distribution against the background of the Swedish developments during the last two decades (but without reference to any specific data) and focusing on the effects of changes in the distribution of income on consumption. Using a theoretical model and

assuming plausible values for key parameters he arrives at the conclusion that changes in the structure (as contrasted with the level) of income cannot have had significant effects on the level of consumption.

Noting that "we have a theory of functional distribution running in terms of income shares that can hardly ever be measured, with the consequence that the theory is beyond the reach of empirical verification" (p. 284), O. Aukrist uses Norwegian data since 1930 to investigate cyclical and larger-run changes in the share of wages and salaries in factor income. Aukrist follows Denison's example in narrowing the analysis down to the "ordinary business sector." He finds that export industries account for a greater part of the contracyclical behavior of the ratio of wages to income than home-market industries. Another interesting finding is that interindustrial shifts rather than changes within individual industries account for the bulk of the changes in the wage fraction which, in comparison to 1935-39, has risen in the postwar years for the entire economy, but declined in manufacturing, even though even here interindustry shifts tended to raise this fraction. However, the over-all wage ratio rose because the number of wage earners rose more rapidly than the number of all employed persons, and not because of the relatively greater increase of wages and salaries.

Volumes such as the one reviewed present a mosaic. Their value cannot be judged by whether the mosaic adds up to a picture—it obviously does not—but why should it? The purpose of the conference was to open up new sources of data and to stimulate probing and questioning. Results of such explorations are never lasting: they live on in the stimulus which they give to further research, in the seeds they plant in the minds of those who try to translate administrative data into meaningful economic categories and to answer the question on the essence and implications of change. That there has been change is amply demonstrated by data on size distributions for all countries covered in the present study. Those who reach for this volume will find in it a representative range of technical and analytical problems challenging those working with size distributions of income.

The problems of model building are on a different level: clearly, by using a sufficiently large number of equations a satisfactory model can be constructed to describe almost any web of past relationships. It is the linking of the future with the past that causes all the trouble. Every analyst will have his own specifications in deciding whether a given model is a usable bridge between the two. This reviewer can only repeat Christ's conclusion after reviewing the rich crop of models for the United States: "The reader may make his own appraisal."

GEORGE GARVY

Federal Reserve Bank of New York

L'investissement. By PIERRE DIETERLEN. (Paris: Librairie Marcel Rivière et Cie. 1957. Pp. 362. 1,200 fr.)

This volume is the sixth in a series "Bilans de la connaissance économique" edited by Robert Mossé, designed to provide a survey of contemporary thought

and of its development in various areas of economics. In French usage, *investissement* corresponds approximately to capital formation, the term *placement* being used for the financial transaction by which investment is facilitated. While *placement* is discussed incidentally, it is to *investissement* proper that Dieterlen addresses himself, devoting successive chapters to definitions, determinants, effects, means, bounds, and policies of investment.

Investment provides a rather unique focus for a survey of economic thought, in that while investment so frequently forms a crucial element of a theory, it is seldom accorded an extensive treatment of its own in the elaboration of that theory. Dieterlen has collected his material from a wide area, ranging from theories of secular growth to monetary theories; the main focus, however, has been with the role of investment in the stability of various macroeconomic systems and models. In fitting material from such diverse sources into his scheme, he has considered his assignment one of catholic coverage and the posing of unresolved questions rather than that of attempting to reduce knowledge in the field to an integrated system. Thus what emerges is a montage rather than a neat picture. But in this way divergent currents of thought are brought together in a striking way, and it may be that the greatest service that this volume will render will lie in the stimulation of further thought by these challenging juxtapositions.

Eclecticism in such brief compass is likely, of course, to become cryptic in spots, as when Kalecki is cited as putting profit = investment + consumption out of profits, without indicating either the assumptions behind the equation or the precise meaning assigned to the term profit in this context. Also the limitation of the discussion to investment shows rather badly when stabilization is given categorically as the paramount objective of short-term investment policy, without any consideration of the possibility that monetary or especially tax policy might prove sufficiently powerful to take care of stabilization, leaving investment free for the fulfilment of other objectives.

The usefulness of this volume is greatly enhanced by the 80-page annotated bibliography, which abstracts not only the conclusions, but to a considerable extent the essentials of the argument of some 44 books and articles, as they relate to investment. It is perhaps carping to complain that these summaries are in alphabetical rather than chronological or topical order, so that in reading through them sequentially one frequently comes across the criticism of a thesis before the thesis itself, and must work out the relative position of the various pieces for one's self.

This volume should prove even more useful than the preceding ones in the series, in that it covers a field that has more divergence of thought and in which recent developments have been more luxuriant than in other areas of economics. The student will find in it a counterpoint to other more unified but necessarily more restricted treatments of macroeconomics, and even those already well-versed in the field will find it a useful check-list of ideas and points of view that can all too easily be forgotten in the day-to-day work of analysis.

WILLIAM VICKREY

Columbia University

Nipponkeizai to Keikihendo. (The Japanese Economy and the Business Cycle.)
Edited by HIDEO AOYAMA. Monograph No. 3. (Tokyo: Kyoto University
Research Center of Economic Development and Foreign Trade. 1957.
Pp. viii, 264. ¥ 480.)

The symposium under review represents the first attempt of Japanese
economists to treat empirically the problem of the business cycle in the
concrete context of the Japanese economy. As such, it is a pioneering departure
from the traditional and especially postwar preoccupation of Japanese econ-
omists with abstract model-building in the field of business cycles. The volume
is of special interest to American readers, partly because it reflects the extra-
ordinary influence of the National Bureau approach to business cycles, and
partly because it reveals the significant effect of changes in America's
propensity to import on the cyclical behavior of the Japanese economy.

The business cycle in the Japanese economy is characterized by two
peculiarities, according to the empirical studies made by the contributors to
the symposium. First, in terms of *real* national income the Japanese economy
has experienced less marked cyclical fluctuations than has the American
economy. This statistical fact is interpreted (notably by Hitotsubashi Uni-
versity economists) to mean that the Japanese business cycle expresses itself
mainly in price fluctuations and that the Japanese national income deflated
by the price index approximates Harrod's "steady growth" without cyclical
fluctuations. The Kyoto University economists participating in the sym-
posium, on the other hand, seem to feel that Japan's "aggregate economic
activity" in the National Bureau sense (of including financial and trade in-
dices) and in real terms definitely exhibits "cyclical growth" of the Schumpeter-
Goodwin variety. Both interpretations seem to neglect the historical role of
the government sector in reducing the structural instability of the Japanese
economy via an increasing combination of paternalistic subsidies to private
enterprise, the nationalization of public utilities and other select industries,
and the provision of welfare-statist "built-in" stabilizers. Both interpretations
are sidetracked by the statistical issues of "real" vs. "monetary" indices and
"aggregate economic activity" vs. "gross national income" estimates. ·

Second, the symposium reveals that the cyclical behavior of the Japanese
economy is covariant with that of the world's effective demand. Noteworthy,
in this regard, is the finding that the ratio of the Japanese activity index to
the American activity index has covaried with the index of Japan's trade
balance with the United States, thus indicating the extent to which Japan's
domestic prosperity and depression are affected by fluctuations in the United
States. Japan is not alone in entertaining misgivings about the cyclical insta-
bility of the American economy, if one may judge from the apprehensions
expressed by the economists of other trading nations in *The Business Cycle in
the Post-War World,* E. Lundberg, ed. (New York 1955). The symposium
shows that the vulnerability of the Japanese economy to the dictates of
international economic fluctuation has increased considerably since the end
of the second world war. Japanese economists, therefore, are understandably
interested in the quantitative studies of the international income-price elasti-
cities of demand for imports, comparative marginal propensities to import,

and the international balances of payments. It is a pity that the symposium does not discuss the relation between foreign trade and economic development as well.

Valuable though the above findings are, the volume under review suffers from the following weaknesses. First, there are too many mechanical applications of the National Bureau technique to the Japanese business cycle without a critical examination of the *conceptual* (in contradistinction to statistical) issues involved in "reference cycles," "reference dates," "aggregate economic activity," "leads and lags," and "peaks and troughs." Second, the familiar criticisms of the National Bureau approach (notably by Koopmans, Hansen, and Metzler) remain unanswered, with the possible result that the contributors to the symposium could be misunderstood as preferring "the jungle of facts and figures" to "the dreamland of equilibrium." The Mitchell-Burns argument that the range of fact-finding should not be restricted by any particular hypothesis is not posited with the counterargument that bold and imaginative hypotheses are impossible to make so long as economists are narrowly confined to facts and figures. Lastly, there is in the symposium an unwarranted aversion to the theoretical approach to the business cycle, for after all the synthesis of theoretical hypotheses and empirical investigations is the *raison d'être* of econometrics. In sum, fact-finding must be coupled with hypothesis-testing and parameter-fitting if business-cycle research is to be fruitful for both analysis and policy.

KENNETH K. KURIHARA

Rutgers University

Gleichgewicht und Konjunkturtheorie. By KLAUS ERICH RHODE. (Stuttgart: Gustav Fischer. 1957. Pp. xii, 236. DM 18.00.)

The relationship between equilibrium analysis and business-cycle theory has been a controversial issue in German theoretical literature. The discussion began in 1926 with the significant contribution of A. Lowe with regard to whether a systematic theory of the cycle is possible. In the debate, Spiethoff rejected any form of equilibrium analysis as incompatible with cycle theory. F. A. Lutz denied the possibility of recurrent cycles as well as of a systematic business-cycle theory. Subsequent work built largely upon Lowe's thesis that a theory of the cycle is possible only if statics is replaced by dynamics.

Accepting Frisch's definition of dynamic equilibrium, the author seeks to discover the significance of the evolution from statics to dynamics for our understanding of the cycle. In addition, he compares the typical factual pattern with the various theories of the cycle to ascertain which theories are compatible with the typical Juglar cycle.

The aim of the first task is to replace the present "pluralistic meanings" of the equilibrium concept by an integrated typology of equilibrium theories. This typology should exhibit three characteristics: There should be a logical continuity of the equilibria from the individual units to the whole economy. Continuity should be followed by a declining degree of abstraction in the sequence of models. Finally, dynamic equilibrium theory should provide the "analytical instruments necessary for explaining the cycle."

Logical continuity calls for overcoming the dichotomy between micro- and macro-analysis. Required is a microeconomic total model, which starts from the plans of individual units, proceeds to the equilibria in markets, and ends with a total equilibrium for the economy. Such a model is preferable because it is built upon the motives of the individual units. Yet the micro-total model pertains only to one moment of time. A change of the magnitudes over time is required for an explanation of the business cycle. The resultant difficulties are not resolved and the effort to replace macro-theory is given up.

A distinction between "abstract" and "concrete" equilibrium theories is introduced. Neither term is defined. We find short summaries of the identity of saving and investment, of the acceleration principle, and of so-called "core processes." A random comparison of the strategic relations in different theories is supposed to show, via a diminishing degree of abstraction, "the meaningful connection between the various forms of the idea of equilibrium and the principle instruments of business cycle analysis" (p. 114).

A similar discernment prevails in the discussion of static and dynamic models. The thesis that cycle theory is not possible within the field of static theory is strongly underlined. Yet it is not clear whether the static model is one of full or less-than-full employment. Nor is the relationship between cyclical and developmental models examined. In refraining from analyzing substantive problems, the author deprived himself of the opportunity to achieve a "synthesis of the types of equilibrium analyses."

The comparison between the factual pattern and theories of the cycle is handled with much more understanding. Four questions are considered: How do selected theories explain: (a) the causes of the "first" upswing, (b) why the upswing is a cumulative process, (c) why there must be a downswing, and (d) why cycles recur? The theories of Wicksell, Schumpeter and Spiethoff as well as of Halm, Haberler and Jöhr, are examined for their compatibility with the typical factual cycle. Keynesian theory is not included. Examination of the theories mentioned leads the author to two conclusions: He accepts Spiethoff's position that even the "first" upswing should not be deduced from an equilibrium position of full employment. Such a static beginning forces us to disregard the idle resources of the depression as a cause for the upswing, and permits us only to examine the growth element (known as overproduction) of the first cycle. Yet the author insists that Spiethoff was wrong when he asserted an inescapable conflict between equilibrium analysis and cycle theory. Both become compatible when—following Schumpeter— the upswing is explained out of the disequilibrium of the depression. Compatibility is achieved because the equilibrium point is then located in the middle of the cycle, deviations above or below constituting situations of over- or underemployment.

Yet this oscillation theory does not consider two other objections of Spiethoff to dynamic cycle theory. His objections deal with the impossibility of transforming disequilibria into a cumulative process, and of deriving numerical from equilibrium values. In not examining the oscillations substantively, the author cannot disprove Spiethoff's position on the cumulative process. Yet there is an indirect effort to disprove Spiethoff's assertion on equilibrium

values. Schumpeter's characterization of equilibrium as a "point of reference" is translated into German as a *Massstab*, which standard presumably enables us to measure the distance between equilibrium and disequilibrium points. Of course, Schumpeter's figure of speech did not imply numerical values. The author's thesis that Spiethoff's critique did apply to only static and not to dynamic theory, and that the latter is fully compatible with Spiethoff's own theory, is not convincing to this reviewer.

ARTHUR SCHWEITZER

Indiana University

Veblenism, A New Critique. By LEV E. DOBRIANSKY. (Washington: Public Affairs Press. 1957. Pp. xii, 409. $6.00.)

Professor Lev E. Dobriansky of Georgetown University administers to Veblen the same type of medicine that Veblen used on his victims—to lay bare their fundamental philosophical preconceptions. In pursuit of this task Dobriansky reveals his own preconceptions. He has written a critique of Veblen in terms of the metaphysics of *Philosophia Perennis*, a body of philosophical doctrine compounded during the middle ages from the ideas of many doctors among whom St. Augustine and St. Thomas were the leading architects. *Philosophia Perennis* is characterized as striking a happy balance between operationalism and essentialism, becoming and being, synthesis and analysis, empiricism and rationalism, abstraction and concreteness, theory and practice, dynamics and statics, organicism and individualism.

Within the framework of his philosophical approach, Dobriansky sets three objectives: (1) to present Veblen's ideas as a systematic body of thought; (2) to make a contribution to the integration of the social sciences with the aid of systematic Veblenism; and (3) to shed light on the practical alternative to "totalitarian-bound socialism," on the one hand, and "an abstractionist reversion to the socially disintegrative tendencies of laissez-faire capitalism," on the other hand. The first objective is more fully realized than the other two. After a brief survey of Veblen the man, Dobriansky takes up in successive chapters medieval philosophy, modern philosophy, Veblen's concept of science, his evolutionary economic science, his cultural analysis, institutional economic theory, and Veblenian managerialism.

Dobriansky is a trained philosopher as well as an economist, and his chapters on medieval and modern philosophy are likely to overwhelm the unphilosophical economist. Relatively few who read these chapters with understanding will accept the author's view that modern philosophy from Descartes to Dewey has been a frightful and unnecessary muddle which could have been avoided if *Philosophia Perennis* had not been ignored or rejected. The author does not face up to the reality that most modern philosophers, scientists and economists reject the metaphysical absolutes of medieval philosophy. Whatever the intrinsic merits of *Philosophia Perennis*, Dobriansky's two chapters on philosophy, constituting approximately one-fourth of the entire volume, may well discourage most economists from venturing further into the book.

Veblen's philosophical position is labeled "radical empiricism." According to Dobriansky, this antirationalistic epistemology caused Veblen to over-emphasize process and becoming to the neglect of essence and being. Veblen's evolutionary science is said to assume naively that discovery of the origins of institutions is a substitute for an analysis of their nature (being). In order to surmount some of the obvious shortcomings of a one-sided empiri-cism, we are told that Veblen surreptitiously slips in some rationalistic ele-ments. One is his use of instincts, which however turn out to be moralistic precepts in the guise of scientific categories. While Veblen paid lip-service to the unity of science, his defective method precluded genuine progress toward that goal.

Although Dobriansky thinks Veblen's economics leaves much to be desired, he rates it as a landmark in the history of economic thought. Veblen's cultural analysis and institutional theory represent significant contributions to what economics should be. Dobriansky's ideal appears to be some sort of combina-tion of a Veblen-type evolutionary economics and Lionel Robbins means-end principles. In place of "economics" or "political economy," Dobriansky pre-fers the term "social economy," which he defines as "a philosophical science which analyzes and interprets institutionalized human behavior as a relation-ship between ends and scarce means which have alternative uses" (p. 215). Veblen's holistic conception of society, organically related in all its parts, yields valuable insights into the conditions of the good society, which the author refers to in passing as "an organically integrated pluralistic society" (p. ix).

On policy issues Dobriansky contends Veblen was unreceptive to com-munism, socialism, and New Deal-type interventionism. He asserts that nationalization was for Veblen just another form of absentee ownership. Since Veblen viewed government as a weapon of the vested interests, he mistrusted it as guardian of the rights of the common man. Veblen's program is described as technocratic managerialism under political anarchy: a blend of James Burnham's managerialism, technocracy, and guild socialism. Veblen's program is characterized as utopian; any attempt to adopt his managerialism would spell disaster for democratic institutions and also fail to realize the material abundance which Veblen's engineers were supposed to bring to the underlying population after elimination of the price system. While Dobriansky's inter-pretations of Veblen's views on policy merit careful consideration, they are by no means simple deductions from Veblen's writings, because on most of these issues Veblen is not clear. Other interpretations are possible.

Despite a penchant for polysyllabic words, complex sentences, and philos-ophical jargon, Dobriansky has written a significant and scholarly book. Within the framework of his own metaphysical preconceptions and in rela-tion to the goal he sets for himself, he achieves a high degree of success. One may wish that he had employed a different framework, but such a judgment must be conditioned by the reminder that in metaphysics everyone is entitled to his own illusions.

<div style="text-align: right">DUDLEY DILLARD</div>

University of Maryland

Economic History; Economic Development; National Economies

Economic Analysis and Policy in Underdeveloped Countries. By P. T. BAUER.
(Durham:, Duke University Press. London: Cambridge University
Press. 1957. Pp. xviii, 145. $3.00.)

This volume contains a series of lectures delivered by the author at the
Commonwealth-Studies Center of Duke University. The first lecture deals
with the scope, method and limitations of economics in the study of under-
developed countries, and the effects of the increased interest in underdeveloped
economies on the development of economic theory and policy. The second
lecture considers features and developments characteristic of underdeveloped
economies, while the third examines problems of policy relevant to a large
number of underdeveloped countries. These studies raise few new issues;
their chief contribution lies in drawing attention to matters which have been
handled inadequately in the literature and practice relating to underdeveloped
economies.

However, in some cases the author comes close to a kind of extreme oppo-
site to that against which he warns. For instance, he relies too heavily on
direct observation: while the occupational statistics show trade to be a
relatively "insignificant occupation" in West Africa, Bauer observed a
"large volume of trading activity" absorbing "a large volume of resources,"
and was thereby led "to doubt the empirical and analytical bases of some
widely accepted propositions about the relation between economic progress
and occupational distribution" (pp. 13-14, 67-69). But quantification, sta-
tistics and, mainly, economic principles provide an indispensable check to
direct observation. Compartmentalization, and limited specialization and trade
are fundamental characteristics of underdeveloped economies, noted by Bauer
in other contexts (pp. xii, 26, 50, 58, 61-62, 69). In addition, production in
agriculture and industry requires a relatively large volume of resources,
partly because it is conducted on a small scale (see p. 67). Hence, propor-
tionately fewer resources are employed in trade than in more advanced coun-
tries. It is unfortunate that Bauer allowed direct observation to override
these basic propositions (pp. 13-14, 26-27, 67-9).

In examining Nurkse's international demonstration effect Bauer overem-
phasizes advantageous imitation: he concludes that contact with more ad-
vanced countries "almost invariably accelerates economic growth" and states
that "this indeed is a commonplace of economic history" (pp. 26, 65). But
the tendency to imitate is a basic determinant of all kinds of economic, politi-
cal and social behavior: for instance, it is not unusual for workers in under-
developed countries to bargain for wages and other benefits comparable to
those in advanced countries in spite of their much lower productivity, and
for governments, public servants and capital-owners to "adopt technical, edu-
cational and social standards which are inappropriate and wasteful" (p. 66).
It is not immediately obvious, nor has economic history or direct observa-
tion proved, that international demonstration may not, in the net, have
impeded the economic development of some countries, just as it may have

promoted the development of others. Again, it does not seem "obviously inconsistent" to believe, as the "distinguished Indian" criticized by Bauer does, that, while "the great majority of Indians . . . [have] a strong leaning towards asceticism," "higher incomes would attract luxury or semi-luxury imports" and lead to "the construction of cinemas, the manufacture of soft drinks," and similar uses of capital (p. 17).

, The damage from direct observation which is not complete (much of it can not be) or which is extrapolated unduly in forecasting future developments tends to have multiplier effects: since such observations and conclusions are presented vividly and come "from the horse's mouth" they may be accepted readily by less critical scholars even though they clash with established principles of economics and logic.

In other parts of his analysis Bauer seems to neglect factors which are fundamental in the discussion of economic development, for instance, the significance and impact of different rates of change in the study of response to economic incentives. Yet consumption patterns, saving, business behavior and economic welfare in many underdeveloped countries may be different from those in the West because the former countries are stagnant rather than underdeveloped, or because the rates and patterns of change in incomes, leisure or prices are different. For instance, the rate of change, rather than the level, of wages may help in explaining cases of a backward-rising supply curve of work; restrictive economic measures are probably just as much a function of the rate and pattern of economic development as they are of the degree of specialization of the economy (p. 77); and many underdeveloped countries may have a low saving-income ratio because they are stagnant rather than poor. The last proposition rather than the "substantial capital formation in agriculture," would solve the riddle: "it is difficult to see how the developed countries could have reached their present position since all developed countries began by being underdeveloped" (p. 63). The distinction between underdevelopment and stagnation and the features and problems relating to the two conditions has been neglected badly in most of the literature on underdeveloped economies; yet if such countries advance rapidly they may for a time share the former features while shedding the latter.

<div align="right">S. G. TRIANTIS</div>

University of Toronto

Management of Direct Investments in Less Developed Countries. A Report submitted to the International Bank for Reconstruction and Development. By the Foundation for Economic Research of the University of Amsterdam. (Leiden: H. E. Stenfert Kroese. 1957. Pp. 238. ƒ 16.50.)

The title of this work may be somewhat misleading. Readers may well expect it to be a study of the experience of management of direct foreign investments in underdeveloped countries, based on data obtained in a number of representative countries in this category, sufficient to give it universal value. This seems, implicitly, to be claimed for it. As a report to the International Bank it does not offer practical conclusions on which loan policy

may be based, nor is it discernibly theoretical, although theories of Frank H. Knight and Joseph A. Schumpeter were chosen to constitute its nonpractical framework. It is mainly of an advisory character.

The study is limited to Dutch, French and Belgian foreign investments in underdeveloped countries and to foreign enterprises in the Netherlands. J. F. Haccoû, economist in the University of Amsterdam, who is the author of the work, rejects cultural, sociological and economic criteria for determining. stages of development comparatively. In a sustained discussion of prerequisites, characteristics and conditions, which is the main part of the book, he examines aspects of such investments which are in general common to the areas studied, although with important differences of degree.

For the purposes of this report conduciveness to successful foreign investment is made the essential criterion for development. But this makes the effort to eliminate the cultural and economic criteria hardly worth while, for prerequisites that may be essential for, and conditions that may be conducive to, development cannot be satisfactorily appraised without reference to culture, duly defined, and to economic conditions.

The aspect which has received most emphasis is "homogeneity" of purpose of all managerial personnel of the metropolitan entrepreneur. The purpose is, of course, profit. The prospective gain must be large enough to warrant incurring every conceivable risk. So calculability and virtual certainty of return are essential conditions for combination of factors into a "dynamic entity" for maximum producitivity, while profit must be maximized. Haccoû asks for complete cooperation, to this end, in the countries where the investments are made. He differs from the relevant part of Knight's theory which he evidently chose as a basis for the discussion of profit. While Knight makes profit or loss depend on the "rashness or timidity of entrepreneurs as a class" rather than on "abundance or scarcity of mere ability to manage business successfully,"[1] Haccoû discusses profits as the result of managerial functions. His concern about minimum risk and maximum profit seems undue. The entrepreneur cannot assume that profit and risk are mutually dependent, but it is common business experience that rates of return vary directly, not inversely, with risk.

The location of the top-management of a firm investing in an underdeveloped country, elsewhere than in that country, was found to be frequently the case in all areas studied, but we cannot conclude, from the evidence offered, that production costs are usually significantly higher because of separation; and it is difficult to accept Haccoû's generalization that spatial separation of the entrepreneur from plant management will cause resentment in the underdeveloped country. Absentee ownership is resented in many underdeveloped countries; but the actual presence of entrepreneurs in such countries could not preclude this result. Usually the objections are two: (1) the low rates of payment for field and factory work, and (2) the unlikelihood that a sub-

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[1] See *Risk, Uncertainty and Profit* (Boston 1921, reprinted by the London School of Economics and Political Science, University of London, 1946, p. 283.

stantial part of dividends and undistributed profits will be reinvested in the underdeveloped country.

At the core of the book Haccoû makes direct foreign investment on a "Western large scale" depend on technical possibilities in the country. He conceives of development as typically starting with an economy dominated by import activities, and he allows for the possibility of expansion of investment as technical preparedness increases. But his "dynamic entities" are to achieve stability, presumably in the short run, and thereafter to remain static for long periods during which the foreign investors are to enjoy calculability of returns. This would preclude sustained speedy development in which structures for production must change.

It is admitted that governments of underdeveloped countries have a role as investors, but investment in basic assets such as roads and harbors is preferred for them, as prerequisites for private investments, and it is recommended that they guarantee any of these investments which are made by private enterprise.

New or increased direct foreign investments and foreign initiative would undoubtedly contribute largely to economic growth in underdeveloped countries, but it does not appear that many of these countries are in a position to provide a substantial part of the capital needed, from taxes and tariffs, for construction of basic assets, as Haccoû suggests, or that direct foreign investment and initiative would suffice, even if the governments did substantially increase expenditures for these purposes out of current revenues.

The recent recommendations made by the United Nations' experts, the Millikan-Rostow proposal, and the scale of the Soviet Union's aid for these purposes seem to be evidence enough of the magnitude of the capital investments necessary for basic equipment.[2] The United Nations' experts' recommendation of $10 billion per annum is a minimum, and the report does not show that needs of capital in the Belgian Congo, French Equatorial Africa or Indonesia, as a part of total needs in underdeveloped countries, are proportionately less.

A narrow sense of nationalism in countries sensitively conscious of newly won independence is repeatedly mentioned as a deterrent to foreign investments. This is undoubtedly true in some cases. Nevertheless cognizance has to be taken of the onerous task of urgently needed development in former colonies. Haccoû's belief in inevitability of extreme gradualness of economic growth in these countries seems too pessimistic. The progress of projects of the TVA type in India does not support this conclusion.

[2] The United Nations' experts recommended additional annual capital of $10 billion from industrial countries, for a 2 per cent increase in per capita income in underdeveloped countries. See Gunnar Myrdal, *An International Economy* (New York 1956), p. 123. The Rostow-Millikan proposal was that Congress supply $10-$12 billion in capital and assistance. And John Foster Dulles testified to the Foreign Relations Committee on the Mutual Security Act of 1956 that the Soviet Union's economic and technical assistance to free countries was approximately $600 million in 1956. See The American Assembly, *International Stability and Progress* (Graduate School of Business, Columbia University, May 1957), pp. 86, 91.

The colonial power usually leaves a very inadequate basic structure, since the desire for complementarity rather than for autonomous development guided its policy. The greater the deficiency the greater must be the responsibility of the new government for displacement of the "equilibrium state previously existing,"[3] and the more urgent the need of increase in the national income. Consequently, the greater must be this government's concern about disturbances caused by foreign investors whose plans could distort its own design for growth.

WILLIAM E. GORDON

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[3] J. A. Schumpeter, *The Theory of Economic Development* (Cambridge 1951), p. 64.

Contribuições à análise do desenvolvimento econômico. (Rio de Janeiro: Liv. Agir. 1957. Pp. 354. Cr$ 300,00.)

This is a collection of essays, written in homage to Professor Eugenio Gudin, by a great variety of authors, some American, some English, some French, and some Brazilian. Those who might shy away because the title is in Portuguese should be reassured; the essays are in the usual tongues of the writers, so that about a third of the book is in English, about a third in French, and about a third in Portuguese. The non-Brazilian authors are drawn mostly from those fortunate souls who have had the privilege of visiting Brazil to lecture or for some other good purpose. Seldom, I think, can a *Festschrift* have been written with such uniform feelings of warmth and affection for its guest of honor; Gudin is regarded with deep respect and affection by all who know him, and while one cannot begrudge his country the long and valuable service which he has given it, one regrets also the linguistic barrier which to some extent isolates Brazil and prevents her great men from receiving the world attention which they often deserve.

The articles reflect in a curious degree the present condition and interests of economics in the three main national groups from which the authors are drawn. The Americans mainly write what might be called notes on applied theoretical economics; the theoretical frame of reference is pretty much taken for granted, and the interesting problems are seen to be those of its application, especially, of course, to countries like Brazil. Thus Bernstein writes on tariff protection and economic development, Brozen on industrialization, Arthur E. Burns on some limitations of economic assistance, Haberler and this reviewer on inflation, Nurkse on the problem of fluctuations in the export of primary products, Singer on gains from trade, Viner on disguised unemployment. The French authors—notably Leon Buquet, Maurice Byé and Daniel Villey, write long, often rather elementary, theoretical articles, as if they were still in the middle of the theoretical excitement which prevailed in English-speaking economics a generation ago, as no doubt they are. The Brazilians combine both interests: Bulhões on external economies, Furtada on the marginal analysis and Sampaio on the equation of exchange are on the theoretical, or French pattern, Kafka, on the capacity of absorption of foreign capital, Borges and Loeb and Martins on problems of the labor

force, and Roderigues on Brazilian fiscal policy, are on the applied, or American pattern. The lone Englishman, Lionel Robbins, contributes a Note on the Formal Content of the Traditional Theory of International Trade.

One does not expect high uniform quality from the pieces in a *Festschrift*. The present volume however has a unity and a sustained interest which is rare in such collections. And it reflects in an interesting manner the present position of Brazil not only as a country with an economic history and problems of unusual interest, but also as a place where an attempt is being made to synthesize the Anglo-Saxon and the French contributions to life and thought.

KENNETH E. BOULDING

University of Michigan

Preliminary Report—December 1956. By Royal Commission on Canada's Economic Prospects. (Ottawa: E. Cloutier. 1956. Pp. 142.)

"The Gordon Report," whose preliminary draft we here review, constitutes one of the most ambitious efforts at political self-assessment and economic forecasting in economic history. The initial volume has been followed by a dozen or so subordinate ones, and others are in progress. At the end the Commission will undertake a final reassessment. We will not attempt at this time to treat the various specific subordinate reports nor the statistical forecasts contained in the preliminary one. All show a high degree of technical skill and responsibility. Doubtless there may be flaws on the technical side— for who is infallible? But all of the issues to date bear the unmistakable marks of care and competence. The problems which concern me lie on quite another level.

Putting the matter in a nutshell, all this careful technical work tends to take more or less for granted one enormous *ceteris paribus* assumption. That assumption is that the institutional and cultural organization of Canada will continue to show the same dynamism in the future that it has shown in the past. But since the report itself has certain implications or undertones which, in my opinion, seriously call in question this very point, it does not seem out of place to consider in this review some of these cultural-institutional problems.

My task in reviewing, as an American teaching in Canada, has been made easier by the previous review in the *Queen's Quarterly* by Jacob Viner, a native Canadian teaching in the United States. But I find myself a bit more sympathetic than Viner. For though I was born at the other end of the North American continent the problems of the U. S. South and those of Canada have a recognizable resemblance. There is the same desire for economic growth coupled with the same fear of cultural domination. It seems to me quite natural that Canadians should want to see the great part of their wealth controlled by Canadian hands. The question is how high a price are they willing to pay? And what are the best means for ensuring the desired relative independence?

Let us first deal with a subordinate point. Since Canada is relatively a wealthy nation, it seems also reasonable to me for Canadians to want to encourage a diversified, light, consumer-goods industrial development at home.

Primary products are notoriously unstable; and it is not unreasonable to take moderate means for making the home market somewhat less dependent. Canada is growing and the infant industry argument is particularly applicable. But with virtually all economic theory behind me, I submit that it would be far cheaper and more desirable to do the job by subsidies than by tariffs. Here the preliminary report offers (p. 68) a meritorious suggestion. Let light industry which it is desired to sponsor, have a quicker rate of write-off on its plant. Personally, it seems to me, still more direct subsidy might be permissible.

But we come now to the more fundamental question—that of foreign capital investment in Canada and foreign control. The basic problem here is that people who risk their money in new investment naturally want to control the way that money is spent. Without control they are unwilling to take risks. On the other hand the receiving nation wants the investment but resents the control. All these tensions can be found stretching through United States history from Jamestown 1607 to 1914. During that entire period the American colonies and the later nation were net debtors to Europe on capital account. The preliminary report here distinguishes between the U. S. experience and Canada's by saying that foreign investment in the United States was in debts not equities. I am not sufficiently a historian to check this, but I have an idea research might show the distinction considerably overdrawn.

What really interests and disquiets me, however, are certain singular omissions in the remedies suggested by the report. Granted the need, in general, of more capital in Canada, would it not be advisable instead of grumbling so much about the Americans and other foreigners to try to build up the Canadians? To be more specific, why not change the Canadian tax laws to foster a higher average rate of *Canadian* saving and capital accumulation? Putting it bluntly I would say that a capital-hungry country like Canada ought to have a considerably less steep rate of income tax progression and also of inheritance taxation. The (so far) brilliantly successful West German income tax rates might be in point here. But the preliminary report says nothing of any such idea.

Again if it is desired to have more Canadian-managed and -owned businesses and more Canadian executives in foreign-owned business, why not do something to increase the extent and level of Canadian business education? The report does not mention this idea either.

Still more extraordinary is the manner in which the report, while questioning the extent of foreign investment control, goes out of its way to exonerate (p. 93) foreign union influence. I should have thought that the effects of a wage policy more interested in protecting entrenched foreign wage groups than in developing Canada, would have been at the very least, to put it mildly, quite as distasteful to any Canadian interested in his country's development. But, indeed, the preliminary report seems to me to have a certain (albeit doubtless unconscious) labor-controllist bias. The "soft" policy toward cartels, perhaps adumbrated on page 69, may be a case in point; though to be sure the railroad unions do receive a suave chiding on page 78.

I feel sure that an honest expression of opinion, however strange, will not be resented by those who so strongly believe in tolerance and free speech.

And so I am obliged to submit that the greatest danger I see to Canadian growth and the realization of all the glowing forecasts contained in this report is not U. S. investment penetration, but the ideological or cultural penetration of that European and English left-wing labor-socialist philosophy which has shown itself so decisively inferior in the matter of economic growth. Suppose Canada increasingly penalizes Canadian capital accumulation and income incentive, permits more and more irresponsible clamor for money-wage increases, regardless of productivity or employment, sees an increased fostering of the quack idea that economic growth and job security are capable of 100 per cent reconciliation, finds its youth increasingly taught that the desire for achievement and a career are neurotic, silly and immoral, and, on the other side, allows its capitalism to cease to be an enterprise one of *independent* policy and helps it become a mere cosy coalition of vested interests under a "mixed economy" slogan—suppose all these things, the inevitable fruits of the European left outlook, what then will happen to the rosy predictions of the Gordon Report?

Such, according to my misguided brain, are the real dangers to Canadian (and Canadian-owned) expansion. And it seems to me not impossible that the fear of "foreign investment" could turn out to have been the most effective means of ending not just foreign enterprise but also that vigorous home enterprise from which Canada in the past has so signally benefited.

<div align="right">DAVID McCORD WRIGHT</div>

McGill University

The Chinese Economy. By SOLOMON ADLER. (New York: Monthly Review Press. 1957. Pp. xi, 276. $5.00.)

To say that the volume under review is pro-Communist and anti-Nationalist is not entirely accurate. While condemning the previous regime at every opportunity, it goes to such length in praising and justifying every step or policy of the new regime that it sounds as if Peiping speaks. In an important sense, that is true, for the author relies almost entirely on English-language materials officially released by the Chinese Communist authorities, and accepts them without question. Furthermore, the volume is essentially a restatement of the first five-year plan, with enthusiastic running comments and with a review of progress up to the middle of 1956. Therefore, it stands far apart, for example, from W. W. Rostow's *Prospects for Communist China* (New York 1954), which, though outdated in its quantitative data, shows proper restraint and balanced judgment regarding the historical development. With a volume of this nature, it would be futile to argue about matters of interpretation. This review will be confined to comments on the data used.

The first fifty pages are introductory, dealing with the pre-1949 background, progress up to 1952, and the sectorial composition of the economy. The rest of the volume is devoted to the first five-year plan, following very much the same order of treatment, with two exceptions, as in the original Chinese version—planning, industry, agriculture, transportation and commerce, and living conditions. The exceptions are one chapter on finance and another on foreign trade, topics that are not treated as such in the plan.

The statistical data used are chiefly derived from the annual communiqués of the State Statistical Bureau and the reports of high government officials—documents available in English. Although noting that the figures are not always reconcilable "perhaps because the coverage is not identical and because of revisions of preliminary data for the earlier years" (p. 81n), the author does not fully appreciate the significance of this fact. If the data for the earlier years are revised, it is important to make use of only the revised figures. The State Statistical Bureau, which was established in late 1952, took more than a year to set up something like a national statistical reporting system. Since then, the quality and coverage of statistical work has clearly improved and an effort has been made to correct (practically always upwards) some of the data for the earlier years, particularly for 1952 through 1954. In view of the lack of a national reporting apparatus before 1954, it is questionable if sufficient revision of the figures for 1952 and especially for 1949 can be made so as to make them comparable with those of the later years. That being the case, what conclusions can legitimately be drawn by comparing the figures, say, for 1955 with those of 1949? As is well known, the statistical work in the 1930's was very inadequate. What meaning then may be given to conclusions based on comparison of data for 1952 or 1955 with those for the prewar years, as the author repeatedly does in this volume?

Using data available only in English has serious limitations. A great deal of quantitative and qualitative information is found in the periodical literature published in Chinese, such as newspapers, magazines and academic journals. The results of national surveys have been released from time to time. Failure to get access to these sources, plus uncritical acceptance of what is available to him, has led the author to many erroneous observations of fact. One example will suffice. In the chapter on finance, a field in which the author professionally has been an expert, it is stated that the note issue has not been used to finance budgetary deficits and that even in 1950 (the year the Korean war started) bond sales had more than covered the gap between current revenue and current expenditure. As a matter of fact, a gap of 289 million yuan had to be covered by note issue in that year—and the information was published in January 1955. The author stresses the soundness of Communist China's national finances by referring to the large annual budgetary "carry-over" up through 1955. But as repeatedly pointed out by the Department of Budget in Peiping since the middle of 1955, the so-called surplus had invariably been *currently* used for credit expansion by the People's Bank. Budgetary spending of the carry-overs would therefore result inevitably in an increase in note issue, as the realized budget for 1956 testified. Only an uncritical acceptance of the official data without an attempt to inquire into their meaning from Chinese sources has led the author to reach the unwarranted conclusion.

This volume frankly "attempts, however inadequately, to fill the gap" (p. viii) left by R. H. Tawney's *Land and Labour in China* (New York 1932) which is now out of date. But the reason why Tawney's book has long been considered a classic is found in his firm grasp of the factual situation from various points of view, in his command of historical perspective, and in his un-

biased and well-balanced judgment. It will take quite a different volume from the present one under review to take its place.

<div align="right">Choh-Ming Li</div>

University of California, Berkeley

The Economics of Communist Eastern Europe. By NICHOLAS SPULBER. (New York: Technology Press, Massachusetts Institute of Technology and John Wiley & Sons. 1957. Pp. xxviii, 525. $12.50.)

Dr. Spulber's useful summary of postwar development in Bulgaria, Czechoslovakia, Hungary, Poland, Rumania and Yugoslavia[1] has three objectives: the encyclopedic task of describing what is known qualitatively about the area; the statistical task of assembling and interpreting fragmentary data prepared in ways often not acceptable to Western statisticians; and the analytical task of making the other two operations intelligible to the reader.

Spulber has admirably succeeded in the first task, presenting his data in such a way as to facilitate international comparisons, yet emphasizing the dangers of too glib generalizations, particularly with regard to timing of events. In particular, his treatment of Soviet assets in Eastern Europe (Ch. 6) is to be commended for bringing together much scattered data. As statistician, he has assembled the most important data for the years 1938, 1948, 1953, and 1955, together with plans for 1960; and has made numerous interperiod comparisons. The reader not wishing to wade through the entire East European Handbook Series (of the Mid-European Studies Center, to which Spulber is an important contributor) will certainly gain from the synthesis. The specialist might cavil at the summary treatment given to industrial financial controls (*Khozraschet*), materials balances, and controls over the flow of funds between city and countryside through wage and retail-trade operations (these are only institutionally described), but these may fall outside Spulber's analytical framework.

Analytically, Spulber considers that the focal issues in Eastern Europe (except in some cases for Yugoslavia) have been: (1) concentration of investment in processing facilities in industry, to the detriment of raw materials output, especially agricultural output; (2) suppression, on doctrinaire Marxist grounds, of private agriculture; (3) establishment of a foreign trade system which is an odd mixture of autarky, planning, bilateralism and Soviet colonialism. He views the general objectives of the governments as given or else changing slowly; and, although economic policies do change, they respond only slowly to the march of events. Thus after Stalin's death, there was the semblance of a new policy; but Spulber considers this a delayed response to internal pressures which had long been building up. Likewise he considers the return to more "Stalinist" policies in 1955-56 only the overt expression of what had always been the intent of the governments in these countries (and/or their Soviet masters).

[1] There are passing references to Albania, neglect of which is justified on the principle *de minimis;* the scant treatment of East Germany is more difficult to explain, since (despite vague hopes of German unification) this area is unfortunately part of the Soviet orbit. Yugoslavia has followed a course of development often differing sharply from that of the other countries. Thus, some confusion occasionally arises from his selection of countries.

This view regards the development plans (first about 1948; and again about 1955) as expressions of long-term intentions, and analyzes them in some detail (Ch. 8-9). If this conception of them is justifiable, then changes in policy in the interim can be regarded as incidental, and as adaptation to circumstance. There is, however, a basic difficulty in seeing such a plan as "a definite set of tasks which *must be* accomplished and which *are binding* as a law for society as a whole" (p. 282, italics added) if the statement that "the product mix obtained at the end of any plan period does not bear a close resemblance to the product mix scheduled" (p. 344) is correct. If the product mix differs, either the plan has changed, or it has been impossible to attain (*a fortiori*, not binding), or both. Actually, Spulber's evidence shows that plans were changed in 1950-51 (as a result, perhaps, of the outbreak of the Korean war); in 1953-54; and again in 1955-56. Were these changes in binding plans due solely to the pressure of events (pp. 354-56)?

Assuming, then, that there is something in a communist economy which may force the government to do something it does not wish to, the task is to locate what it is. Spulber shows us some obstacles: If collectivization proceeds too fast, private farms go out of business more rapidly than cooperatives can expand, so that acreage under cultivation drops (pp. 251, 265); given fixed prices, limited supply and increasing demand, rationing and dual pricing become necessary (p. 125 ff.), etc. However, there is no general statement as to the limitations which "scarce means" actually impose upon the desires of communist governments to expand heavy industry, collectivize agriculture, and have "good" relations with the USSR. The inquiring reader will ask how the limitations on the process of planning and controls actually have manifested themselves. It is perhaps too much to expect a complete answer yet, but to ask the question suggests a need for further exploration. It is to be hoped Spulber will later explain why "planning cannot accomplish . . . sustained growth at *expanding* rates" (p. 360, italics his); or, if "industrialization, *à la russe* could not be carried out 'administratively' by each and every country" (p. 362) just how Russian a country must be to play the Russian game (we will not call it roulette).

EDWARD AMES

Purdue University

Western Enterprise in Indonesia and Malaya. By G. C. ALLEN and A. G. DONNITHORNE. (New York: Macmillan. 1957. Pp. 321. $5.75.)

The authors have added another volume to their studies on western enterprise in Asian countries. The present treatise follows rather closely the framework of the earlier study on China and Japan. However, while the first book emphasized the contrast in development between China and Japan, the countries now covered show many similarities. In Indonesia and Malaya modern development occurred under a colonial administration, both territories suffered from the effects of the business cycle and war devastation, both countries recently acquired independence. Their history is entirely different from that of either Japan or China.

In describing the pattern of development under the vagaries of world mar-

kets, war and politics, the authors have painstakingly tried to be descriptive
rather than critical. Agricultural estates, mining, banking, shipping, public
utilities, commerce, and manufacturing industries are dealt with in chrono-
logical order, comparing both territories sector by sector. In general, Indonesia
is treated first; significantly, in mining the sequence is reversed.

The general conclusion following from this sectoral approach seems not
unjustified—in Indonesia and especially on Java, the government fulfilled
as (Western) entrepreneur a much more significant role than in Malaya.
It may be questioned however, whether this was the result of a more *laissez-
faire* attitude on the part of the Malayan administration. A distinction can be
made between the policies followed in Indonesia—roughly speaking before
1870, from 1870 to 1913, and thereafter. In the first period, the government
established a great variety of large enterprises, in the second period it left the
initiative largely to private business, while in the third period an economic
and social policy developed aiming to reconcile the interests of big business
and the Asian masses. In Malaya, and likewise on Sumatra and Borneo, the
pre-1870 period was skipped. As usual, latecomers are apt to be more modern,
just as the policy on Java was modern when at the end of the eighteenth
century any idea of slave-labor was discarded.

The comparisons would have profited and the underlying bias against "gov-
ernment-in-business" might have been further submerged, if the historic se-
quence and perspective had been emphasized more.

The motivation for more government intervention in economic life in
Indonesia in the early part of the 19th century was the lack of a developed
market for capital and products. After the separation of the budgets of
Indonesia and the Netherlands, government ownership or participation in
railroads, forestry, estate agriculture and mining was considered a method
of local capital formation as well as a catalyzer for private investment. There
was hardly a trace of a policy to keep private Western enterprise out; to the
contrary, there were numerous measures designed to attract it. But at the
time of the first world war social legislation developed rapidly, designed to
protect the laborer and regulate relations between Western enterprise and Indo-
nesian society. Similarly, an economic policy towards diversification and
defense against the instability of export prices (it could not have been a
countercyclical policy) was developed. The authors do not deny the existence
of these general policies, but in view of their importance for Western enter-
prise they might have been highlighted.

The description of the effects of the second world war in both countries, of
the difficulties experienced after the war and the prospects in independent
Malaya and Indonesia are well written and the gloomy expectations for
Western enterprise in Indonesia have an almost prophetic ring. As in the pre-
vious book, the authors display a lucidity of style and a balance of judgment
which is impressive. The available material is so overwhelming that a careful
choice of sources had to be made. On the whole the authors have succeeded
extremely well in their task.

E. DE VRIES

The Hague, Netherlands

Statistical Methods; Econometrics; Social Accounting

The Soviet 1956 Statistical Handbook: A Commentary. By NAUM JASNY.
    (East Lansing: Michigan State University Press. 1957. Pp. xii, 206.
    $4.95.)

Compared to the trickle of data on the Soviet economy from 1937 to 1956 the published statistical information currently becoming available from Soviet sources has taken on flood proportions. A number of commentaries on these new handbooks have recently appeared.[1] In one of the more ambitious and rewarding of these commentaries Naum Jasny has provided a very useful critique of the new data as illustrated by the statistics available in the first, and one of the most important, of the statistical handbooks. But even in limiting himself to one handbook he has had to emphasize selected areas in his coverage. Agriculture, for example, is covered in great detail, whereas transportation is all but passed over.

A historical summary of Soviet statistical reporting forms a particularly interesting section of the book. In Jasny's view the current change in statistical reporting is more of a return to the methods of the 1930's than to the pre-industrialization period of the 1920's. It is also his opinion that the quantity of statistical data may have risen sharply but that the quality is still very low by either contemporary Western standards or Soviet standards during the 'twenties.

Likewise Jasny's page-by-page comments on the current statistical data which comprise the bulk of his book, make worth-while reading. Specifically, he points out omissions, shortcomings, and the appearance of new data, as well as suggesting interpretations of specific data. The attention he gives to this specific data is apparently governed by his own personal interest rather than by any objective standard of importance. He does not attempt to deal with the larger omissions in the over-all coverage of this handbook, nor does he attempt to interpret the new data in any broader context. We learn, for example, that horses are omitted from the category "productive livestock" (p. 118), but our attention is not drawn to the large gaps and omissions in Soviet reporting for such areas as foreign trade, defense, budgetary or fiscal matters.

Any economist interested in using Soviet data would do well to refer to the pertinent comments in Jasny's book before attempting a full analysis of the data involved. Although these comments may sometimes appear to include everything that came into the author's mind, the author's long experience with Soviet statistical material has nevertheless provided him with a wealth of information and many insights on a wide range of aspects of the Soviet economy.[2]

[1] See A. Nove, *Soviet Studies,* Oct. 1957, IX 127-28; A. Gaev, *Bull. Inst. Study of U.S.S.R.,* Aug. 1957, IV, 58-60; V. Marchenko, *Vestnik Institute,* No. 4, 1956, 82-88; *Economist,* July 20, 1957, CLXXXII, 229-31.

[2] Jasny's firsthand experience with Russian statistical data includes participation in the following activities: Economic Division of the All-Russian Union of Cities during the first world war; the Supreme Economic Council under the Kerensky regime, and work in Germany as a grain expert for the Soviet government during the 1920's.

As to the reliability of Soviet statistics, Jasny points out that "Soviet statistics are an amalgam of elements varying from trustworthy data (mostly pertaining to physical units or details) through ambiguities to obviously distorted estimates (mostly data from aggregates)" (p. 14). This reviewer would certainly agree that the data presented in physical units in these handbooks are much more reliable material and will probably be much more useful to the student of the Soviet economy than the other statistical measures which are given, particularly the aggregative measures in index number or value terms. Outstanding examples of unreliability are figures for the gross value of industrial output by year or by region and labor productivity for various years. Measures such as these are difficult to interpret, and at best can be used to represent orders of magnitude.

The difficulty of interpreting Soviet data is further aggravated by the lack of documentation or explanation of methodology. Any one who is accustomed to using the U. S. *Statistical Abstracts* would be surprised to learn that not one line in the Soviet handbook commented on by Jasny is devoted to source references or explanations of methodology. Thus, even the best data, the physical units of industrial output, must be interpreted with great care, especially if they are used to make international comparisons. One might be tempted to accept the Soviet comparisons of production levels of individual commodities such as steel, coal, electric power, and petroleum. But these measures do not reflect changes in quality over periods of time or internationally. Moreover, the Soviet policy of providing more data for the favored areas of heavy industry may lead one to assume greater over-all industrial growth than has actually occurred.

The statistical handbook reviewed by Jasny is but one of an increasing number of handbooks now available on the Soviet economy. A partial list of the formal handbooks is presented in Table I, but the new relaxation of restrictions on the issuance of statistical data is also reflected in many other Soviet publications. For example, no serious student of the Soviet economy should overlook the November issues of the Soviet industrial and economic journals, which provide data on the basis of a 40-year summary in commemoration of the October Revolution.

The above list could easily exceed 100 by the end of 1958 if all other organizations comparable to those listed in the table publish statistical handbooks. The number of handbooks is particularly inflated by the issuance of oblast data by the Central Statistical Agency's units in the oblast (province) and comparable administrative regions. Almost all the regional handbooks have a similar sector break-down and time coverage. Most of the handbooks have individual sections devoted to industry, agriculture, capital construction, transportation and communications, labor force, domestic force, education, and health. The years covered in most cases include 1940, 1945, 1950, and 1955.

A detailed survey of the contents of individual handbooks in this series is obviously impossible within the limited space of this review, but one comment seems quite appropriate. Although some new data appears in each handbook issued to date, the inclusion of much unnecessary and repetitious data considerably inflates their size. For example, nearly all presentation in

TABLE I.—SOVIET STATISTICAL HANDBOOKS

(A Partial List)

Compiling Agency and Publisher	Title	Publication Date	Pages
	Sector and Industry Handbooks		
*1. TsSU, Moscow#	Kul'turnoe stroitel'stvo S.S.S.R., st. sb. (Cultural Progress in the USSR)	1956	331
*2. TsSU, Moscow	Sovetskaia torgovlia; st. sb. (Soviet Trade)	1956	351
3. TsSU, Moscow	Zdravookhranenie v S.S.S.R. (Public Health in the USSR)	1957	178
*4. TsSU, Moscow	Promyshlennost' S.S.S.R.; st. sb. (Industry in the USSR)	1957	446
5. TsSU, Kiev	Zdravookhranenie v U(kr) S.S.S.R.; st. sb. (Ukrainian) (Public Health in the Ukraine)	1957	140
*6. TsSU, Moscow	Posevnye ploshchadi S.S.S.R.; st. sb. (Tilled Area in the USSR)	1957	2 v.
*7. TsSU, Moscow	Chislennost' skota v S.S.S.R.; st. sb. (Census of Livestock in the USSR)	1957	619
8. TsSU	Ugol'naia promyshlennost' S.S.S.R., st. sb. (Coal Industry of the USSR)	1957	368
*9. TsSU, Moscow	Transport i Sviaz' (Transportation and Communications)	1957	259
	All-Union and Republic Handbooks		
*1. TsSU, Moscow	Narodnoe khoziaistvo S.S.S.R.; statisticheskii sbornik (National Economy of the USSR; Statistical Handbook)	1956	262
*2. TsSU, Moscow	National Economy of the USSR; Statistical Handbook (English edition)	1957	230
*3. TsSU, Moscow	N.kh. R.S.F.S.R.; st. sb.	1957	370
*4. SU, Kiev	N.kh. Ukrainskoi S.S.R.; st. sb. (Ukrainian)	1957	534
*5. TsSU, Moscow	N.kh. S.S.S.R. v 1956 g.: st. ezhegodnik	1957	296
*6. SU, Kishinev	N.kh. Moldavskoi S.S.R.; st. sb.	1957	197
*7. SU, Ashkhabad	N.kh. Turkmenskoi S.S.R.; st. sb.	1957	171
*8. TsSU, Moscow	N.kh. Belorusskoi S.S.R.; st. sb.	1957	319
*9. TsSU, Moscow	Dostizheniia Sovetskoi vlasti za 40 let, v tsifrakh (Soviet Achievements During the Past Forty Years, in Figures)	1957	370
*10. SU, Frunze	N.kh. Kirgizskoi S.S.R.	1957	207
*11. SU, Riga	N.kh. Latviiskoi S.S.R.	1957	227
12. SU, Stalinabad	N.kh. Tadzhikskoi S.S.R.	1957	387
13. SU, Tallin	N.kh. Estonskoi S.S.R.	1957	307
14. SU, Tashkent	N.kh. Uzbekskoi S.S.R.	1957	197

Handbooks for Cities, Oblasts, and Comparable Administrative Regions
(by name of region only)

*1. Kostromskoi oblasti	*6. Stalingradskoi oblasti
*2. Sverdlovskoi oblasti i goroda Sverdlovska	*7. Cheliabinskoi oblasti
	*8. Tatarskoi ASSR
*3. Ivanovskoi oblasti	9. Iaroslavskaia oblast'
*4. Kirovskoi oblasti	*10. Buriat-Mongol'skoi ASSR
*5. Goroda Leningrada	*11. Molotovskoi oblasti

12. Zakarpatskoi oblasti
13. Ul'ianovskoi oblasti
*14. Chkalovskoi oblasti
15. Karel'skoi ASSR
16. Murmanskoi oblasti
17. Orlovskoi oblasti
*18. Leningradskoi oblasti
*19. Novosibirskoi oblasti
*20. Chuvashkoi ASSR
21. Arkhangel'skoi oblasti

*22. Irkutskoi oblasti
23. Kalininskoi oblasti
24. Komi ASSR
25. Kabardino-Balkarskoi ASSR
26. Velikolukskoi oblasti
27. Kuibyshevskoi oblasti i goroda Kuiby-
     sheva
28. Abkhazskoi ASSR
29. Udmurtskoi ASSR
30. Adygeiskoi avtonomnoi oblasti

* At the Library of Congress.
# TsSU refers to Tsentral'noe statisticheskoe upravlenie (Central Statistical Agency) attached to the Council of Ministers of the U.S.S.R.; SU to the appropriate regional office.

physical units is followed by needless percentage breakdowns. If in the regional handbooks attention were restricted to physical units of industrial production in 1955 and 1956, the latest years given, a handbook totaling some 300 pages might be reduced to a very small number of pages, in several cases to a single page.

In following the almost daily arrival of new handbooks at the Library of Congress a question recurring to this reviewer is: Why have the Soviets decided to release this new economic data? On this question Jasny concludes "Thus, the very manner of presenting the material stamps the *Handbook* as a propaganda publication" (p. 7). But propaganda as the sole or even primary explanation does not seem credible to this reviewer. Instead I would suggest a series of interrelated explanations drawn from Soviet and Western sources.

First, the new policy on availability of economic statistics may be a part of the "destalinization policy" or "thaw" in the Soviet body politic. This is a particularly difficult hypothesis to test or even to examine, as the relaxation in various aspects of Soviet domestic life following the death of Stalin has been very uneven since the initial period of rapid change. Moreover, the motivation and the actual degree of relaxation are often difficult to measure.

Second, the preparation of these handbooks may represent an effort on the part of the Soviet leaders to improve their own statistical reporting machinery. It may be true as is suggested by a number of Western writers, among them Jasny, that the State Planning Commission (*Gosplan*) has a complete set of statistics available for internal use. It may also be true, however, that the uniformity, comparability, and quality as measured by other statistical tests are poor for the data collected, especially from local reporting units.

The accumulation of reports from all the operating levels, which appears to be the procedure followed in issuing the new statistical handbooks, is a good initial approach to an improvement of the published statistics. A logical next step for Soviet statisticians would be to establish something on the order of the standard industrial classification system (S.I.C.) used in the United States. The S.I.C. was designed to bring about uniformity and comparability of reporting of all economic activity in the United States, the lack of which in Soviet statistical reporting is evidenced by these statistical handbooks.

Third, Khrushchev's decentralization program apparently calls for a larger share in the planning and operation of the economy at lower administrative levels. Detailed reporting of statistical data by oblast and city may be considered a necessary first step for greater participation of these administrative units in the operation of certain economic sectors. The fuller reporting in these regional handbooks of industries such as timber, fuel, and agricultural products suggests that the coverage coincides with the economic activities in which the role of regional planning is intended to increase. It is of interest that nearly all the oblast and ASSR handbooks currently available are from the RSFSR, the largest of the Soviet republics. Most of these administrative units coincide geographically with the new regions of the economic councils (*Sovnarkhozy*) under Khrushchev's decentralization program. The economic regions also generally coincide with the oblasts of the Ukrainian republic, although few handbooks are as yet available from Ukrainian oblasts.

At the same time, some reports indicate that the new decentralization would paradoxically result in more central control. However, as some of these reports were made by officials such as Zverev, Minister of Finance, they may indicate a centralization of *financial* control as a concomitant of relaxed control on *physical* production.

Fourth, competition between regions is a significant factor. This theme is receiving considerable attention in the Soviet press and may be part of an effort to exhort the Soviet people and responsible local leaders to greater efforts. As pointed out by A. Nove, data unfavorable to the Soviet Union in terms of international comparisons such as for relative consumer goods production are presented in each of these handbooks.[3] The appearance of unfavorable data might be explained as laying the basis for a later claim of overtaking the U. S. for various production levels.[4] Or it may be a method employed to spur on the local party secretaries, plant managers, etc.

Fifth, the increased availability of statistics is probably intended to increase the quality of Soviet research and theoretical debate. The very interesting discussions on long-term growth and the theory of value currently appearing in the Soviet press are surely encouraged and aided by the availability of more published data.

Finally, pride in national accomplishments may be cited as a rationale for the new availability of economic data. Some aspects of Soviet economic growth have, in fact, been remarkable, and desire to present these accomplishments in statistical form both for the world and for their own people was undoubtedly a factor in the decision to make available more statistical data on Soviet economic development. Closely related to this theme is that of propaganda, as it can hardly be contended that the Soviet leaders are attempting to place an accurate picture of the Soviet economy on the record. Still, if propaganda were the primary consideration, it would have been more logical

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[3] A. Nove in an unpublished commentary on *Socialist and Capitalist Countries in Figures*.

[4] Suggested by D. Gale Johnson in comments on Soviet agricultural production at the meeting of the American Economic Association, December 29, 1957.

for the Soviets to prefer a smaller number of titles with larger circulation and translated into various foreign languages.

Economists interested in the Soviet Union may be thankful for whatever additional assistance they get in their research. However, the increased availability of data does not appear to be sufficient to warrant a basic shift in the use of the Soviet statistics from the painstaking, detailed type of examination and interpretation necessary to date. Some greater emphasis on general policy questions based on the accumulated work of Western scholars and the additional data now availiable will certainly be possible, but only after careful examination and interpretation of the data.

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Quantity and Price Indexes in National Accounts. By RICHARD STONE. (Paris: Organisation for European Economic Cooperation. 1956. Pp. 120. $1.50.)

In recent years more comprehensive and consistent approaches to index number construction and interpretation have been evolving in this country and abroad in good part as a result of the development of national accounting statistics. This development has been fostered by the United Nations and the Organisation for European Economic Cooperation by their recommendations on how statistics on production, prices, and national income and expenditures should be developed and presented. Professor Richard Stone has been a leading participant in work by both organizations, and his present report on quantity and price indexes done for the OEEC is a significant contribution to the subject of measurement of "real" output of the national economy.

While Stone's style is likely to seem forbidding to all but a relatively small group in the field of index number and national accounting methodology, his report has broad implications for important statistical programs in this and other countries. Stone deals mainly with two questions. One involves construction of a consistent set of price and quantity index numbers for the total economy. The other relates to an ever-present challenge in index-number construction—measurement of changes in quality.

In order to achieve consistency in a system of price and quantity indexes for total national economic activity, Stone contends that the accounting of this total in "real" terms should be confined to "commodity transactions." Commodities are defined to include goods and services, other than factor services. Thus Stone suggests that for a consistent presentation of national output statistics in terms of constant prices two approaches are appropriate. One is the value-added framework, showing the goods and services supplied by each industry. The other is the expenditure framework showing the final value of goods and services purchased by broad classes of consumers including households, business, and government. With a consistent basis of valuation, the value-added and expenditure frameworks yield equal aggregates, apart from errors in data.

The different kinds of detail provided by each framework serve different analytical purposes. A presentation of the industrial origin of goods and services helps in analysis of supply developments and of the use of resources including use of fuel, manpower, and electric power. The detail in the expenditure framework lends itself more to study of demand, and in approximate fashion to study of welfare. Work with both frameworks helps to verify and make more coherent the study of supply and demand developments in the economy. Also, both frameworks can be systematically integrated in input-output tables showing the sources and uses of national product.

The changing values in current prices shown for the components of each framework,when divided by the corresponding values in constant prices, yield implicit price indexes or "deflators." These three sets of data—current price series, constant price series and "deflators"—are, in Stone's view, the basis for systematic analysis of price and quantity changes throughout the economy. On this latter point, however, differences of view are likely to develop because of possible confusion between regularly compiled price indexes and these implicit measures.

In this country, work on regular publication of national accounting statistics in real terms is in various stages of development. On the final expenditure side, the gross national product statistics in constant dollars are published annually with some prospect of quarterly publication. On the side of value-added industry measures—several major sectors of this framework are regularly published monthly including an index of industrial production (manufacturing and mining), an index for electric and gas utilities, and an index of construction activity. In addition, various indexes of agricultural output are available annually. Unofficial annual totals for the private economy, including measures for trade, transportation, and services have been prepared. Also, for the manufacturing sector, work has been done for the years 1947 and 1949 through 1953 on "net output" indexes involving a deflation of industry inputs (except factor inputs) and outputs with net output derived by subtraction.

In recognition of the importance of real output measures, the *Economic Report of the President* for 1958 headed its list of recommendations for a program of improved federal statistics with proposals for quarterly estimates of gross national product and its major components in constant prices and for measurement in constant prices of net output for several major industry groups. Stone's report does not seek to answer all the detailed questions that would be involved in implementing these recommendations. He does distinguish by implication between short-term measures and longer-term measures. More systematic treatment on this point would have been desirable. Thus Stone's discussion of the detail in the data required for full analysis of commodity flows, for adjustment to reflect quality changes, and for input-output analysis, would have benefited by a distinction between what might be done on a current basis (monthly, quarterly, or even annually) as against what might be accomplished more completely at "benchmark" dates.

This distinction is particularly relevant in connection with Stone's suggestions for adjustment for quality changes. As is well known, individual products often treated as identical tend to improve in quality because of changes

in functions, design or other specifications. Sometimes, such changes can be measured with reasonable accuracy if sufficient detail by type of product is available. However, even considerable detail does not help when the bulk of the individual items made are in fact different in different periods. In such a case, changes in value may result from changes in quality as well as in price or in the simple count of the quantities produced.

Stone suggests that quality differences can be measured if information can be obtained on a set of specifications which can "explain" price (*i.e.*, quality) differences among different grades of a product in the base period. For example, if poundage and horsepower could explain nearly all of the price differences among autos of different make and design, then an appropriate weighting of these two specifications might account for changes in quality. Such a measure might be superior to a simple unit count of autos. It does involve the assumption, however, that these specifications would be the chief source of *change* in quality of individual makes and models. This is a difficult question, and like Stone's discussion of seasonal adjustment problems, might have been more suitably put in a technical supplement.

There is full recognition in Stone's report of the need for a flexible and eclectic approach to the problem of measurement. The need to exercise judgment and ingenuity in the use of imperfect data is clear. Those familiar with measurement problems realize how much heroic effort goes into the job of estimating the facts. In the United States it should not be necessary for compilers of official indexes to make ingenious estimates of output for such key areas as producers' and military equipment. While conceptual or index-number problems of valuation will always be very perplexing in this area, the problem is in good part perhaps a practical one of obtaining the basic information. The answer seems, to this reviewer, to lie in giving greater prestige and importance to the need for a more adequate factual record of economic growth. The reports by international organizations, such as the report by Stone, should be expected to further this objective.

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* The views expressed in this review are those of the author and do not necessarily reflect those of the Board.

Economic Systems; Planning and Reform; Cooperation

Factory and Manager in the USSR. By J. S. BERLINER. (Cambridge: Harvard University Press. 1957. Pp. xv, 386. $7.50.)

The principal character in this book, the factory manager, is central to the question of the operating effectiveness of the Soviet economy, because upon his shoulders rests the primary responsibility for translating the formalities of the state economic plans into concrete decisions and specific activities. But the very nature of his position presents a complex and obscure picture to the analyst. The fact that Professor Berliner has succeeded in bringing many individual aspects as well as the over-all view of the Soviet manager

into sharper focus therefore makes this an important book and one that will interest a wide range of readers.

The book opens with a lucid summary of Soviet economic institutions in so far as they form the operating milieu for the factory manager, following which the "goals of management" are discussed. In developing this subject at some length, the view is put forth that, of all considerations which might be termed "goals," the desire for money premiums stands clearly to the fore. I shall comment on this conclusion below.

The largest portion of the book is devoted to an elaboration of the operating techniques employed by the Soviet manager in attaining his goals. In addition to techniques *pro forma* acceptable to the authorities, "three leading principles of action" are cited which, although in varying degrees unacceptable officially, appear to be in fairly wide application: (1) the cultivation of a "safety factor" in production and procurement; (2) the alteration of the "assortment of production," either in quantity or quality, or through the falsification of results; and (3) the use of activities combining "illegality with personal influence."

The final section of the book (aside from a summary chapter and one on post-Stalin reforms) is devoted to the matter of controls over management. Berliner weaves into a discussion of the formal aspects of control, involving the ministerial apparatus, the Communist party and the trade unions, the essential features of what he calls "mutual involvement" between management and the control representatives, a condition which goes some way to explain the continued existence of the illegal or semilegal activities outlined in the preceding chapters.

Many of the aspects of Soviet managerial life discussed in this study have appeared in other studies, notably that of David Granick.[1] What is unique in this instance is partly the inclusion of the postwar period, but mostly the novelty of the sources, which comprise in addition to Soviet printed materials (of a type also used in other studies), the testimony of forty-one former Soviet citizens with experience in various managerial positions prior to their defection following the second world war. These first-hand sources give the author insights not otherwise obtainable; but at the same time, broad areas of similarity remain in comparison with other studies. The defector materials do not so much reveal aspects of managerial behavior heretofore completely unnoticed, as they permit a reordering of emphasis and the injection of considerably more depth and "personality" into the presentation.

The use of a relatively small number of defectors raises methodological questions which the author discusses at some length, concluding that the testimony is "reliable," given the structure of the interviews and the analysis to which the results were subjected. Along the same lines, however, it is interesting to note that, although this is a study of the "factory" and the "manager" in the U.S.S.R., not one of the forty-one informants had ever been a manager of a factory. The closest resemblance involves one director of a construction trust and one chief of the regional office of the Commissariat of Procurement, while the higher echelons of factory management are represented by six chief

[1] D. Granick, *Management of the Industrial Firm in the U.S.S.R.*, New York 1954.

engineers and a number of department heads. Having in mind that the author's basic objective is to portray "what it is like to be a manager of a Soviet industrial enterprise" (p. 1), and recalling that the manager himself, as distinct from all other individuals in the management complex, is responsible for decision-making, I am not sure that the absence of bona fide factory managers among the defectors does not qualify the usefulness of the materials to a certain extent. Apparently Berliner assumes otherwise, however, when he observes that he is (perforce) interested in these individuals "not as 'average respondents' representative of a parent statistical population, but as 'expert informants' capable of . . . reporting objectively on their own and others' experiences" (p. 8).

The interview materials are most effective in the examination of operating techniques, but they are also used to support a re-evaluation of the "goals of management." The conclusion is that money premiums are the "dominant goal [that] more than anything else stands at the forefront in the making of decisions and explains why one alternative is chosen in preference to another" (p. 43). I am not completely certain what this is meant to convey as an indicator of basic managerial motivation. It is a fact that premiums have been intended primarily to reinforce "plan fulfillment and overfulfillment," and that the latter phrase encompasses what is generally considered (by Berliner and others) to be what the authorities reckon as the most desirable managerial objective. In other words, premiums have been designed to achieve a relatively specific purpose, as distinct from representing in and of themselves the net effect of all managerial activities, in the sense of "profits" under capitalism.

Subject to this limitation, the seeking after premiums is officially respectable behavior, however much it reflects basic motivation on the part of the manager. But Berliner seems to be suggesting something even stronger; he seems to suggest that there is a tendency for the manager to regard premiums purely as ends in themselves. Unfortunately, this is difficult to prove, because premiums and "plan fulfillment" pull in the same direction most of the time. In any event, I question the idea, at least as far as the "successful" manager is concerned. I would not expect him to become absorbed in the quest for premiums to the exclusion of their "real" counterparts, because if he did, he would make himself psychologically vulnerable in a way inconsistent with success as a manager.

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Business Fluctuations

Monetary and Fiscal Policy under Uncertainty. By KARL-OLOF FAXÉN. Stockholm Economic Studies, n.s. 1. (Stockholm: Almquist and Wicksell. 1957. Pp. 212. SKr. 20.)

The *fiscal* policy examined by Faxén is represented by the following government parameters of action: public expenditures (for purchases from business) and (proportional) income taxes on wage earners and business firms; as to

monetary policy, Faxén considers actions influencing the interest rate and those directly influencing firms' borrowing. The various models for the economy which he discusses have in common: (a) a simple consumption function (consumption as function of "yesterday's" disposable income), (b) an oligopolistic price-wage relation according to which price is a multiple of the wage rate, (c) a particular production function relating output to labor input in the same period and shifting according to the changes in the stock of capital and the—imperfect and slow—interindustrial and technological adjustments. The various models also have in common the following unknowns: (1) the quantity of production, (2) the price level of products, (3) the volume of employment, (4) the wage level (all four in index form), (5) the gross value of business investment, and (6-8) changes in the national debt and in the financial assets of the business sector as well as of the wage earners.

Any system of difference equations in which both price and quantity of output are to be determined, is essentially nonlinear, and can be solved only in exceptional cases. Faxén succeeds, however, by extreme simplifications (zero interest and dividend payments, constant investment, and taking the business sector's financial assets as a fixed proportion of sales) in rewriting the equations for employment and prices in such a way that besides the government's parameters of action they contain only past values of the variables. The price and wage level proves not to be influenced by business taxation; hence it can be controlled by government action independently of the volume of employment. If, on the other hand, the situation in the labor market is assumed to influence wages (and indirectly prices), the price-wage level and the volume of employment cannot be fixed separately; in other words, full employment implies higher prices than underemployment, and extraneous tendencies to rising wages and prices cannot be counteracted by government policy without employment also being affected.

To go beyond this result, Faxén works out some numerical examples for an underemployment economy, under the following assumptions: firms are supposed to face the alternative, in the first half of the period, of either maintaining the current rate of investment of 25 units, or increasing it to 27 units; and in the second half of the period firms face the alternative of either investing 27 units or increasing it further to 29 units. The investment decisions for the second subperiod are influenced by government policy, for which, in turn, two alternatives are considered: whether in the second subperiod the government imposes heavy quantitative credit restrictions or higher taxes on households. Examining the effect of the policy chosen on income and what he calls the "liquidity" of business, Faxén establishes a certain order of preference for the seven possible alternatives; naturally, processes leading to overemployment (hence inflation) or, at the opposite end, processes not eliminating underemployment get a low mark; furthermore the government may be presumed to strive for the largest possible volume of investment following a steady course of development, as Faxén somewhat cryptically expressed it. Thus the government would prefer the firms to produce 27 units in both periods, but this combination does not prove the most profitable for the producers regardless of whether quantitative credit restrictions or higher taxes

on households are imposed. However, by pursuing the latter policy the government can realize the second-best combination, 27 units in the first period and 29 in the second one; investment is higher and consumption is lower than under the first combination.

While the result is acceptable from a common-sense angle, it cannot be considered proved—an inevitable outcome of an analysis based on a numerical illustration. Is it certain that no more can be done? To attain his results, Faxén had to resort to further drastic simplifications: the increase in the stock of capital is not supposed to affect prices and output in succeeding periods, nor will the investment process slacken in periods of declining utilization and consumption. The dynamic character of the process is ensured by retaining the consumption time-lag—no other dynamic features occur; and for the wage earners this time-lag has been shown by Tinbergen to be negligible. Had Faxén discarded also this last dynamic element and, where necessary, chosen for his production function a linear approximation, more general results might have been obtained both for the general model and for the modified model.

The book contains interesting supplementary studies on planning by business enterprise and on wage policy. For lack of space we limit ourselves to a brief comment on the former. Here particular attention is paid to the "self-financing" policy of firms. If the oligopolistic firm's expansion depends on self-financing and a fixed relation exists between the stock of capital and the rate of production, there is an optimum price policy for the firms, depending, *inter alia,* also on the reactions of the competing oligopolists. The graphical analysis does not seem in this case to entail a serious loss of generality; it is extended by Faxén to include the effect of government tax policy, interest rate policy and partial self-financing. He also believes that the results obtained by micro-analysis of the firm can be made applicable in macro-analysis to aggregated business sectors, but does not explain the meaning of aggregation of demand functions for the various products of individual firms, though each of the functions could be set up only under the assumption of given price and wage policy of the competing firms.

While the principal analysis of the study could have been fruitfully expanded in various directions,[1] the lengthy methodological chapters comprising almost half of the book could have been substantially condensed or omitted altogether. We are informed by now about the disequilibrium approach of the Stockholm School; the graphical presentation of alternative policies in "partition trees" is understandable even for a reader who skips Chapter 3, and moves directly from the Introduction to Chapters 4 and 5.

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[1] The identities in the general model also require careful checking. In identity (1) the sales value of products is put equal to the sum of the wage bill, gross investment, business taxes and change in "liquidity." Suppose a firm does not sell anything during a given period but produces for inventory: would it have to compensate by net changes in financial assets (in "liquidity") both the wages paid and inventory investment during the period? In other words, should not inventory investment have been excluded from the gross value of business investment, contrary to his statement (p. 113)?

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

The Federal Funds Market: Its Origin and Development. By PARKER B. WILLIS. (Boston: Federal Reserve Bank of Boston. 1957. Pp. 39.)

It would be hard to find many definite references in the general literature to the nature and significance of the federal funds market. Partly as a consequence, the growing importance of the federal funds rate as a key policy instrument has been overlooked in many quarters. What Parker Willis has done for the layman in this little book is to spell out the role of "a sensitive indicator of shifting pressures in the banking system" of the United States.

One cannot come away from a reading of this excellent little book without a greater appreciation for the increased complexity of open-market policy in recent years and the vastly enlarged possibilities for flexibility in the administration of that policy.

But what Willis does not say is in its way just as important and it remains to be said. Here is the one market where is determined currently the one rate that is now being thought of as the "key" rate. Since by nature, it is a "residual" market, the funds market tends to reflect the changing impact of business conditions and policy pronouncements more readily than any other. But what are the implications of this "new" market for Federal Reserve policy? Are current policy instruments adequate for their assigned task?

Moreover, little is said about the possible evolution of the funds market; for it seems certain that today's organization is in the process of dramatic change. On the one hand, banks, particularly those in the larger centers, may turn to funds transactions more and more as they find this market more convenient and more pervasive. On the other hand, this market which up to now has been dominated by non-bank firms may become a bank-controlled entity.

Willis provides a basis for understanding the nature of the funds markets. He does a less effective job in describing the possible role of that market. And he leaves his reader rather in the dark when an explanation seems necessary to account for concern of the Federal Reserve Board over the current structure of policy instruments. But his *Federal Funds Market* is an essential part of the general literature on money markets.

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International Economics

La Zone Sterling. By JEAN DE SAILLY. (Paris: Armand Colin. 1957. Pp. xviii, 131.)

The persistence of the sterling area as a regional monetary arrangement since 1939 has given rise to a growing body of economic literature. A recent

addition is the booklet of some 130 pages presenting an "over-all" view of the sterling system, by Jean de Sailly.

The author's approach reflects his basic concern with an evaluation of the relative effectiveness of the British and French regional monetary systems in coping with problems of achieving balance-of-payments equilibrium. De Sailly regards the entire twentieth-century British experience as a basically successful application of extraordinary flexibility and judgment by successive governments in meeting recurring crises in the international monetary realm. The persistence of the comprehensive arrangements of the sterling area without detailed written agreements and elaborate formal steering machinery is considered a remarkable achievement.

The system tends to strengthen the economic cohesion of the area. Through reinforcing the imperial preference system, exchange restrictions buttress the traditional complementary relation existing between industrially and financially advanced Britain and the raw-material-producing Overseas Sterling Area. Moreover, the accumulation of blocked balances by the colonies allowed the metropolis to resume investment in the Independent Sterling Area. Without this flow, the export of capital goods would have been impeded and the pressure to accelerate imports from the United States aggravated.

Yet de Sailly is aware of the formidable character of the stresses and strains afflicting the system. Without dollar aid, the British would have been unable to generate over-all balance-of-payments surpluses necessary to sustain the export of capital to the independent members of the area. Since 1955, domestic inflationary pressures have restricted the magnitude of this flow. India not only has been a recipient of British investment but has drastically reduced the size of its sterling assets accumulated during the war. Yet her development programs have required increased financing from the United States and the World Bank. Finally, as the colonies achieve a greater measure of independence, it seems unlikely that they will continue to augment their sterling balances by exporting to the dollar area (especially when the dollar values of the balances could be adversely affected by devaluation) and to purchase British rather than American equipment.

The establishment of current-account convertibility at fixed rates of exchange with the elimination of the distinction between American-account and transferable-account sterling presents serious hazards. It would foreshadow increased competition with the United States not only within but outside the sterling area. The mitigation of the risks associated with this move would require the unlikely achievement of the following combination of conditions: a "sufficient" level of dollar and gold reserves, the adoption of an enlightened commercial policy by the United States, and equilibrium in the balance of payments for the sterling area. In the long run, a significant expansion in the British capacity to export will be required when the maintenance of full employment and rising living standards are "perfectly anchored in the British mentality" (p. 123). Despite the profound and continuing nature of these problems de Sailly appears confident that the flexible nature of the sterling area will produce more effective solutions than the French system.

Yet this conclusion reflects both the strength and weakness of the work. Nowhere do we find an examination of the criticisms of the functioning of the sterling system raised by Bell and Zupnick. In his descriptive treatment of the major postwar problems, de Sailly avoids considering whether more effective coordination would have lessened the severity of the recurring crises. Extended discussion, moreover, is lacking of the fundamental problems confronting Great Britain in achieving a more desirable allocation of output between exports to the dollar area, the sterling area and the rest of the world, and domestic investment. It is easy to criticize a short work for acts of omission, which partially reflect differences in emphasis and approach. Although no original information is provided, this booklet should be interesting to those seeking a synthesis by a French student of the international economics of the sterling area.

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L'Echange international. By MICHEL MORET. (Paris: M. Rivière & Cie. 1957. Pp. 371. 1,350 fr.)

The main objective of this study is to review the present status of international trade theory. The first part deals with the classical theory of international trade. Moret then explains that the modern theory has developed in two principal directions. On the one hand, there have been the attempts to improve the classical theory and to adapt it to realities and new circumstances. Among these improvements the author mentions various refinements concerning the terms of trade and their calculation, and attempts to evaluate properly the probable effects of devaluation. Ample use is made of econometrics, and the problems of elasticity of demand and supply and of multiplier are discussed.

Other economists have tried to break completely with the past and begin with a new foundation. The income theory which is linked with national accounts is prominent in the new international trade theory and the needed adjustment in the balance of payments is derived not from interplay of prices but from changes in national expenditure. In this connection the author touches on international economic relations within the framework of economic growth.

Moret sees the main weakness of the present state of the subject in the fact that the two principal theories ignore each other. Thus a new orientation in research is proposed which should attempt a synthesis of the price and income approaches, and should study the effects of economic domination and the problem of centrally controlled economies in international relations.

In his conclusions Moret quotes Jacob Viner who, while insisting on the necessity of improving the present theory, expresses doubts that a general theory can be worked out. He says that the classical theory cannot be directly applied and perhaps no adequate general theory can be developed for the world of today. Moret, however, thinks that the economic theorist should become the guide of external economic policy. To that end he should determine the limits of a theory on the basis of which policy-guidance could be given.

Appended to the book are some very interesting observations by J. Weiller dealing with the transition from theories to policies of international trade. According to Weiller, the theory of economic policies must be adjusted to circumstances if adequate results are to be achieved. He illustrates his point in discussing problems of underdeveloped countries, and the questions related to economic growth and touches on the complex problem of fundamental disequilibrium. Weiller is in favor of working out a theory of international economic policies. This reviewer agrees with his comment that contemporary authors complicate schemes which had already been too abstract and transform them into models with numerous variables. Very often such an approach does not prove very helpful in dealing with concrete situations.

The book also has a very stimulating foreword by C. P. Kindleberger who deals mainly with the fields in which future research should concentrate. Of seven items proposed by him the following seem very important: economic integration in world economy; the possibility of applying the theory of foreign trade to underdeveloped countries; effects of foreign trade on income distribution within a country and between countries. Kindleberger rightly stresses that international trade theory needs to be in closer touch with the facts of real life and he concludes that we are only beginning to understand the mechanism of international economy in theory and in practice.

The book by Moret and the comments by Weiller and Kindleberger agree on one item—the time has passed when the theory of international trade focused on so-called equilibrium and outlined which adjustment was needed to restore a disrupted equilibrium. The term "adjustment" was a universal formula which would cover everything. This theory operated in a vacuum, often not examining the political and social complications of any major adjustment and not taking into account important structural changes which many economies had experienced. After all, it would have been possible to produce arguments that an adjustment as postulated by the classical theory could have solved even the postwar dollar problem.

Whether a new general theory of international economics will emerge is perhaps not too important because any such theory would have to be based on many assumptions which would need to be qualified or adjusted so as to fit any real situation. It is much more important to study the multiplicity of problems of the present international economy, to clarify the various concepts and to try to find what might be considered the most acceptable solutions. Moret's study is one of those which can stimulate future research showing the shortcomings of the past and present theories and the ever-growing gap which should be closed.

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International and Interregional Economics. By SEYMOUR E. HARRIS. (New York: McGraw-Hill. 1957. Pp. xiv, 564. $7.00.)

This book might be likened to a Harris tweed. Its quality is high but its finish is rough. It deals with a series of topics such as might be selected for a

graduate course in international economics. One of the topics, the dollar shortage, is treated as a unit and constitutes more than half the book. The other topics appear almost as a series of readings, each one interesting but not thoroughly integrated with what precedes or follows. This may be a "hand-book" but it is not a treatise.

Seven chapters in Part I deal primarily with the classical position on the distribution of output among nations. The quotations from classical authors are pertinent and the author's comments penetrating, but the reader feels adrift until he reaches the summary in Chapter 7. The introduction to Part I does not provide a theme sufficient to hold together the range of material presented. Coverage, of course, is incomplete. For example, in the discussion of the gains from trade, the two-country, two-commodity case is presented and the multicountry case is introduced, but Graham's criticism of Mill's "limbo" ratio is not mentioned. Throughout this section, the imminent presence of Haberler is felt by the reader as it must have been by the author's students.

The six chapters of Part II trace the development of monetary policy from the days of the classical economists to the present. The material for the earlier period is in uneven detail, and when taken together with the author's asides, the reader is left with a cloudy perspective. The two chapters on recent objectives and policy are well done—fresh and astringent.

Part III, one chapter on regional economics, makes perceptive observations on the regional aspects of imports and exports of the United States. As an example of the small irrelevancies that abound in the book, however, this chapter ends with a plea for a revised federal spending policy for New England.

Parts IV and V contain fifteen chapters on the adjustment process, with special reference to the "range of problems subsumed under the generic term 'dollar shortage'." This section might have been a separate book on the subject, and it will be unfortunate if the title of this volume diverts the attention of the student or the general reader from its excellent discussion of the world dollar problem. Chapters 22 and 23 put the United States tariff in proper perspective as an "enemy of the people" but not in number one position. The statement that the philosophy of the trade agreements program should be discarded, with policy emphasis on "more imports, not more imports *and* more exports," seems to be at variance with the emphasis in Part I on the importance of economic allocation of resources, and to place the solution to the dollar shortage in first position as a present-day problem. A summary of the controversy over an increase in the price of gold concludes with arguments opposed to this move that are relevant to an inflationary 1957. Will they be relevant in 1959? An interesting chapter on convertibility suggests that a gold and dollar reserve for Great Britain of $4 to $5 billion, and a rate of exchange "floating" within a 10 to 15 per cent range with stabilization-fund support, might make a restoration of convertibility possible, especially if most of the inconvertible currencies took the step together.

This is not a book to be picked up by a beginner. Parts I-III, particularly,

will serve best as a reference, supplementing organized lectures or readings. Parts IV and V constitute a review of literature and interpretation of facts that require prior acquaintance with the problem of dollar shortage. Properly used, the book will undoubtedly contribute much to other seminars, as it must have to the one out of which it grew.

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Business Finance; Investment and Security Markets; Insurance

Corporate Finance. By ELVIN F. DONALDSON. (New York: Ronald Press. 1957. Pp. x, 876. $7.50.)

This well-organized and readable book gives a wide choice of assignments for a one-semester course. The text is well suited for what might be called the "orthodox course in corporation finance"; it is also suited for a curriculum that does not offer a course in investments, and must combine this material in the one finance course. There are chapters on investment banking, regulation of security issues, buying and selling listed securities (with an account of exchanges, the over-the-counter market, short selling, margin accounts); and a chapter on investment companies, where a careful distinction is made between a closed-end and an open-end company.

A full discussion is given on the market price of closed-end company shares and charges for buying shares of open-end companies. The monthly investment plan of the New York Stock Exchange and dollar averaging are explained. A clear and well-developed chapter is given to privileged subscriptions.

The book is divided into seven parts: Forms of Business Organizations; Corporate Securities; Promotion and Financing Through Securities; Working Capital; Administration of Income; Expansion and Combination; and Readjustment, Reorganization, Receivership and Dissolution.

The author's knowledge in the field of business organization, which made his earlier text *Business Organization and Procedure* a real contribution to the field, helps relate the material, brings in specific examples, and points up the principles underlying the modern business corporation's financial policy.

Partnerships are covered more fully than in most texts, employee ownership of securities is discussed, depreciation according to the Revenue Code of 1954 is covered, under- and overcapitalization are well explained, and inventory valuation is presented. The choice of policy in relation to taxation is emphasized.

Questions and problems are at the end of each chapter and an instructor's manual prepared by the author is available. There is an excellent bibliography of selected readings. It is a long book, but this reviewer is so impressed with its "teachableness" that he is adopting it as his text next semester.

GEORGE W. SANFORD

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Industrial Organization; Government and Business; Industry Studies

Antitrust in Perspective: The Complementary Roles of Rule and Discretion.
By MILTON HANDLER. (New York: Columbia University Press. 1957.
Pp. iii, 151. $3.00.)

This monograph had its origin in three guest lectures delivered in 1956 at
the University of Buffalo School of Law. The first chapter recounts the develop-
ment and application of the "rule of reason", in the interpretation of the
Sherman Antitrust Act. Whereas that act forbids "every contract . . . in re-
straint of trade" and "every . . . attempt to monopolize," it has long since
been construed to forbid only unreasonable restraints, *i.e.*, those with signifi-
cant deleterious consequences. The second chapter deals with the interpreta-
tion of the condition attached to the Clayton Act proscription of tying clauses
and exclusive dealerships "where the effect . . . may be to substantially lessen
competition or tend to create a monopoly in any line of commerce." The
final chapter deals with the interpretation of similar conditions to the pro-
scription of corporate mergers under the original Clayton Act and the 1950
Celler-Kefauver Act.[1] Over half the volume is given over to footnotes which, in
addition to citing court opinions (and dissents), present many illuminating
insights into Handler's views.

Like other attorneys representing antitrust defendants, Handler is an elo-
quent opponent of any extension of the doctrine that certain acts always, or
under specified conditions, are in themselves (per se) unlawful.[2] One of his
principal targets is the position taken by the Supreme Court in the last ten
years that the Clayton Act is violated by any tie-in that foreclosed a sub-
stantial amount of business (the "quantitative substantiality" rule). This, he
argues, is inconsistent with the Court's application of the antitrust statutes in
somewhat similar situations; furthermore, he asserts that on occasion the rule
prevents a practice that would enhance competition.

Handler has similar misgivings with respect to the test of the probable
competitive effects of a merger which the Court employed in determining that
DuPont must divest its stock holdings in General Motors. In this case the
(original) Clayton Act was held to have been violated because the stock
acquisition established a likelihood that competition may be foreclosed in a
substantial share of a substantial market.[3] Handler asserts that this test

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[1] For corporations "subject to the jurisdiction of the Federal Trade Commission" the
Celler-Kefauver Act replaces the merger section of the Clayton Act. The new law applies
to acquisitions of assets as well as stock and phrases somewhat differently the test of
probable competitive effects. The original Clayton Act provisions remain in effect with
respect to corporations such as banks, meat packers and interstate motor carriers.

[2] He concedes that *"per se* doctrines are appropriate where trade practices, like price
fixing, are inherently antithetical to the precepts of a competitive order" (p. 69).

[3] This decision was handed down by a four-member majority, with two members dis-
senting. Justices Clark and Harlan took no part, because of previous connection with the
case. Justice Whittaker was appointed to the court after the case was argued. In the briefs
little attention was given the Clayton Act count. Handler suggests that in view of the
various special circumstances subsequent cases may have a different result.

jeopardizes "thousands of acquisitions that have taken place since 1950," and implies that many such mergers either are without competitive effects or "invigorate the competitive process." Consequently, he calls for "a comprehensive analysis . . . based on an awareness of all relevant market data."

Three observations are in order. A rule of reason has merits, but only in a most general sense can it be said to provide "one essential standard" for the application of the antitrust laws. The major thesis of a rule of reason after all is that each case is unique. Secondly, in the interpretation of the Celler-Kefauver Act, as early as 1952 the Federal Trade Commission adopted the policy urged by Handler. The Department of Justice has been most circumspect in initiating proceedings involving mergers. Finally, the government assumes a very heavy burden in undertaking "a comprehensive analysis based on all relevant market data." A merger case becomes at least as broad as a Sherman Act case,[4] and the market effects are more difficult to assess, since they largely lie in the future. Rarely can the government count on willing witnesses from the trade. Excepting instances where a major supplier acquires one of their competitors, other firms generally welcome mergers. And where they fear the new consolidation, they may feel it poor business to parade their dangers before the world. As a result, the demonstration of probable competitive effects tends to rely heavily on statistical exhibits even though the merit of such evidence rarely is established without lengthy testimony. Thus the preparation and trial of a single merger case typically involves a substantial drain on the professional talent available to each of the parties involved.

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[4] Perhaps broader, since whether the acquired corporation was a failing company frequently is an issue.

Land Economics; Agricultural Economics; Economic Geography; Housing

Urban Land Use Planning. By F. STUART CHAPIN, JR. (New York: Harper and Brothers. 1957. Pp. xv, 397. $8.00.)

Professor Chapin has not written a conventional textbook on urban planning. Rather, he has focused attention on one aspect of planning, namely, land use: the forces affecting the use of land, methods of analyzing such forces, and means by which planners can specifically plan land use. Within these limits it is by far the best book available, and since it is a study concerned primarily with the allocation of a scarce resource to alternative uses, there is much in the book of interest to economists.

In Part I Chapin very carefully and ably classifies the various forces controlling the use of land as economic determinants, socially rooted determinants, and the public interest as a determinant. In the process of classification, he supplies a census of the factors affecting the distribution of land among alternative uses through an examination and review of current literature and

practices. As a result, the section on determinants is as adequate as, and as inadequate as, all previous discussions of the same material. More importantly, the section lacks an integrating theme, and therefore there is no sense of the relationship among land uses. Chapin could have overcome this difficulty by centering discussion around the establishment of hypotheses concerning urban space arrangements or on the testing of hypotheses such as those of Homer Hoyt and Walter Firey. However, he has not chosen to do so and because of this lack of a central theme there is the implicit suggestion that all the determinants are of equal magnitude; yet there is evidence from other sources to indicate that they are not. There are, for example, many instances where the economic determinants of land use have forced major changes in public policies. Whether this is good or bad does not detract from the fact that it is erroneous to believe that all forces affecting the way in which land is used are of equal significance and it is important in any study to suggest the circumstances under which individual factors are likely to dominate.

Granting that economic forces, social forces, and the interests of the public are the determinants of land use, how do you measure these forces? In Part II, Chapin competently reviews various measurement methods. Considerable attention is devoted to the input-output analysis of Leontief as adapted by Isard; to Bogue's work on metropolitan regions; to the use of Census data for defining regions; and to economic base studies. This excellent review is followed by chapters on employment and population with emphasis on regional analysis. The final chapters in this section explore the relationship of transportation to urban land use—a most significant subject which is given only limited treatment—and methods of classifying, mapping, and reporting various types of land use.

In Part III, Chapin finally focuses attention on the key element in land-use planning—the relationship among different types of land use. Again, emphasis is placed on space and location requirements as reported in previous studies rather than on the development of principles or hypotheses. As a guide to the planner in terms of how to map, how much land to allocate for different uses, and so on, the section cannot be improved upon. However, Chapin does not point out to what extent the planner has freedom to select alternative patterns of possible land arrangements and to what extent his choice is limited by the determinants of land use. In the last analysis, if the planner cannot direct land use within fairly broad limits, then an understanding of the determinants of land use will always be sufficient for adequate planning, and there is little need for the development of a general theory of land allocation. By cataloguing the various factors affecting land-use arrangements rather than by analyzing the interrelationship of these forces and possible alternative land-use arrangements, Chapin does not come to grips with this problem.

The lack of attention to some of the fundamental issues of land planning does not, however, destroy the value of the work. Indeed, by emphasizing the necessity for economic and social analysis in urban planning, Chapin has made a very important contribution.

JAMES GILLIES

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The Federal Lands: Their Use and Management. By MARION CLAWSON and
    BURNELL HELD. (Baltimore: Johns Hopkins Press, for Resources for the
    Future, Inc. 1957. Pp. xxi, 501. $8.50.)

Within a generation the disposition of federal lands practically ended;
this was followed by a wave of acquisitions, and there now has been reached
a point of stability. Of incalculable noneconomic worth, the federal lands are
of great direct and indirect economic value. Federal lands amounting to 24
per cent of the nation's total land area (377 million acres) produce $331 mil-
lion of annual gross receipts. This is roughly equal to the authors' estimated
needs of those properties for development, and since 1950 there has been a
positive balance of income over expenditures. The authors contend that the
time has come for placing these active public assets on a more businesslike
management basis. The book plainly declares that a much better job could
be done.

Clawson and Held have written an introduction to a needed re-examination
of the federal lands and the whole system of federal land management. Written
and published under the sponsorship of Resources for the Future, a Ford
Foundation subsidiary, the book is designed for a public generally interested
in the federal lands and the issues concerning them. It will also be informa-
tive to those with special interest in aspects of federal land utilization, be-
cause the book considers the interrelations of the various public lands, their
uses, and their management. Written in nontechnical language and with the
authors' partisanship held in check, the book provides valuable instruction
in the highly complicated maze of the federal lands, their administering
agencies, and the large body of diverse legislation affecting federal land
management.

After a simplified historical review of federal land disposition and custo-
dianship, the authors give detailed consideration to the-many uses of federal
land, influences at work upon policies, and the utilization of the land. The
senior author refers ruefully to the role of administrators, drawing from his
own service as Director of the Bureau of Land Management. Perhaps the
most interesting part of the book is the statistical material and graphic
illustrations, both in the text and the appendix, which are designed to show
the value of federal lands to the Treasury and to the national economy. It is a
calm book free from propaganda but it will not end disputes over public
land administration.

Setting as a goal the efficient utilization of resources, the authors plead for
the adoption of new policies of management comparable to the efficiency of
private development. They maintain that lessons from the past have relevance
for present and future policy, and they suggest that those lessons point to
a coupling of revenues and disbursements in a unified way under the supervi-
sion of a government corporation which would absorb the work of many
scattered agencies. Detailed consideration is given the government corporation
recommendation although alternate plans for reorganization are considered.
The book provides a sound basis for discussing the public issues of federal
land management.

JAMES H. SHIDELER

University of California, Davis

The American Oasis. By EDWARD HIGBEE. (New York: Alfred A. Knopf.
    1957. Pp. xiv, 262. $5.00.)

This book, a general treatment and written in a very readable style, will be
interesting to the lay reader. Although some 21 general works on natural
resources are included in a brief bibliography, neither footnotes nor specific
citations of literature are included. This book is therefore not one that will
prove particularly useful for purposes of research.

The foreword, by Fairfield Osborn, reflects that the work under review,
". . . describes, region by region, the varying characteristics of the productive
lands of the United States, taking full cognizance of the geologic and climatic
influences." In the preface, the author cites as a principal objective, ". . . the
clarification of regional differences in our farming and related soil and water
conservation problems." In a sense, the book satisfies both claims.

The first of the two main parts into which the book is divided gives an
indication of the impact of different social cultures on land-use practices,
including the impact of the various European immigrations on the native
Indian land culture. The limitations of land use by virtue of its climatic and
geologic characteristics are also touched upon. From the outset, the author's
professional competence in agronomy is apparent. The early chapters present
a lucid description of the main elements of soil formation, and discuss several
types of climatic and biological influences that result in soils of differing
agricultural potential.

The second part aims at a statement and explanation of current land uses
and their historical evolution within six broad agricultural regions of the
United States. Regional demarcation is drawn largely on the basis of the
crop and farm types characteristic of portions of the country. In the discus-
sion of regions, the most revealing comparisons are in terms of soil attributes.

In undertaking to deal with present farm types and tillage practices from
the historical standpoint, little attention is devoted to the particular institu-
tions that have been the vehicles of historical influence. Reference, in broad
terms, is made to the Homestead Act, systems of water rights, and early New
England land grants, but in no instance are the particular institutions
analyzed in terms of their functional influence on land use.

Higbee's work is both prescriptive and descriptive, with frequent sugges-
tions of desirable changes in land practices and use. These recommendations
include increasing meadow lands and grazing culture, retirement of eroded
lands, and other measures related to the physical properties of the soil. The
fundamentally economic nature of issues associated with resource use and
management is not brought out. Higbee recognizes certain present practices,
but consideration of the causal interplay of economic and institutional factors
that lie behind existing practices is largely omitted. Issues of taxation, credit,
and producer's organizations have been omitted, and tenure is mentioned only
briefly. The book is descriptive rather than analytical. The recommended
changes in land use are predicated on physical criteria. In failing to deal with
the institutional factors behind individual decisions on land use, the author's
recommendations are general pronouncements, without indication as to
methods of their implementation.

Agricultural and land-use policy cannot be formulated on the basis of physical data alone. It must be recognized that the individual user will make decisions on the basis of costs and returns. Depending on the economic context within which these decisions are made, the result may or may not be in accord with what the physical data indicate. Higbee states that, ". . . it is the potential productivity of land, not money, which is vital to society in the long run." It must be recognized that no less with society than with the individual farmer, the potential productivity of land must be valued, and monetary valuation has emerged as the most practicable method, although studies of uncertainty and time preference reveal that it is certainly not a perfect technique.

This criticism does not imply that the reviewer feels a statement of the physical characteristics of natural resources superfluous. Indeed, these are essential, but an analytical consideration of the social issues of resource-use within the physical context appears to be a prerequisite to policy planning. It is with this in mind that one can state the need for complementary work to that performed by Higbee before policy recommendations such as those appearing in *The American Oasis* can be validly put forward.

MICHAEL F. BREWER

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America's Natural Resources. Edited for the Natural Resources Council of America by CHARLES H. CALLISON. (New York: Ronald Press. 1957. Pp. v, 211. $3.75.)

This volume comprises essays or excerpts from other works by eleven authors and was sponsored by the Natural Resources Council of America. Seven chapters are devoted to particular resources, two to the general relationship between man and his environment, and one each to the statement of an ecological perspective for problems of the use of natural resources and to the need for and nature of a national resources policy. Each chapter ends with a brief list of suggested readings, but there are no other citations.

The seven chapters include a statement on soil by Firman Bear, which deals with selected soil conservation organizations, their history and current operations. Harold Wilm has contributed a chapter on water in which he indicates some of the economic and legal problems entailed in planning for and effecting water management. A chapter on grasslands by David Costello is perhaps the most informative contribution. The configuration of present vegetation is contrasted with the original grasslands of this country. Following a brief section describing selected grasslands uses, certain contemporary management issues are considered, including the use of fire and the grazing of federal lands.

A chapter on forests by Clepper and Besley includes a discussion of the history of the conservation movement within forestry, a brief mention of the size of forest industries in this country, and a statement regarding the contents of a needed national forestry policy. Joseph Shomon has contributed a chapter on wildlife, which gives a comparative picture of species and populations 150 years ago with those of today. It is suggested that the increasing pressure

of human population upon his fellow forms of wildlife will give rise to new measures of control on man's activities rather than the control of wildlife per se. He mentions the possibility of the zoning of land-use to prevent agricultural encroachment on wildlife habitat. A chapter on fish by Hazzard and Voigt focuses upon recreational aspects as opposed to commercial fishing and mentions the impact of certain water uses upon the fish population. In the final chapter on specific resources Harold Zahniser deals with parks and wilderness, presenting a chronological statement of the development of recreational parks and wilderness areas in this country. He then considers, in extremely broad terms, the general private and public benefits derived from them, concluding that the latter are sufficient to warrant specific legislative definition and approval of the wilderness concept rather than permitting it to remain the largely discretionary matter it is today.

Of the remaining four chapters, one by Shirley Allen briefly states the necessity for an ecological approach to the understanding of resource problems. Fairfield Osborn, in another, indicates the general types of problems concerning the use of renewable resources which have been brought about by world population increase. He notes that the conservation movement reflects an awareness of these problems, and that it constitutes the most promising influence upon the future status of our renewable resources. Edward Graham, in a chapter on land use principles and needs, makes four assertions that are reminiscent of natural laws: these define land, state two of its characteristics, and claim a relationship between human actions and natural environment. The concluding chapter by Ira Gabrielson considers the need for a natural resources policy. Little information is given as to the specific nature of such a policy nor the levels of state or national organization at which it would be effected.

This collection of pieces is interesting. It contains useful facts regarding some of our natural resources and several institutions concerned with their use. If its purpose is to point out problems and suggest answers, it may be criticized from several standpoints. The chapters on the individual resources fail to define specific problems of resource use or to identify the conflict of interests from which they arise. The more general chapters, while of a strongly normative character, fail to present any framework within which specific problems can be analyzed. In the absence of this, policy innovations or changes seem to be stated on an intuitive basis.

This volume, like many others, frequently employs the term "conservation," implying an inherently correct and good, albeit obscure, body of logic. If the specific problems to which it is addressed and the criteria adhered to in pursuing their solution are not stated, conservation can hardly be considered either consistent or logical. The term holds emotional and political implications, but until its area of concern and the criteria for decision are specified, it remains of questionable value in assisting the decision-making process.

<div style="text-align: right">MICHAEL F. BREWER</div>

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Labor Economics

The Economics of Discrimination. By GARY S. BECKER. (Chicago: University
of Chicago Press. 1957. Pp. x, 137. $3.50.)

This is an unusual book; not only is it filled with ingenious theorizing,
but the implications of the theory are boldly confronted with facts. Though
the argument is at times quite technical, there is little attempt to engage in
pyrotechnics for their own sake; the author wants theorems only in order
to explain observed phenomena. The intimate relation of theory and observa-
tion has resulted in a book of great vitality on a subject whose interest and
importance are obvious.

Becker begins his analysis by dividing what is received by an individual
in a transaction into two parts; the receipt as ordinarily defined, and an
element of satisfaction or dissatisfaction resulting from the social contact in
which the transaction involves him. Where the contact required by a given
class of transactions is considered a source of dissatisfaction, the net gain in
such transactions is less than otherwise. In such cases, the market demand
price will be lower than when there is no such dissatisfaction. If the contact
is unpleasant to a specific individual, *and he behaves as though he were willing
to pay to avoid such contact,* that individual is said to exhibit a taste for dis-
crimination. (If the contact is pleasant, the individual is said to exhibit a taste
for nepotism.) An individual's taste for discrimination (or nepotism) is re-
flected in a "discrimination coefficient," DC. This discrimination coefficient
(with respect to a given type of transaction) is defined as a fraction of the
money cost of that transaction.

Where an economically dominant section of a community exhibits a taste
for discrimination against a certain class of individuals—say Negroes—in, say,
the employment relation, the ratio of the wage rate for Negro labor services to
that for comparable services rendered by whites will be less than in the absence
of such taste for discrimination. The difference between the ratio of the wage
rates for Negroes and whites in the presence of a taste for discrimination, and
in its absence, is defined (p. 9, especially n. 6) as the "market discrimination
coefficient," MDC. This coefficient is functionally related to the individual
DC's, but it also depends upon the whole range of economic forces that affect
the relative "economic importance" of various members of the community as
this importance relates their individual DC's to the MDC.

Using these concepts, Becker proceeds to argue the following: (1) In a
purely competitive society with two groups of persons, the effect of a taste for
discrimination against one group (as reflected in a positive MDC against that
group) is to reduce the per capita real incomes of both groups (pp. 11-13).
(2) Discrimination will harm the group discriminated against (call it N) more
than the discriminating group, W, "if N is more of an *economic* minority than
W is a *numerical* minority" (pp. 18-19; my italics). Less abstractly, this
means that if N furnishes less labor than W, discrimination will always reduce
N's income relative to W's, but if N furnishes more labor than W, N will be

harmed by discrimination more than W only if the income yielded by all of N's resources (human and nonhuman) would be less, in the absence of discrimination, than the income yielded by all of W's resources. (3) When there is discrimination against N as labor sellers (but not to an important degree as employers) its effect is to raise the wage rate for W as laborers, but to harm W as capitalists by causing them to pay more than otherwise for their labor (pp. 13-14). (4) If the members of N attempt to retaliate, they will lower their own incomes further, and by more than they lower the incomes of the members of W (pp. 23-24).

The first two propositions are both correct and important; the last two are subject to some qualification. Proposition (3) is widely, though not universally, applicable.[1] I consider (4) to be misleading; it is true only in so far as it refers to small groups of persons. For if Negroes *collectively* refuse to deal with discriminators they may raise the marginal cost of discrimination sufficiently to alter the relative demand for their labor services and thereby increase their incomes.[2] The possibility of creating a seller's monopoly of Negro labor services and/or a monopsony of their household purchases does not contradict anything Becker says; his argument assumes pure competition. However, he neglects the fact that proposals for economic retaliation against discriminators almost always envisage collective action. It should, of course, be recognized that the success of collective action by Negroes would depend crucially upon whether collective counteraction was undertaken by whites.

One of the more important theoretical contributions of Becker's analysis is the sharp distinction drawn between discrimination and segregation (pp. 14-16, 48-50). He points out that the mere fact of white distaste for contact with Negroes need not, of itself, inflict economic injury upon the latter, provided Negroes do not have a parallel distaste for contact with one another. The existence of discriminatory tastes could result in Negro employers hiring Negro workers; selling to Negro customers, etc., leading to two identical economies, one Negro and one white. Economies of scale aside, this does not happen because whites have more complementary nonhuman resources per capita. Because of this, Negroes would earn less, under complete segregation,

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[1] It is conceivable that N and W labor are strongly cooperant in production; *i.e.*, the marginal productivity of either type of labor may increase with the amount of the other that is used (capital constant). In this case, discrimination by capitalists against either type of labor may lower the wage rates of both. A case where this might arise is where white labor is restricted to jobs of a supervisory character and requires Negro labor to supervise.

[2] To see this, consider the following possibility: Negroes initially offer white employers the following alternatives: (1) hire us on the same terms as whites or (2) none of us will work for you. If all white employers had a positive DC, none would (assuming competition) hire Negroes and whites on *identical* terms. But those with relatively small DC's would compromise on employment terms involving less discrimination than before. Employment of Negroes would then be more concentrated on employers with relatively low DC's, though there would be fewer Negroes employed. If the elasticity of demand for Negro labor (which depends, *inter alia*, upon the distribution of DC's) was less than one, their income would be increased.

than some of them could earn by working with white capital under discriminatory conditions; therefore, some of them accept discriminatory employment.

However, the pattern of discriminatory tastes is such that Negroes are compressed into job categories of low prestige and income. The relatively few cases of Negroes holding high-status jobs are disproportionately in government or in the segregated portion of the economy where Negroes supervise other Negroes, etc. Becker effectively uses the concept of economic segregation to explain the relative numbers of Negroes in various professions (pp. 71-73), and also why there is a negative association between value added per establishment and the relative number of Negroes employed.[3]

One of Becker's most challenging conclusions (Ch. 9) is that there has been relatively little change in discrimination during the interval 1910-1950. (He has data only for the years 1910, 1940 and 1950.) This conclusion is supported by a comparison of the relative "occupational position" of whites and Negroes in both the North and the South. "Occupational position" is measured by placing all workers in one of three occupational categories (skilled, semiskilled and unskilled) which are assigned numerical values proportional to the average incomes in 1939 of the white workers therein. Treating these numerical values as weights, and applying them to the percentages of workers (in each race) in each occupational category, a weighted average is constructed; this average indicates the occupational position of the racial group in the relevant year.

The relevant indices for both whites and Negroes climbed secularly during 1910-1950, in both North and South. However, in the North, the relative advance of the Negro index (as compared with that for whites) was small; and in the South, the Negro index declined relative to the white during 1910-1940 and, despite a slight relative advance during the 1940's, stood relatively lower in 1950 than in 1910. Consequently, Becker concludes (p. 114) "that almost all the increase in the absolute occupational position of Negroes was caused by forces increasing the position of whites as well. Changes in variables affecting the relative position of Negroes presumably either were minor or offset one another."

However, it is well known that since 1940 the relative median wage and salary earnings of Negroes rose appreciably as compared with those of whites,[4] which suggests that Becker's measure of relative occupational posi-

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[3] Becker's argument on this point (pp. 69-71) may well be correct. However, I would feel happier about it if he had considered the following alternative: there is (1) a positive association between size of establishment and wage rates and (2) a negative association between establishment wage rates and the fraction of the work force that is Negro. These two associations together could produce Becker's result even though his hypothesis were false.

[4] H. M. Miller, *Income of the American People* (New York 1955), Table 51, p. 99. This table shows that in 1951, the median wage or salary income of nonwhites was 4.32 times its 1939 level, while the corresponding figure for whites was 3.01. (Similar relations hold for males and females taken separately.)

tion does not respond to certain types of forces which bear upon the relative earning power of Negroes. One of the more important of these forces is rural-urban migration, which differentially affects Negroes.[5] That is, new rural migrants to cities tend disproportionately to accept jobs at unskilled labor. Thus this type of occupational movement is likely to be undetected by Becker's index. But it would not be seriously denied that such migration reflects occupational improvement for the workers concerned.

Furthermore, because Becker's index uses fixed relative income weights, it fails to reflect the advance in income of unskilled occupations relative to that of others between 1940 and 1950. As Negroes are disproportionately represented in these occupations, the use of fixed-income weights tends to understate the relative economic improvement of Negroes.

A further weakness of Becker's argument on this point is that he presents his index for the South and the non-South separately, but not for the country as a whole. But there has been a secular movement out of the South by Negroes relative to whites, so that the relative nationwide occupational position of Negroes has improved, even though there may have been no change in relative position in either South or non-South separately. This type of relative improvement could take place even though every single individual DC remained unchanged. It may be this latter possibility to which Becker refers when he argues that there is little evidence that economic discrimination against Negroes has diminished.

Regional disequilibrium in the allocation of Negro labor is at least partially responsible for two other phenomena of which Becker speaks: (1) discrimination appears to be greater in the South than elsewhere (pp. 97-103); and (2) it is positively associated in urban areas with the relative number of Negroes (pp. 103-7). Becker "explains" this by asserting (pp. 86-87) that Negroes are more "mobile to the South,"[6] but he does not ask why Negroes do not move where discrimination is less. The tendency of Negroes to be relatively more numerous where their relative incomes are lower could be explained if one or more of the following were the case: (1) a tendency for Negro absolute income to be negatively correlated with the ratio of Negro to white income; (2) for some reason a preference existed among Negroes for those locations where their relative income was comparatively low and (3) a disproportionately large number of Negroes were born in areas where their relative incomes were low and, by migration, were in process of relocating themselves, but that the adjustment had not yet been completed.

[5] Thus the percentage of nonwhites who were farmers and farm managers or farm laborers and foremen shows a much greater decline between 1940 and 1950 than of the population as a whole. United States Census, 1950, Bull. PC-7 No. 2, Table 6, pp. 28-29.

	Total		Nonwhites	
	1940	1950	1940	1950
Farmers and Farm Managers	11.5	8.0	15.0	9.5
Farm Laborers and Foremen	4.3	2.8	11.0	3.9

[6] *I.e.*, the supply curve of Negro labor services in the South lies below the curve referring to the North.

The first of these explanations is inconsistent with well-known facts and the second seems highly implausible, which leaves the third.

The validity of this last explanation follows from the fact that Negroes are known to be concentrated in certain occupational categories (*i.e.*, non-skilled labor) which tends to increase the relative supply to these occupations, and lower their relative wages. Thus, if labor demand and white labor supply were the same in two locations, that location which had the greater proportion of Negroes would have relatively lower wages in those occupational categories in which Negroes were concentrated and the relative income of Negroes would also be lower.[7] Intercity and regional differences in the ratio of Negro to white incomes might thus be explained without assuming the existence of any discrimination. Since, however, it is quite unlikely that Negroes and whites in the same occupations in given communities have the same incomes (the differential incidence of unemployment would almost preclude it), there must be another factor or factors at work. Discrimination is an obvious candidate, and Becker is right in emphasizing it. However, it seems likely that the influence of labor market disequilibrium, which Becker ignores, also plays an important role in explaining these phenomena.

There are several phenomena to which Becker calls attention that require a more detailed theory about the taste for discrimination than anything so far mentioned. One of these is the fact that the relative median income of Negroes tends to decline as we move from persons with less to those with more years of schooling. Becker's rather tentative, through plausible, explanation of this fact is that the aversion of whites to contact with Negroes is greater the higher the occupational status of the Negro (pp. 124-25).

A lesser aversion to Negroes in temporary than in permanent jobs is posited to explain why the ratio of Negro to white mean income was lower, and the relative number of Negroes less, among persons employed 12 months (in 1939) than among those employed less than 12 months (pp. 86-87). These alleged differences in tastes for discrimination seem consistent with what is generally known. However, the difference between the Negro-white mean income differential for persons employed 12 months and that for others is open to an alternative interpretation. Among the persons employed less than 12 months are included not only those who seek work steadily but also seasonal workers and others out of the labor force during part of the year. The differential incidence of unemployment upon Negroes placed a larger fraction of full-time Negro job seekers in the "less than 12 month" class, thereby creating an upward bias in the ratio of Negro to white mean incomes in this category relative to its complement.

The critical tenor of some of the above comments should not be interpreted as reflecting in any way on the general quality of Becker's empirical work; on the contrary, this work is both skillfully and carefully done. As a

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[7] This hypothesis implies (*ceteris paribus*) that the differential for skilled over nonskilled workers will be greater, the greater the proportion of Negroes in the labor force. This is consistent with the gross facts of experience, but it should be tested in detail. This hypothesis, as far as I can see, cannot be derived from Becker's model.

result this is a vital book that will influence thinking on the economics of discrimination for a long time to come.

M. W. REDER

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Contemporary Collective Bargaining in Seven Countries. Edited by ADOLPH STURMTHAL. (Ithaca: New York State School of Industrial and Labor Relations, Cornell University. 1957. Pp. ix, 382. $4.50.)

This is the fourth in a series of studies on international labor problems. It presents the collective bargaining systems of seven Western nations and concludes with the editor's analysis of uniform and diversive trends. Sturmthal assembled an impressive panel of experts and the countries selected serve to demonstrate the many ways and means by which collective bargaining, with its "annex" of government regulations, achieves its ends under widely different institutional patterns.

First, Allan Flanders summarizes the postwar Labor market in Great Britain as "most remarkable for its lack of innovations." He points out that interim war trends largely continued after 1945. By 1950, collective wage determination covered some four-fifths of all employees but its methods and procedures varied from industry to industry and consisted roughly of three types: collective bargaining, bargaining through joint industrial councils, and statutory wage-setting by wage boards. The methods often overlap, however, and are, as Flanders indicates, not so essentially different since statutory wage-setting also follows bargaining procedures. Flanders devotes much of his essay to comparisons of the British situation with continental and American labor markets and finds many significant contrasts that apply, in particular, to the concepts of a national wage policy.

John Inman's report on the Norwegian system of collective bargaining contrasts strongly with the British situation. He explains that the structural development of union and employers' federations provided optimum conditions for a centralized wage policy. Moreover, the control of these associations over their affiliates made it possible for the national wage policy soon to be shifted from government arbitration to a system of private compulsion based on the national federations of workers and employers. Inman analyzes the content of this wage policy with regard to the wage structure and to general wage levels; he reports that union wage restraint assumed at times the form of complete wage stops but indicates that "unofficial" increments of earnings tended to counteract the official wage policy. In spite of its shortcomings and of the inflationary impact before and since the Korean War, the author feels that Norwegian wage policies have more or less achieved their objectives.

Similar to that of Norway, the collective bargaining system in the Netherlands after 1945 was characterized by a high degree of centralization, backed up by the collaboration of its multiple structure of ideological labor and employers' federations. P. S. Pels offers a vivid picture of the objectives, as well as

of the problems of a wage policy on the tripartite basis of organized labor, organized management, and a high degree of government participation. The objectives of this truly national policy were partly given by the needs of reconstruction, partly by the workers' willingness to put employment before wages and, finally, by the desire for "social justice" which implemented meant a planned and rational system of permissible wage differentials. Inman's report on the gradual emergence of such a policy, based on classified skill differentials, and on a national system of job evaluations, is particularly interesting. Yet recent developments indicate that, even in the Netherlands, a second thought seems to question the advisability of too narrow differentials. It seems that, under the tight regime of wage controls, price inflation has been more limited than in other countries, but that real wages have also lagged during part of the period.

Sturmthal's study of the French labor market eliminates all notions as to a more or less "uniform" Continental bargaining system. The author traces the development of collective bargaining conditions in France back to 1919, and points to the apparent weakness of unionism during most of the period as the main reason that government regulations were needed to supplement the actual bargaining strength of the divided, and not too bargaining-conscious, labor movement. Up to 1950, Sturmthal reports, French wages rested largely on public regulations motivated by anti-inflationary considerations. The more recent emphasis on freer collective bargaining leaves the government still with a strong influence.

Clark Kerr examines the collective bargaining system of Western Germany and finds that it shows "few obvious traces" of the past Nazi period. Two important differences separate it from Weimar: the trade union movement has been unified, and compulsory public arbitration has been abolished. Kerr's analysis of postwar policies explores the attitudes of the unions and employers' associations and their relative strength. He finds that organized labor in Germany has made a "choice of political over industrial warfare" (the struggle for codetermination); that members prefer employment security over strikes and aggressive wage policies; that employers' associations are more disciplined than unions; and that the climate of negotiation is still largely that of of a "class society": tense and formal. The German wage structure appears to the author more compressed than the American.

Two chapters of the study deal with postwar Italy. Luisa Sanseveroni traces the evolution of collective bargaining from its Fascist heritage to the middle fifties. The postwar emphasis, the writer reports, was on voluntarism; government regulations pertained mainly to the constitutional provisions (such as the certification of unions or the institution of works councils dealing with grievances, layoffs and other issues), and to a policy of making collective bargaining more effective. Sanseverino outlines the complex structure of bargaining ("sector-wide" and "industry-wide") and the superstructure of wage payments in Italy. On the latter subject, Cesare Vanutelli provides a more detailed analysis. Summarizing his findings, it can be said that cost-of-living allowances and social charges tended to make basic wage rates (based on skills, geographic

regions, etc.) less and less important. A more recent agreement instituted a readjustment allowance in favor of the higher-paid skilled labor groups.

Neil W. Chamberlain provides a comprehensive picture of the American labor market as it has emerged from the secular growth of unionism. He points to the importance of union locals in the American system of collective bargaining; his essay contains a brief survey of the legal and institutional framework of American labor markets and analyzes the wage patterns of postwar bargaining. He reviews the discussions among economists of the impact of union wage policies on prices under full employment.

In Sturmthal's concluding essay on comparative collective bargaining the share of governments as a third party in labor-market policies is evaluated with regard to the diversified conditions of the countries discussed. The author also examines the trends of types of wage differentials and the causes behind their more or less pronounced reduction in various countries. The concept of a "rational wage structure" is examined with regard to its economic implications. Finally, Sturmthal deals with restrained wage policies of unions under full employment, and offers his conclusions for their effectiveness under short-run and long-run conditions of full employment.

The reader will find that the editor and his collaborators have made a notable and significant contribution to the literature of comparative collective bargaining, and, likewise, to the contemporary discussions of wage policies under full employment.

LUDWIG WAGNER

Duquesne University

The Teamsters Union. By ROBERT D. LEITER. (New York: Bookman Associates. 1957. Pp. 304. $5.00.)

One should be forewarned that this book does not deal with "the shame of a union." It is, instead, in the words of the subtitle, concerned with "a study of its economic impact." Professor Leiter admits that most of his research was completed prior to the recent revelations about the Teamsters Union. But even if this were not so, it still would be the type of book that should have been written. For only through an understanding of the economics of the industry, with which the writer concerns himself in part, can one fully understand the present position of the Teamsters Union.

Leiter gives us not only a general history of the Union, but also a picture of its leadership and the various problems that it has faced over the years. He describes the industry as consisting of a large number of small-scale operators who require small amounts of capital in order to start in the trucking business. As a result there is no problem of entry into the industry. In turn, one finds "truck operators poorly trained, inadequately financed, and in some cases irresponsible."

It seems to the reviewer that this short and incomplete summary of the economics of the industry explains a good deal about the Teamsters Union. Although Leiter does not always make the specific connection, many of the problems the union has faced and most of the methods it has employed for

their solution are easily understood when related to the economics of the industry. Let me illustrate. The existence of a large number of small operators automatically makes organization and bargaining difficult. Therefore, the Teamsters Union helped organize employer associations with whom they could negotiate and obviously felt it necessary to obtain the closed shop as a basis for organizing the workers in the industry. Thus, the alleged close connections between the union and employers are quite understandable.

Another illustration: the strength of the union, as Leiter points out, stems from the fact that truck operators are fearful that if a strike took place shippers would provide their own means of transportation. Thus, truck operators are extremely vulnerable. In view of the fact, also, that certain industries have established their own shipping facilities it is to be expected that jurisdictional disputes would develop between the Teamsters Union and other industrial unions. And Leiter devotes a chapter to this subject.

If one accepts the hypothesis that an understanding of the economics of the industry will yield insights into the labor relations problems of that industry, then the book under review can be exceedingly useful as a case study. It seems to the reviewer that such an approach leads to an understanding of the problems found not only in the Teamsters Union but other unions as well. If this hypothesis is acceptable then one might well question whether the installation of "good" men in unions or the enactment of legislation will necessarily eliminate some of the activities of unions which are currently being revealed by Congressional committees.

What is commendable about the book is that it gives us a basis for understanding why certain things have occurred in the Teamsters Union. This does not mean that we condone such activities. But unless one understands the basic causes it is impossible to offer appropriate solutions.

<div style="text-align: right">JACOB J. KAUFMAN</div>

The Pennsylvania State University

Labor in a Growing Economy. By MELVIN W. REDER. (New York: John Wiley & Sons. 1957. Pp. xii, 534. $6.50.)

In assessing the merits of a textbook for review purposes one is usually forced to adopt a number of yardsticks—none of which is satisfactorily objective. Judged by this reviewer's criteria of appeal to students, comparison of presentation and content with alternative labor texts, and "the kind of textbook I should like to use," Professor Reder's addition to an already abundantly stocked field is a 2½-star entry. (In fairness, it should be added, this reviewer's "ideal" 3-star textbook would probably be a publisher's failure.) Apart from a surfeit of parenthetical comments, Reder's style is fluid and easy, making reading a pleasure rather than a chore. The chapters flow smoothly into one another so that one feels that the book comprises an integrated whole. The content and order of presentation are rather traditional, but competently—and in some areas, superlatively—handled.

The book is divided into two major parts, an eight-chapter section concerned with "Unions and Collective Bargaining," followed by seven chapters

on "Employment, Wages, and Income." While this is roughly a division into "institutional" and "theoretical" material, Reder states (p. viii):

> It has been my endeavor, in all chapters of the book, to interweave theoretical and descriptive materials so that they are inseparable. If the book could be said to have a motto, it would be: "No theories without facts, no descriptions without explanations."

This aim is well conceived and well executed.

The institutional section opens with two chapters on the growth of American unionism, emphasizing organization and philosophy. These are good concise surveys, including an analysis of the causes and possible consequences of the AFL-CIO merger. Succeeding chapters on the organizational structure of trade unions and the process of collective bargaining are among the best in the book. Between the lines one can see the shadow of modern organizational, decision-making, and games theory, although the material is presented on a level well suited to the undergraduate. (In fact, the student will not even realize that he has been subjected to theory.) Part II is rounded out by a chapter on the content of collective bargaining, and three chapters on the relation of unions and government. In the latter section major emphasis is placed upon the reasons for enactment, and the consequences, of the Taft-Hartley Act. This is a very well-balanced treatment for the student who approaches the subject with strong biases of either extreme. Throughout this whole section Reder skilfully concentrates on the salient aspects of unionism and industrial relations without getting bogged down in a lot of interesting but irrelevant minutiae.

"The chapters on wage theory were, by far, the most difficult to write," notes Reder in the preface; they are also the most difficult for a reviewer to assess because of the very marked differences in taste of potential instructors. The first three chapters of Part III give a theory of wage determination without reliance on traditional concepts (e.g., marginal productivity) and without diagramatic treatment. These are followed by an excellent discussion of the structure of wages, a chapter on the formal procedures of wage-setting in the firm, and two chapters devoted to the macroeconomic aspects of wages, income and employment. The theoretical discussion is carried on at a rather low level of abstraction. The instructor who wishes to involve his students in a minimum of economic theory can follow the text closely; a good theoretical structure lies behind the rather simplified presentation, and the student is bound to absorb some of it. Alternatively, the instructor who wishes to emphasize labor *economics* can easily supplement the text without being undermined by it. The chapters on wage determination represent a compromise, and although this involves the author in some constraints which may seem unnecessary to many users of the book, it is carried off quite successfully.

The book concludes with three general chapters on causes of income inequality and public and private measures for guaranteeing minimum income standards.

The reservations the reviewer has concerning the theoretical treatment are of a passive nature. Reder's chapters on wage theory are as good as those of

two or three other textbooks, and much better than most. When a good theorist sits down to write an undergraduate textbook, it is cause for rejoicing; it is only unfortunate that some publisher will not make way for a book more particularly aimed at the undergraduate with a modicum of theoretical sophistication.

Judged by the standard of alternative books in this field, Reder has written an undergraduate labor textbook which, in style, presentation and content, must rank with the two or three best ones currently available.

ALLAN CARTTER

Duke University

Labor Economics. By PAUL SULTAN. (New York: Henry Holt & Co. 1957. Pp. xii, 580. $6.50.)

This is an unusual labor economics text. For an economics text, unusual in the extent to which material from other disciplines, such as social psychology and anthropology, are woven into the economic description and analysis. For an introductory text, unusual in the depth of discussion of many issues in organization and collective bargaining. But unusual, too, in the relatively brief treatment of theory (about 80 pages).

Sultan's approach throughout the book is from the side of change and development. In every section there is either an historical development of the subject or heavy historical reference. Some readers may find the historical hand too heavy. But in most cases the historical presentation is such as to induce appreciation of changes in philosophy as well as practices. No attentive student will miss the historical contrasts or the bases of present institutions and ideas.

Some idea of the emphasis on dynamics is given by the titles of the five sections of the book: The Impact of Economic Change and the Evolving Labor Market; Unionism: Determinants and Characteristics of Growth; The Dynamics of Collective Bargaining: The Process and Its Impact; The Public Interest and Industrial Relations: A Review of Labor Legislation; and The Economics of Wages and Employment.

Typically, the discussion of wage theory is heavily historical. After a thorough discussion of mercantilist, classical, and marginal productivity theory, the bogey of unemployment is thrown in. Thus the reader is caught up in the dissatisfactions of twentieth-century economists, Keynesian analysis, and current problems of wage policy. This presentation is novel, for a text, both in plan and execution, and on the whole is effective.

The book's greatest weakness is imbalance and overdetail in the treatment of some issues and institutions. Sultan seems to go to extremes in presenting full description and analysis of certain topics, *e.g.*, labor racketeering, rank-and-file participation, control by the national hierarchy. It is difficult to see how some of this detail will benefit or arouse the interest of the undergraduate beginning the study of labor economics.

Teachers will find in this text much quotation from the source materials for which they have been using books of readings. In addition, the book is heavily documented throughout. There are more footnotes than this reviewer

has seen for many a day in an introductory text. In some of these notes are interesting discussions and quotations, which the student should not miss. (But most students will!) A very annoying practice is the frequent use of second-hand citations accompanying quotations—citations not to the original sources, but to other authors who quoted. There are at least sixty such citations, most of them to easily available originals. Either Sultan is scrupulous in identifying specifically his obligations to other authors for finding good references, or he is negligent in not, himself, going back to the originals.

Sultan's greatest successes are in the breadth of his treatment and the emphasis on dynamics—both of which go well beyond other texts.

<div align="right">HOWARD M. TEAF, JR.</div>

Haverford College

Population; Welfare Programs; Standards of Living

The Economic Status of the Aged. By PETER O. STEINER and ROBERT DORF-MAN. (Berkeley and Los Angeles: University of California Press. 1957. Pp. xx, 296. $5.00.)

Students of social policy in regard to the aged have good reason to be grateful to Professors Steiner and Dorfman. An impressive body of data concerning the labor force and income status of the population aged 65 and over has been made available, based on a special nationwide follow-up study carried out by the Census Bureau at their request in connection with the Current Population Survey of April 1952. Hitherto, our most comprehensive information about the economic status of the aged had been based on the Survey of Old Age and Survivors Insurance Beneficiaries (the so-called "Beneficiary Survey") which was conducted by the Social Security Administration in 1951. Comprehensive as was the information sought in this survey (indeed in some respects it covered more ground than the Steiner-Dorfman study), the group was a selected one and, being limited to beneficiaries, necessarily excluded uncovered persons (a fairly large group in 1951) and aged persons under 65 who had not ceased working or who had benefits withheld under the then relatively low earnings limit.

The data collected in the follow-up study has been analyzed and organized by the authors with scrupulous care, and the value of the book is greatly enhanced by five methodological appendices. It covers the social characteristics of the aged, the labor-force status of men and women, their receipts and needs, their support status, the sources of their receipts and the size and adequacy of their assets.

Some of their findings are perhaps not as novel as the authors imply. The fact that, for example, the aged are not a homogenous group, or that the major problem group consists, and will increasingly consist, of aged women and notably widows will come as no surprise to students of social security policies. In other respects their findings lend support to hypotheses drawn from general observation. Thus it is being increasingly recognized that the social problem of "the aged" is in significant measure a problem of the broken family: the

aged individuals who present the greatest need for social action, whether in the form of socially provided income, housing, or medical care and hospitalization, seem to be the single individuals and the widowed. It is thus of particular interest that the authors find that there is a significant relationship between the presence of a wife and the fact that an older man feels "well enough to work" and that if he is living with his wife he is more likely than other older men to be in the labor force, a difference that persists even when identical age groups of men are compared.

In view of the emphasis currently placed on the desirability of policies and programs directed to overcoming the prejudice against employment of older workers and on special guidance and placement services for the aged, perhaps the most interesting of the findings concerns the reasons for non-labor-force participation on the part of men. In 1952, 59 per cent of men 65 and over were not working. Of this group 24 per cent (13 per cent of all aged men) had ceased work involuntarily because of the provisions of a re-tirement system, age, lay-off or other reasons. But another 60 per cent (33 per cent of all aged men) were not in the labor force because of poor health. Curiously enough, the authors classify "health" as a "voluntary" reason for retirement, a classification which may well give rise to much misunderstanding unless care is taken in interpreting this table (p. 48) which is likely to be given much publicity. On the basis of these findings the authors hold that efforts to increase the employment opportunities of those who are already old are necessarily limited in effectiveness: "What is required is attention to prevention rather than to rehabilitation. This, in turn, implies a shift of focus to younger men" (p. 48). They take, however, a dim view of the impact of medical advances on the economic problems of older people, partly because they anticipate that these may increase not only working life but also life expectancy. They appear to underestimate the probable effect on ability to work of more adequate preventive and curative health services throughout life prior to age 65. Not all those who reported that they "did not feel well enough to work" were suffering from degenerative or chronic age-related diseases.

The authors also expect little success from efforts to mitigate the influence of occupational obsolescence, which they find to be a second major cause of withdrawal from the labor force (frequently in combination with health). Holding that in the future, as in the present, large numbers of the aged will be unable to provide satisfactorily for themselves, they conclude that the comprehensiveness and generosity of programs of public or private support will be a major factor in determining the economic status of the aged.

Yet the present aged are the product of a time when education was much less freely available than it now is, when wages were lower, living conditions less healthy and social services far less highly developed. Thus in planning for the future we must expect that the social, health, occupational and income characteristics of each ten-year cohort will show significant differences from preceding cohorts. It is probably too much to expect that the authors can be persuaded to undertake the time-consuming task of repeating their

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General Economics; Methodology

BERLE, A. A., JR. *Economic power and the free society*. (New York: Fund for the Republic. 1957. Pp. 20.)

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PAPANDREOU, A. G. *Economics as a science*. (Philadelphia: Lippincott. 1958. Pp. xi, 148.)

PEACOCK, A. T., TURVEY, R., STOLPER, W. F., HENDERSON, E., ed. *International economic papers no. 7*. Translations prepared for the International Economic Association. (New York and London: Macmillan. 1957. Pp. 184. $3.50.)

STONIER, A. W. AND HAGUE, D. C. *A textbook of economic theory*. 2nd ed. (New York: Longmans, Green. 1957. Pp. x, 513. $5.)

WAFFENSCHMIDT, W. G. *Wirtschaftsmechanik*. (Stuttgart: W. Kohlhammer. 1957.)

United States economic problems in the next twenty years. Fifty distinguished scholars and leaders in public affairs present their views. (New York: Com. for Econ. Development. 1957.)

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

BENTZEL, R. *Den privata konsumtionen i Sverige 1931-65*. (Stockholm: Industriens Utredningsinstitut. 1957. Pp. xvi, 475.)

This book is a modest counterpart to Stone's studies of consumption in Britain. On the basis of consumption expenditures in the period 1931-1955, conventional regressions have been computed with electronic computing machines. The chief purpose was to produce a forecast for 1965. There is some discussion of consumption theory.

BOMBACH, G. AND GABRIEL, S. L. *Löhne und Preise*. (Darmstadt: C. W. Leske. 1957.)

BORCHERS, H. *Das Abstraktionsproblem bei David Ricardo*. (Jena: Gustav Fischer. 1929.)

BÖSSMANN, E. *Probleme einer dynamischen Theorie der Konsumfunktion*. (Berlin: Duncker & Humbolt. 1957.)

CANNAN, E., ed. *Lectures on justice, police, revenue and arms—delivered in the University of Glasgow by Adam Smith, reported by a student in 1763*. Reprints of Econ. Classics. (New York: Kelley & Millman. 1956. Pp. xxxix, 293. $7.50.)

CROSARA, A. A. *Il guidizio economico, la proprietà e il tempo nella questione sociale (capitale, interesse e ripartizione dei consumi)*. 2nd ed. (Rome: Ed. Studium. 1957. Pp. viii, 235. L. 2000.)

DORFMAN, R. SAMUELSON, P. A. AND SOLOW, R. M. *Linear programming and economic analysis*. (New York: McGraw-Hill. 1958. Pp. ix, 527. $10.)

FÖRSTNER, K. AND HENN, R. *Dynamische Produktionstheorie und Lineare Programmierung*. (Meisenheim/Glan: Anton Hain. 1957. Pp. 125. DM 14,50.)

HENDERSON, H. *Supply and demand*. Cambridge Econ. Handbooks reprint, originally pub. 1921. (Chicago: Univ. of Chicago Press. 1958. Pp. x., 142. $2.25.)

JONES, R. *An essay on the distribution of wealth, and on the sources of taxation*. Reprints of Econ. Classics. (New York: Kelley & Millman. 1956. Pp. xlix, 378. $8.50.)

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LIPSCHITZ, E. *Die theoretischen Grundlagen David Ricardos in Lichte des Briefwechsels*. (Berlin: Duncker & Humblot. 1957. Pp. 214. DM 18.)

ROBBINS, L. *Robert Torrens and the evolution of classical economics*. (New York: St. Martin's. London: Macmillan. 1958. Pp. xiii, 367. $7.50.)

ROBERTSON, D. H. *Lectures on economic principles*. Vol. 1 (London: Staples. New York: John de Graff, distrib. 1957. Pp. 172. $3.50.)

SHACKLE, G. L. S. *Time in economics*. F. de Vries lectures delivered in Amsterdam in 1957. (Amsterdam: North-Holland. 1958. Pp. 111. f 6.50.)
The lectures are concerned with the following aspects of the general subject: (1) the complex concept of economic time; (2) decision and uncertainty; (3) some dynamic mechanisms (including investment, the multiplier, and the rate of interest). There is an appendix on the limitations of economic theory.

SURIYAKUMARAN, C. *The economics of full employment in agricultural countries, with special reference to India and Ceylon*. (Kandy, Ceylon: K. V. G. de Silva. 1957. Pp. 307. Rs 12/50.)

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The articles included have not been previously published in English. "The papers have been divided into four groups. The first contains two early lectures, one explaining Wicksell's views on economics in general, the other giving a summary of his monetary theory. The second group consists of three papers containing the kernel of Wicksell's contributions to the theory of production and distribution. . . . Some articles . . . on the works of other well-known economists of his time have been put together as a third group. . . . The last group contains some papers on foreign trade problems, written by Wicksell after the end of the First World War." (From the editor's preface.)

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"There seemed to me a need for a book showing how the Sahara enriched the Carthaginian and bewildered the Roman; how in later times the greater caravan routes, linking the sophisticated cities of the north with the great markets and modest seats of learning of the south, not only influenced the course of events in Barbary, and even beyond, but sometimes determined it; how, all down the centuries, Berbers and Arabs, Jews and Christians, never ceased to draw on the wealth and industry of the Sudanese." (From the preface.)

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This little volume is a staff report of the National Planning Association. The scope of the report, which is designed to be broadly informative for the general reader, both in this country and in foreign countries, who seeks to understand the functioning of our economy, is clearly indicated in the title of the pamphlet. Part I explains how the American economy achieves its great productivity and high living standards, and Part II directs attention to its more important prevailing problems and general future prospects. In the progress aspect of the analysis, the operative factors which are examined include natural resources, labor, business management, research and technology, capital, government, and the values and institutional arrangements which have conditioned these factors and have rendered them outstandingly fruitful. In the analysis of the crucial problems which have fashioned its changing character, the examination includes the issue of balance in economic growth in relation to price stability and full employment, of advances in living standards in relation to the distribution of income, of concentration in economic power in relation to the maintenance of free competition and democratic ideals, and of international economic programs and policies in relation to the needs of underdeveloped countries and their political as well as economic significance. The concluding chapter, dealing with the nature and outlook of the American economy, emphasizes the fact that it "does not operate in accord with any of the 'pure' laws of laissez faire capitalism or of socialism," that it has succeeded in reconciling "the Hamiltonian idea of economic progress with the Jeffersonian ideal of individual self-reliance," and that if war can be avoided, "the United States has the possibility of achieving material abundance for all within the next decade or two." This exposition, largely confined to the actual accomplishments of the economy, is supported by simple but helpful charts; and an appendix contains, for convenient reference, a useful group of relevant statistical tables. The National Planning Association has rendered an important service in providing an excellent brief analysis and appraisal of the American economy.

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NOTES

A nominating committee consisting of O. H. Brownlee, Gerhard Colm, Frederick H. Harbison, J. Richard Huber, Margaret F. Myers and Simon Kuznets, chairman, has submitted the following slate of nominees for 1959 officers of the American Economic Association:

President: Arthur F. Burns

Vice President:
  Kenneth E. Boulding
  Raymond T. Bowman
  Oskar Morgenstern
  Lloyd G. Reynolds

Executive Committee:
  George L. Bach
  Howard R. Bowen
  Abram L. Harris
  Edward S. Shaw

Representative, Social Science Research Council:
  Gardner Ackley

Representative, American Council of Learned Societies:
  Frank H. Knight

The annual meeting of the Association will be held at the Palmer House, Chicago, Illinois, December 27-29, 1958.

REPORT OF SURVEY OF MEMBERSHIP WITH REGARD TO AMERICAN ECONOMIC REVIEW

In September 1957 a questionnaire was sent by the managing editor to two samples of the membership of the Association. The purpose was to learn what use was being made of the various features of the *Review* and to elicit suggestions as to ways in which the journal might be of further service. The results of the survey are summarized as Exhibit I in connection with the report of the managing editor in the May 1958 (Proceedings) issue.

In general the responses indicate that the "service" sections of the *Review* (book reviews, titles of new books, titles of periodical articles, etc.) are widely used, that review and expositional articles are regarded as useful features and that the leading articles and communications are ranked high, in general quality and usefulness, relative to those in other general economics journals.

In response to an invitation to offer suggestions, however, many replies stressed ways in which the articles in the *Review* might be improved. Apart from a few criticisms of the quality of the articles, the most frequently recurring suggestions were that the *Review* publish fewer specialized, technical and theoretical papers, and more papers of general interest, particularly on current problems of economic policy. Altogether, 65 respondents (out of 322) made suggestions of this sort. In addition, 36 respondents (including 11 of the 65 just mentioned) urged acceptance of fewer (or no) articles involving mathematical analysis.

The members of the board of editors believe that the point of view reflected in these particular replies is sufficiently widespread to call for some explanation of the board's present policy:

1. Since the primary purpose of the articles section of the journal is to encourage, and to

report the results of, economic research, most of the papers published will necessarily be technical, and often theoretical.

2. There is, however, no policy or prejudice on the part of the board against articles on current economic problems. In fact many of the articles now published are concerned, directly or indirectly, with such problems. (Many papers on current economic problems also appear regularly in the annual Proceedings volume of the *Review*.) The editors would warmly welcome the submission of a larger number of high-quality manuscripts in this category.

3. It should be remembered that, except for a few review, survey and expositional articles, the editors do not "commission" papers on particular topics. We seek to select for publication the best papers of those voluntarily submitted. (By contrast, nearly all of the papers published in the Proceedings volume are commissioned in connection with the program of the annual meeting.) Hence the articles included in the four regular issues of the *Review* reflect the kinds of research on which members of the profession are engaged and the subjects in which they are interested.

4. The board would hesitate to change its policy of selecting leading articles from those submitted to one of commissioning articles in order to obtain more nontechnical papers or papers of broader interest. First, it believes that the primary emphasis in the four regular issues of the *Review* should continue to be on the stimulation of fundamental research and the reporting of its results. Second, competition for the opportunity of publication in the *Review* is more likely to result in high-quality papers than when articles are commissioned. Third, a large annual publication of the Association is already made up of papers that have, with a very few exceptions, been commissioned.

5. The objection to articles involving mathematical analysis is entirely understandable. Many, particularly the older, members of the profession cannot, or cannot easily, read them. On the other hand, an important segment of the profession does find mathematical techniques useful; and the editors do not believe that the official journal of the Association should be closed to them. However, the present policy is to: (1) limit somewhat the number of such articles accepted; (2) require that the mathematics should not be at an advanced level; (3) urge authors of such articles to make clear, for nonmathematical readers, the implications of the mathematical sections of the analysis. We generally do not accept mathematical articles if it is clear that the analysis could just as well be carried through without the use of mathematics.

REGIONAL FACULTY RESEARCH SEMINARS IN ECONOMICS

The Ford Foundation is initiating seven regional research seminars in economics in the summer of 1958. Participating in each seminar are ten selected faculty members teaching economics or business subjects at liberal arts colleges or other institutions of higher learning which do not offer a doctorate in economics. It is the purpose of this program to enhance the effectiveness of teaching and to encourage the preparation of research studies of significance.

The following seminars will be held: Government in the Economy, conducted by Charles E. Lindblom, at Yale University; Industrial Organization and Economic Theory, Fritz Machlup, Johns Hopkins University; International Economic Problems and Policies, Don D. Humphrey, Duke University; Recent Developments in the Theory of the Firm, Kenneth E. Boulding, University of Michigan; Public Finance and Fiscal Policy, Howard G. Schaller, Tulane University; Market Structure, Business Behavior, and Public Regulation, Francis M. Boddy, University of Minnesota; Money and Income, Lorie Tarshis and Edward S. Shaw, Stanford University.

FELLOWSHIP PROGRAM IN EAST ASIAN STUDIES FOR COLLEGE TEACHERS AT HARVARD UNIVERSITY

Six liberal arts college professors of social sciences and humanities have been awarded fellowships jointly by their institutions and by Harvard University's Center for East Asian Studies for the academic year 1958-59. They will undertake post-doctoral programs of studies which will enable them to offer new courses on Asia or to enrich existing ones with

more content about Eastern Asia. Persons interested in applying should write to Dr. Allan B. Cole, Curator, Fellowships in East Asian Studies, Center for East Asian Studies, Harvard University, 16 Dunster Street, Cambridge 38, Massachusetts.

. EXPERIMENT IN TEACHING

Columbia University's Graduate School of Business has undertaken a project to explore alternative methods of incorporating new valuable techniques into current business course offerings. It has been found that many useful new concepts and techniques are now untaught because faculty members and students lack the necessary technical training. A pilot project, under the direction of Alfred R. Oxenfeldt, professor of marketing, explores the use of a "team" to prepare separate materials for teachers and students in the field of pricing; these materials are intended to provide access to recent pricing developments such as the theory of games, mathematical probability models dealing with competitive bidding, marginal preference models which illuminate the nature of demand, and recent developments in psychology which help to explain businessmen's pricing objectives and consumers' responses to changes in prices. The team that will prepare these materials will include specialists in the fields of operations research and psychology and others who prove to be required.

Announcements

Section K—Social and Economic Sciences—of the American Association for the Advancement of Science will hold sessions for contributed papers at the annual meeting of the AAAS in Washington, D.C., December 26-31, 1958. Association members interested in presenting a paper at these sessions should forward titles and abstracts not later than October 10 to the secretary of Section K, Donald P. Ray, National Academy of Economics and Political Science, George Washington University, Washington 6, D.C.

The Fund for Social Analysis, which has been recently established, is offering in 1958 a limited number of grants-in-aid for studies of problems posed by Marxist theory and its application. Grants will ordinarily range from $500 to $3,000 and may be requested for an entire project, or for any part, or for assistance in research, editing or publication. Address the Corresponding Secretary, The Fund for Social Analysis, Room 2800, 165 Broadway, New York 6, N.Y.

The next closing date for receipt of proposals in the Social Science Research Program of the National Science Foundation is October 1, 1958. Proposals received by that date will be evaluated in the fall. Approved grants will be activated in time for work to begin in the second semester or the summer of 1959. Proposals received after October 1, 1958 will be reviewed following the winter closing date of February 1, 1959, with activation of approved grants in the summer and fall of 1959. Inquiries should be addressed to National Science Foundation, Washington 25, D.C.

The Metropolitan Economic Association has elected the following officers for the year 1958-59: Robert D. Leiter, The City College of New York, president; Norris O. Johnson, The First National City Bank of New York, vice president; Harold B. Ehrlich, Ira Haupt & Co., secretary.

The Midwest Economics Association has elected the following officers for the year 1958-59: Harold M. Groves, University of Wisconsin, president; Ben W. Lewis, Oberlin College, president-elect; Victor E. Smith, Michigan State University, first vice president; Joseph T. McKenna, St. Louis University, second vice president; C. Woody Thompson, State University of Iowa, secretary-treasurer.

Visiting Foreign Scholars

Lachlan McGregor of the University of Melbourne is visiting lecturer in economic

history at the University of Illinois. He will spend the coming academic year in Cambridge University, England.

A. W. Stonier of University College, London, will be visiting professor of economics at Duke University for the first semester 1958-59.

Brinley Thomas of Cardiff, Wales, will be visiting professor of economics at the University of Washington during the summer of 1958.

Herman Wold of the University of Uppsala will be visiting professor of economics at Columbia University in the academic year 1958-59.

Research Professorships

The Brookings Institution has awarded National Research professorships for 1958-1959 to the following: Hugh G. J. Aitken, University of California, Riverside; William B. Gates, Jr., Williams College; Marion H. Gillim, Barnard College; James R. Nelson, Amherst College; Richard W. Perlman, Adelphi College; Jozo Tomasevich, San Francisco State College.

Deaths

Norman S. Buchanan, director of the Social Sciences Division of the Rockefeller Foundation, died April 25, 1958.

Michael Dorizas, assistant professor in the department of Geography and Industry, Wharton School, University of Pennsylvania, died October 27, 1957.

Russell S. Grady, associate professor of commerce at the University of Kentucky, died January 6, 1958.

Harold W. Guest died July 8, 1957.

George L. Leffler, professor of finance at Pennsylvania State University, died February 14, 1958.

Sidney L. Miller died November 9, 1957.

Frank Parker, professor of finance in the Wharton School of Finance and Commerce, University of Pennsylvania, died February 14, 1958.

Charles F. Roos, president of the Econometric Institute, Inc., died January 7, 1958.

Walter W. Stewart, retired from the Institute for Advanced Study, and former member of the President's Council of Economic Advisers, died March 1958.

Appointments and Resignations

Arthur J. Altmeyer, former U. S. Commissioner of Social Security, has been reappointed lecturer in economics and social security at the University of Wisconsin for the coming academic year.

Robert E. Asher has been appointed to the senior staff of the Brookings Institution.

Paul H. Banner has resigned from the Senate Subcommittee on Antitrust and Monopoly, and is now chairman, Research Committee, Southwestern and Western Trunk Line Railroads, St. Louis, Missouri.

Michael H. Belshaw has been appointed assistant professor of economics at Douglass College, Rutgers University.

William B. Bentsen, formerly at the University of Wisconsin and this past year at the London School of Economics, has been appointed assistant professor of economics at Beloit College.

Carl Biven has been appointed research associate in the School of Business Administration, Emory University.

Rudolph C. Blitz of Northwestern University has been appointed assistant professor of economics at Vanderbilt University.

Karl N. Bopp has resigned from the department of finance in the Wharton School, University of Pennsylvania.

William G. Bowen has been appointed lecturer in economics at Princeton University.

Ted R. Brannon, recently with Aramco in Saudi Arabia, has been appointed lecturer in management at the University of Florida.

Andrew F. Brimmer has been appointed assistant professor of economics at Michigan State University.

Robert O. Brown has been appointed assistant professor of commerce at the University of Kentucky.

Frederick M. Burgess has been lecturer in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term 1957-58.

Meyer L. Burstein has been appointed assistant professor of economics at Northwestern University.

Phillip D. Cagan, formerly of the University of Chicago, has been appointed associate professor of economics at Brown University.

John E. Candelet has been promoted to associate professor of economics at Trinity College.

Reynold E. Carlson of the International Bank for Reconstruction and Development has accepted an appointment as director, Graduate Training Program in Economic Development, and professor of economics at Vanderbilt University

Pao L. Cheng of Michigan State University has been appointed associate professor of business finance in the School of Business Administration, University of Massachusetts.

Richard M. Cisek has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term 1957-58.

Willard W. Cochrane of the University of Minnesota will be visiting professor in the department of economics of the University of Chicago for the coming academic year.

Jerome B. Cohen has been appointed assistant dean of graduate studies in the Baruch School of Business, The City College.

Leslie Cookenboo, Jr. has resigned from the Rice Institute to accept a position with the Richfield Oil Corporation.

Frederick B. Cornish has been instructor in the department of geography and industry in the Wharton School, University of Pennsylvania in the spring term.

Clifton B. Cox has been appointed visiting research professor of business administration at Harvard University Graduate School of Business Administration.

John S. deBeers has resigned as director of the economic research department of the Government Development Bank for Puerto Rico and will engage in economic consulting in Washington, D.C.

Max W. Fletcher has been appointed assistant professor of economics in the College of Business Administration, University of Idaho.

Harold G. Fraine will return to the University of Wisconsin in the fall after a year's teaching in Indonesia. He will resume duties as director of a two-year research project sponsored by the Life Insurance Association of New York City.

M. Mason Gaffney has resigned from North Carolina State College to accept a position as associate professor of agricultural economics at the University of Missouri.

Hugh G. Hansen has resigned from the State University of Iowa to accept a position as chief of the Census of Irrigation, Bureau of the Census, Washington, D.C.

Joseph R. Hartley has been appointed assistant professor of transportation in the School of Business, Indiana University.

Millard Hastay, formerly of The City College and the National Bureau of Economic Research, has been appointed associate professor of economics at the State College of Washington.

O. E. Heskin has resigned from the University of Florida to accept a permanent position with the State Department in foreign service.

Bert G. Hickman has been appointed to the senior staff of the Brookings Institution.

Donald F. Istvan has been appointed faculty lecturer in accounting in the School of Business, Indiana University.

Robert K. Jaedicke has been appointed assistant professor in business administration at the Harvard University Graduate School of Business Administration.

Ronald K. Jones has resigned as research associate in business administration at the Harvard University Graduate School of Business Administration.

A. D. H. Kaplan has recently been on a lecture tour in India. He is retiring from the senior staff of the Brookings Institution this month.

Richard D. Karfunkle, formerly of Pennsylvania State University, has been appointed staff economist in the operations research group of Chas. Pfizer and Co., Inc.

Robert M. Kaufman has resigned as senior economist, New York State Division of Housing, to accept an appointment as attorney, Department of Justice, Antitrust Division.

Richard deR. Kip has resigned as assistant professor in the department of insurance, Wharton School, University of Pennsylvania.

Arthur J. Kirsch has been appointed assistant professor of economics at Long Beach State College.

George Kleiner of the University of Illinois has spent the year on a Fulbright lectureship at the International Christian University, Tokyo.

Alex Kondonassis has been appointed economic historian at the University of Oklahoma.

Walter Krause, formerly with the International Cooperation Administration, has been appointed professor of economics at the State University of Iowa.

Harold Kuhn of Bryn Mawr College was visiting associate professor of economics at Princeton University during the second term of the current academic year.

Robert L. Lampman of the University of Washington has been appointed professor of economics at the University of Wisconsin.

Carl A. Lawrence has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term.

Louis Lefeber has been appointed visiting assistant professor of economics at Stanford University for the summer quarter.

Robert Lekachman has been promoted to associate professor of economics and has been named acting executive officer of the department of economics, Barnard College. He is also serving as a consultant to the Fund for the Republic.

Abba P. Lerner has been appointed visiting distinguished professor of economics at Michigan State University, for the summer session.

Richard W. Lindholm has resigned from Michigan State University to become dean of the College of Business Administration at the University of Oregon.

Jan B. Luytjes has been instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, in the spring term.

Jesse W. Markham of Princeton University has a Ford Foundation research professorship for the year 1958-59.

Jacob Marschak, of Yale University, will be a Ford distinguished research professor in the Institute of Industrial Administration, Carnegie Institute of Technology, for the academic year 1958-59.

Bert C. McCammon, Jr. has been promoted to assistant professor of marketing in the School of Business, University of Indiana.

Robert E. McGarrah has been appointed associate professor of business administration at the Harvard University Graduate School of Business Administration.

Robert B. McKersie has resigned as research associate in business administration at the Harvard University Graduate School of Business Administration.

Nancy McKinney has been appointed instructor in economics at Carnegie Institute of Technology.

Walter J. Mead, after a year's leave with the Committee for Economic Development, has been appointed assistant professor of economics at the University of California at Santa Barbara.

Merton H. Miller has been promoted to associate professor of economics and industrial administration at Carnegie Institute of Technology.

Roger F. Miller of the University of California, Berkeley, has been appointed assistant professor of economics at the University of Wisconsin.

Basil Moore, formerly of The Johns Hopkins University, has been appointed assistant professor of economics at Wesleyan University.

Theodore Morgan of the University of Wisconsin will join the faculty in economics at Gadjah Mada University, Jogjakarta, Indonesia, this fall.

Richard F. Muth will be on leave from Resources for the Future to be visiting associate professor of economics at Vanderbilt University for the coming academic year.

Lawrence Nabers has been promoted to associate professor of economics at the University of Utah.

William H. Nicholls has been named chairman of the department of economics and business administration at Vanderbilt University.

Howard Nicholson of the University of Virginia has been appointed associate professor of economics at Clark University.

Miklos S. Nicolson is chairman of the business administration division of Phillips University, Enid, Oklahoma.

Ragnar Nurkse will hold the research professorship endowed by the Ford Foundation at Columbia University in the academic year 1958-59.

David W. Nylen has been appointed research associate in business administration at Harvard University Graduate School of Business Administration.

James R. O'Connor has been appointed instructor in economics at Barnard College.

Ronald R. Olsen of the University of Ohio has been appointed assistant professor of labor economics in the department of economics at the University of Kansas.

Guy H. Orcutt of Harvard University has been appointed professor of economics at the University of Wisconsin.

Gardner Patterson has been visiting professor in the department of finance in the Wharton School, University of Pennsylvania, in the spring term. Effective July 1, he will succeed Dana G. Munro as director of the Woodrow Wilson School of Public and International Affairs, Princeton University.

Vernon W. Pherson has been appointed research associate in business administration at Harvard University Graduate School of Business Administration.

Joseph Pincus has resigned from the Division of Research and Analysis for American Republics, State Department, to take a position as tariff adviser, U.S. Operations Mission, Tegucigalpa, Honduras.

J. Stuart Prentice, professor emeritus of Middlebury College, has been visiting professor of economics at Dickinson College, Carlisle, Pa., in the past year.

Elton Rayack has been promoted to assistant professor of economics at the Pennsylvania State University.

Henry Rosovsky has been appointed acting assistant professor of economics at the University of California, Berkeley, for the academic year 1958-59.

Gerhard N. Rostvold has been promoted to associate professor of economics at Pomona College.

Julius Rubin has been appointed instructor in economics at Columbia University for the academic year 1958-59.

Ralph Russell has transferred his farm management consulting service to Connecticut and is working with dairy farmers in New Haven County.

Theodor Schuchat has been appointed executive director of Social Legislation Information Service, Washington, D.C.

Theodore W. Schultz of the University of Chicago is serving as chairman of the Research Advisory Board of the Committee for Economic Development.

Tibor Scitovsky has been appointed professor of economics at the University of California, Berkeley, for the academic year 1958-59.

Stanley J. Shapiro was instructor in the department of marketing and foreign commerce in the Wharton School, University of Pennsylvania, during the spring term.

B. M. Stanfield, who is retired from Columbia University, is at Hobart and William Smith Colleges on a grant from the John Whitney Foundation. He is to be visiting professor at the University of Hawaii during the summer session.

Charles Stein, Jr. has been appointed lecturer in business administration at Harvard University Graduate School of Business Administration.

W. J. Stenason has been appointed director of economic research, Canadian Pacific Railway Company, Montreal.

Richard W. Stevens has been instructor in the department of insurance in the Wharton School, University of Pennsylvania, during the spring term.

George W. Stocking has resigned from the chairmanship of the department of economics and business administration at Vanderbilt University. He will continue as director of Vanderbilt's Institute of Research in the Social Sciences.

Robert H. Stroup has been appointed acting director of the Bureau of Business Research, University of Kentucky.

Joseph Taffet has been promoted to assistant professor of economics at The City College.

Frank A. Thornton has been promoted to assistant professor of economics at The City College.

William O. Thweatt has resigned from the Graduate Training Program in Economic Development, Vanderbilt University, to take a position as economic adviser to the International Cooperation Administration mission in Nepal.

Roger C. Van Tassel has been promoted to associate professor of economics at Clark University.

Jacob Viner of Princeton University has been selected by the American Council of Learned Societies to receive one of their Special Awards for distinguished scholarly accomplishments in the humanities and social sciences for the 1958-59 academic year.

Stanislaw S. Wasowski has been appointed assistant professor of economics at Duquesne University.

Charles C. Withers has been appointed lecturer on business administration at Harvard University Graduate School of Business Administration.

Harry D. Wolfe, who has been visiting professor of marketing at Columbia University and managing director of Behavioral Science Research Inc., will join the faculty of the School of Commerce, University of Wisconsin, in the fall.

James S. Worley of Princeton University has been appointed assistant professor of economics at Vanderbilt University.

John R. Yeager has been appointed lecturer on business administration at Harvard University Graduate School of Business Administration.

William P. Yohe of the University of Michigan has been appointed assistant professor of economics at Duke University.

Edward Zabel of the RAND Corporation has been appointed assistant professor at the University of Rochester.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the *Review* will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the *Review* are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

Social studies: Associate professor or professor of social studies (depending on experience and academic background), beginning September 1, 1958; salary $5,976 or $7,206. Master's degree with major training in economics required. Preparation should include several of the following areas: economic theory, consumer economics, labor problems, international trade, industrial organization, and international economic problems. At least thirty credit hours beyond the master's level are expected and promotion in rank would require considerable organized study at the doctoral level. Write: State University Teachers College, Department of Social Studies, Plattsburgh, N.Y.

Accounting and statistics: A small college in the Southwest will have a vacancy in September, 1958. Applicant must be qualified to teach advanced accounting, statistics, and related courses. Salary and rank depend upon training and experience. Write: Chairman, Department of Business Administration and Economics, Austin College, Sherman, Texas.

Economics: Ph.D. with a background in forecasting, general economic activity trends, gross national product. Especially desirable would be experience as economist for an airline.

Cost analyst: At least 4 years of experience with broad-brush cost analysis, such as preparation of cost inputs for weapons systems studies.

Cost analyst: Several years of experience with computer costs, with emphasis on nonmoney costs and frame time analysis. Experience with real time is desirable. Salaries commensurate with educational background and experience. These three positions are with a nonprofit research organization and carry outstanding fringe benefits. Please send detailed résumé to: Robert W. Frost, System Development Corporation, 2400 Colorado Avenue, Santa Monica, California.

Business administration: Openings in school of business administration in Washington, D.C., from assistant to full professorships. Primary interest in men of ability and initiative, willing to work hard to build rewarding career. Combination of teaching, administration, and work with business groups. Send detailed résumé of education and experience.   P202

Management: Staff member wanted for fall, 1958, in private metropolitan university in Middle West. Must have Ph.D. Salary and rank depend on previous experience, publications, etc. Full-time salary can be supplemented by summer teaching. Opportunity exists for consulting work in the community.   P206

Economists Available for Positions

Economic principles, monetary economics, analysis, international economics, trade policy: Man, 45; Ph.D. British and U.S. education; background in history of economic doctrine and economic history. Teaching experience; knowledge of French and Spanish; several articles published and others in preparation. Eager to pursue research and further develop courses now being given.   E707

Industrial management, accounting, business administration, personnel management, industrial relations, economics, labor economics: Man, 40, married; B.A., National Peking University; M.B.A., Wharton School, University of Pennsylvania; completed Ph.D. requirements at University of Chicago; C.P.A. Six years of college teaching experience; 1 year as department head at Midwest university. Interested in writing with a keen mind of value judgment; book under way. Currently visiting member of Midwest state college faculty. Desires permanent teaching position with academic challenge and leadership. Prefers Central or Midwest; will consider East or other areas. Available September, 1958.   E717

Labor economics and personnel relations, comparative economic systems, economic principles: Man, 34, married; Ph.D., Columbia University. Business research experience; 5 years of teaching experience. Publications in labor economics and collective bargaining. Currently teaching at a Southern university. Desires position with major work in labor courses. Interested in teaching position in East or South.   E720

Accounting, statistics: Man, 47; J.D., Ph.D. Sixteen years of teaching and 7 years in research. Author of several books. Wishes to move to New York Metropolitan area (not more than 200 miles from New York) to teach advanced accounting, auditing, taxes, accounting systems, business and economic statistics, corporation finance, and analysis of financial statements. Minimum salary considered about $7,000 for 10 months.   E721

Industrial and personnel management, labor economics, money and banking, economic principles, statistics: Man, 43, married; A.B., M.A., requirements for Ph.D. completed except dissertation, which is in progress. Ten years of successful college and university teaching in above fields; 9 years of personnel experience in industry and government. Especially interested in college or university teaching position. Prefers Midwest location. Available in fall, 1958.   E722

Economic theory, statistics, money and banking, market research, corporate finance, history of economic thought, economic development, business cycles, national income, mathematics of finance, foreign trade, international economics: Man, 35; Ph.D., University of Pennsylvania. Broad experience in industry, government, and teaching. Desires university connection either full-time teaching or combination of teaching and research.   E723

Marketing: Man, 35; B.S., M.B.A., major Northeast university; almost completed Ph.D. Extensive responsible experience in government and industry as analyst and research manager. Available for consulting and short-term assignments. Will travel. Salary and/or fees open.   E728

Economic planning, history of economic thought, labor economics, government and business, international economics, economic principles: Man, 46, married; Ph.D., Columbia University. Eight years of teaching experience; 10 years of experience with federal government in responsible staff position; publications. Now professor of economics at small liberal arts college. Desires new teaching position.   E729

Economic development, international finance, foreign trade, monetary policy: Man, 29; M.A., Yale. Fellowships, languages, area concentration Southeast Asia and Western Europe; experience in business and research. Seeks research position. Willing to relocate.   E735

Economic theory, statistics, industrial management, money and banking, government finance and policy, marketing and market research, corporate finance, investments and securities, managerial economics, labor relations, economic fluctuations and development, international economics: Man, 35; Ph.D. Extensive experience in business, teaching, and government. Desires teaching or research position.   E737

Industrial management, production control, accounting, budgeting and statistics: Man, 35; B.A. (industrial management), M.A. (economics), M.B.A. (industrial management and engineering). Two years of experience in research and teaching; 5 years of experience in industry. Interested in college or university teaching of the above field. Prefers Middle Atlantic states. Will consider other locations.   E738

Economics, money and banking, investments and corporation finance, economic development, Eastern European economics: Man, 29; Ph.D., 1953. Part-time lecturer at a large Canadian college and economist to an investment banking house; publications; research and teaching experience in Europe and U.S. Interested in full-time college or university teaching and/or research. Available in fall, 1958.   E741

Economic principles, economics of transportation, economics of communications, social control of industry: Man, 28; B.S., M.S., honors graduate, fellowship recipient. Desires teaching position in September, 1958.   E742

Industrial management, personnel management, economics, labor: Man, 37, married; Ph.D. Three years of teaching experience; 10 years in business and industry. Interested in college or university teaching position. Available in fall, 1958.   E743

Basic and applied research, industrial development, area, location, cost studies: Man, 50; M.A., doctorate residence completed at Harvard University. Sixteen years of college teaching experience; held supervising federal economist position; consulting economist to a leading research institute; 6 languages. Seeks position in research and/or college teaching. Consulting work welcomed in Midwest, South, Southwest. E744

International economics, principles, theory, economic systems, fiscal policy, government regulation, money and banking, corporate finance, management, statistics: Man, 39, married; B.S., M.A., Ph.D. Extensive experience in private industry and government; some teaching experience. Desires teaching position with opportunities for supplemental management and economic consulting.   E748

Economic and business history, business and government, insurance: Man, 39, married; Ph.D., New York University. Desires teaching position, research, opportunities to write company histories. Interested especially in a college or university teaching position in the Eastern area. Of special usefulness to Eastern life insurance companies as research assistant, economist, or historian. Has 17 years of governmental experience, including those of an advisory and administrative nature.   E749

Economics, American economic history, consumption economics: Man, 43, married; Ph.D., Columbia. Sixteen years of college teaching; now department chairman in liberal arts college. Seeks position including advanced level teaching or research in fields of interest.   E754

National income and business cycles, fiscal policy, money and banking, public finance, labor economics, price and distribution theory, international trade, statistics: Man, 44, married; Ph.D. Nine years of teaching experience. Now associate professor with responsibilities in both economics and business administration. Desires to relocate either September 1958 or 1959 to a college where full time can be devoted to economics.   E755

Corporation finance, cost analysis, budgetary analysis, pricing, management: Man, 27; B.S., Columbia University, 2 years of postgraduate study in business subjects. Five years of business experience, aircraft industry; management accounting, pricing, cost analysis. One year of experience teaching economics and finance in night college. Fair knowledge of French. Desires teaching and/or business position. E756

Principles of economics, statistics, public finance, accounting: Man from El Salvador, 34, married; requirements for master's completed (U.S.), requirements for doctor's complete (El Salvador). Bilingual Spanish-English; experience in sales and teaching. Currently employed in collecting, assembling, reporting statistical information for forecasting, economic, marketing surveys. Desires economic research or teaching position.   E757

Business and economic forecasting, business cycles, international economics, trade, and finance, statistics: Man, 49, married; master's and doctor's degrees in economics and statistics from leading university. Serving since 1946 in a well-known university; desires new position as professor, chairman of department, and/or director of research in a college or university. Twenty years of experience in teaching, research and direction thereof, including 5 years as senior economist in Washington, D.C.; numerous studies and publications. Available in June or September, 1958, on a permanent basis.   E758

Principles, public finance, international trade, agricultural economics and co-operation: Man (Indian); M.A., M.Litt., Ph.D. Eight years of research experience in pure theory and applied economics; 2 years of teaching experience; now lecturer in economics in a technical college in India. Desires post in U.S.   E759

Economics, banking, marketing: Man, 42; Ph.D., Columbia University, plus postgraduate training in marketing and law. At present with leading firm of management consultants specializing in marketing and management audits. Wishes to return to academic life in September, 1958. Has just completed two extensive surveys for Columbia University and Ford Foundation on foreign investment. Books; scholarly articles. Five years with United States government as senior economist; 4 years with foreign governments and U.N. as economic adviser; 11 years of university teaching, mostly at senior and graduate level. Minimum requirements: tenure, associate professor level.   E760

Economic principles, accounting, economic history, business cycles, public relations: Man, 43; A.B. (English), B.S. (accounting), Ph.D. (economic history), London. Six years of teaching experience (in English) at two large universities and a state college and in adult education. Has taught English and economic principles abroad. European correspondent for American liberal monthly for 4 years; experience in business management, both private and quasi-military; now chief of public relations for quasi-military retailing organization in Europe. Wishes to return to university or private research. Would consider university public relations and teaching-research combination or English-economics teaching or teaching-research combination.   E761

Economics: Man, 35, married; M.S., Columbia Fulbright fellow, University of Amsterdam; Ph.D. requirements (except for dissertation) completed at University of Geneva, Switzerland, degree expected in 1959. Extensive business experience; director of American cultural institute in Germany, 1951-55; CBS correspondent, political and economic affairs in central Europe, 1947-48; publications. Currently assistant professor at a large Midwestern university. Finest references. Available in September 1958 or 1959.   E762

Economic theory, comparative economic systems, money and banking, underdeveloped areas and economic development, American economic history: Man (Indian), 29, married; M.A., LL.B., completing Ph.D. course requirements and dissertation in progress. Half year of experience in business association; year and a half of teaching experience; published 5 articles. Desires teaching position beginning in fall, 1958.   E763

Industrial management, economics: Man, 31; Ph.D. Three years of teaching experience; 3 years of business experience as director of research; several publications.   E764

Economic and business research, community and regional economic base analyses, public finance, principles, finance: Man, 34, married; Ph.D. in economics. Teaching, governmental research, and private economic consulting experience; publications; administrative experience. Currently acting head and research director of leading state university government research bureau. Desires directorship of university bureau of business research, West Coast, Midwest, or Eastern location.   E765

Statistics, mathematics of finance, principles of insurance, social security, life insurance, private and social insurance: Woman, 38, married; Ph.D. Desires teaching or research position. Available in September, 1958.   E766

Statistics, market research: Man, 38; Ph.D. Eight years of experience in research and administration of business statistics, market research, integrated data processing system and business control. Desires research position with university or consultant firm or staff position with large corporation.   E767

Labor economics, industrial relations, economic principles, corporation finance, money and banking: Man, 29; Ph.D., University of Wisconsin; currently working on law degree. Five years of teaching and research experience; consulting experience in labor economics, taxes, and investments. Present position teaching at a large state university. Desires teaching, research, or teaching-research position in an urban area. Also available for consulting.   E768

THE AMERICAN ECONOMIC REVIEW

Volume XLVIII   SEPTEMBER 1958   Number 4

OTHER DEPARTMENTS

A COMPARISON OF SOVIET AND AMERICAN
INVENTORY-OUTPUT RATIOS

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Soviet economists assert that their economic system is more economical of working capital than is the capitalist system. The argument runs that having eliminated the anarchy of competition, the difficulty of selling, and the large inventories arising from overproduction, they can keep inventories to the minimum required to maintain steady production and sale of goods. Prima facie, these reasons seem at least plausible. On the other hand a number of American studies of the working of the Soviet economic system have suggested that there are good reasons why the Soviet economy might require larger stocks of inventories per unit of output than does the American economy. Among other things, the chaotic functioning of the supply system, the slowness of transportation, and the pressures on managers to hoard materials would all lead to higher inventory-output ratios than in the United States.

It is the purpose of the present study to present some measures of inventory holdings and of output in the Soviet Union, to be compared with similar data for the United States, in order to determine which economic system is really the more economical of inventories. One possible approach would be to compare the total stock of inventories in the entire economy with some measure of total output, such as gross national product, for the two economies. There are, however, two deficiencies in such an approach: first, it is extremely difficult to obtain an economy-wide estimate of the stock of inventories in either economy; and secondly, such a comparison would be more or less irrelevant because of structural differences in the two economies. The nature of production is such that inventory-output ratios are different for the various activities and processes which generate the national income, so

that economy-wide inventory-output ratios are influenced by the composition of output as well as by inventory efficiency in individual sectors of the economy.

The approach to be followed here, therefore, will be to make comparisons of Soviet and American inventory-output ratios for two individual sectors, namely, industry and trade. This approach does not completely eliminate the problem of differences in the composition of output, but it does narrow it down considerably. Moreover, these are the two sectors in which the bulk of inventories are concentrated. On January 1, 1956, for instance, something like 86 per cent of all inventories in the USSR were accounted for by these two sectors.[1] Fortunately, also, both in the United States and in the Soviet Union, the available data on inventories is much better for these two sectors than for the rest of the national economy.

I. Industry

Differences in Soviet and American statistical practice make it impossible to delineate "industry" sectors in the two economies that are exactly comparable in scope. It is necessary to take the Soviet concept of industry as given, and then try to approximate it as closely as possible in American statistical data. The closest analogue in American statistics to industry as it is defined in the Soviet Union is manufacturing industry as defined in the Standard Industrial Classification. The Soviet concept is rather broader in scope, including in addition to the manufacturing branches also power generation, mining and fisheries. There is no practicable way to modify the statistics on output or inventories to have comparable figures for the two economies, so we have to take the figures as given, and then subsequently estimate the possible effect of differences in scope on the inventory-output ratios found.

Data for the United States. Figures on inventories in manufacturing are compiled by the Department of Commerce, and are available for the years from 1929 to the present. These figures are simply taken as given. It is more difficult to decide on a measure of manufacturing output. The main choice is between some measure of net output, such as income generated or value added, or some measure of gross output, such as, for instance, the data on sales and shipments of manufacturers. We

[1] According to *Vestnik Statistiki*, 1957(2), p. 94, 38.8 per cent of working capital in the Soviet economy on Jan. 1, 1956 was provided by bank credit, and the corresponding figures for industry and trade were 34.2 per cent and 46.9 per cent respectively. According to *Den'gi i Kredit*, 1957(2), p. 3, total bank credit on Jan. 1, 1956 was 200 BR [billion rubles], and according to A. Popov, *Rol' gosudarstvennogo banka v narodnom knoziaistve*, Moscow 1956, p. 23, 50.1 per cent of this, or 100.2 BR, was extended to industry, 35.2 per cent or 70.4 BR to trade. These figures together imply total working capital in the economy of 515.5 BR, of which 442.2 BR or 86 per cent was in industry and trade.

are almost forced to use a gross concept of output in order to achieve comparability with the available Soviet data, and in order to have a fairly continuous series it is convenient to use the figures on manufacturers' sales compiled by the Department of Commerce. This choice ensures comparability in scope for the inventory figures and the output figures, and also provides a continuous series from 1929. These figures for inventories and for sales are presented in the first two columns of Table 1; and in the last column, the ratio of sales to inventories.[2]

Data for the Soviet Union. The next step is to obtain for the Soviet concept of industry figures which are as close as possible to the U. S. concepts of output and inventories reflected in Table 1. The Soviet

TABLE 1.—SALES, INVENTORIES, AND SALES-INVENTORY RATIOS IN MANUFACTURING, UNITED STATES, SELECTED YEARS

Year	Annual Sales (billion dollars)	End of Year Inventories (billion dollars)	Ratio of Sales to Inventories
1929	70.3	12.8	5.49
1930	57.1	11.3	5.05
1933	34.3	8.2	4.18
1935	50.3	9.1	5.51
1940	70.3	12.9	5.46
1944	165.4	19.6	8.45
1945	154.5	18.5	8.37
1950	231.4	34.5	6.70
1954	280.7	43.5	6.45

Source: U. S. Department of Commerce, Office of Business Economics, *Business Statistics, 1957 Edition,* Washington 1957, pp. 14 and 17.

planners use a number of concepts of industrial output of which the one closest to the output concept chosen for the United States is gross output (*valovaia produktsiia*) in current prices. Except for a few qualifications this is essentially a figure obtained by adding together the value of output of all industrial enterprises, value of output being measured in current prices net of turnover taxes.

Although the Soviet statisticians compute gross output of industry in current prices for every year, unfortunately they never publish the figures. But estimates can be made in various indirect ways. For many of the years in which we are interested the Russians have published a percentage breakdown of outlays on production by large-scale enter-

[2] Throughout the present article, the ratios computed are between output for the year and inventories as of the end of the year. It would be more accurate to use an annual average inventory figure, rather than the year-end figure, but the procedure used generally gives results which are not far different, and permits the calculation of more ratios than would otherwise be possible.

prises by such categories as materials, wages, amortization, and so on.
In addition there are available for many of these years data on the
absolute amount of amortization charged or for the wage bill in large-
scale industry, and these data together with the cost structure imply
figures for the cost of industrial output. Estimates of the cost of output
of large-scale industry obtained by this method are shown in columns
(1) and (2) of Table 2.

For most of the years shown the estimate based on the amortization
charged is larger than that based on the wage bill, probably because

TABLE 2.—GROSS OUTPUT, INVENTORIES, AND OUTPUT-INVENTORY RATIOS IN THE
SOVIET UNION, SELECTED YEARS

Year	(1) Cost of Output via Wage Bill (million rubles)	(2) Cost of Output via Amortization (million rubles)	(3) Value of Output (billion rubles)	(4) End of Year Inventories (billion rubles)	(5) Ratio of Output to Inventories
1928	8,983		16.7		
1929			21.0	5.2	4.0
1930	12,941		27.7	6.1	4.5
1931	22,015		33.9	8.9	3.8
1932	31,827	31,431	38.5	12.0	3.2
1933	38,055	41,000	41.9		
1934	47,498	49,628	53.	25.1	2.1
1935	71,704	81,961	86.	30.4	2.8
1936	100,145	112,347	120.	39.3	3.1
1937	151,935	148,900	159.	49.8	3.2
1938		189,000	190.	56.7	3.4
1939			220.	75.0	2.9
1950			520.	188.	2.8
1955			830.	227.	3.7

Sources: The data for this table are taken mostly from the standard Soviet statistical
handbooks, such as *Sotsialisticheskoe stroitel'stvo*. Detailed explanations of the derivations
are given in a mimeographed appendix, obtainable from the author.

the stated wage bill understates actual expenditures for labor. Other
indications of the cost of industrial output can be obtained from official
statements about the magnitude in rubles of a one per cent reduction in
the cost of industrial output, and these estimates can then be used as a
check on the other procedures. With the addition of profits and subtrac-
tion of subsidies, these figures can then be converted into the value of
output of large-scale industry in current prices. Additional indications
of the size of output are provided by the official series on the value of
output of large-scale industry in 1926-27 prices. For some of the years
not too far removed from 1926-27, this measure is probably fairly close
to the value of output in current prices. These various approaches fre-
quently give contradictory results, but in column (3) of Table 2 there

is shown a series of figures for output which is intended to be a fairly high estimate of industrial output, chosen from among these alternative sources of information.

The intention has been to provide a measure of output of large-scale industry rather than of all industry, for it is likely that the data for inventories cover large-scale industry only. Even if this supposition were wrong, the results would not be greatly affected. In the later 'twenties and early 'thirties large-scale industry accounted for about 90 per cent of all industrial output,[3] so that substitution of all industrial output for large-scale industrial output would raise the ratios in column (5) of Table 2 for those years by about 11 per cent, i.e., from something in the neighborhood of 4 to about 4.4. In the second half of the 'thirties the relative importance of small-scale industry probably declined rapidly, so that a much smaller correction would have to be made for these and for subsequent years.

Estimates of inventories in Soviet industry are shown in column (4) of Table 2. Very little direct information has been published by Soviet agencies on the size of inventories in industry, and so these estimates have necessarily been based on rather roundabout calculations.[4]

The closest Soviet analogue to the American concept of inventories is "circulating capital" (*oborotnye sredstva*).[5] This differs from inventories in the usual sense, however, in including in addition to real inventories also such items as claims against other enterprises and monetary assets. The Soviet enterprise is able to hold circulating capital on the basis of three main kinds of liability items, namely bank credit, indebtedness to other enterprises, and "own working capital" (*sobstvennye oborotnye sredstva*). This last item is defined in Soviet accounting practice as the difference between the sum of a number of liability accounts and the value of various kinds of "immobilized" assets, this difference being a measure of its resources free to be invested in various kinds of circulating capital. There is enough information to build up a fairly accurate series on the own working capital of industry.

For the economy taken as a whole, of course, the indebtedness resource is offset by the claims of other enterprises and so cannot be a means for holding real inventories. This is probably approximately true

[3] TsUNKhU, *Sotsialisticheskoe stroitel'stvo*, 1934, p. 24.

[4] The derivation of individual figures is too complicated to be explained here; a mimeographed appendix giving the details of these calculations can be obtained from the author. There is, however, a great deal of data bearing on the size of inventories, with a number of alternative possibilities of cross-checking the results, so that we can be fairly confident that these figures approximate closely the magnitudes we are interested in.

[5] An account of Soviet working capital concepts can be found in any Soviet textbook on industrial accounting. A particularly careful explanation is given in S. Shchenkov, *Bukhgalterskii uchet v promyshlennosti*, Moscow 1955, pp. 371-85.

even for a single sector of the economy, such as industry. So if bank credit and own working capital for industry are added together, the result is a close approximation of real inventories plus monetary assets. The monetary assets of a Soviet enterprise are held almost exclusively in the form of an account with the *Gosbank*, and so by using *net* indebtedness of industry to the *Gosbank* rather than gross indebtedness it is possible to obtain from the liability side of the balance sheet a measure of real inventories. This is the approach which underlies most of the estimates of inventories shown in Table 2.

Comparing the results obtained for the United States and for the Soviet Union, it appears that the Soviet Union requires larger stocks of inventories per unit of output than does the U. S. economy. In the United States the ratio reached its low point of a little over 4 in the depression years of the early 'thirties, but for most other years it has been much higher. During the second world war when the U. S. economy was reorganized to resemble in some ways the Soviet economy the ratio rose above 8. In the Soviet series, on the other hand, a ratio of about 4 represents the high point, and for most of the years observed the ratio was around 3. There has been a notable improvement between 1950 and 1955. In 1949 an intensive campaign to cut down on inventory requirements was begun. Wasteful inventory management became a standard rebuke in the budget speech, there was a "spontaneous" spate of pledges to get more output per ruble of inventories, and there was a long discussion in the economic journals aimed at finding an effective indicator of inventory efficiency to control enterprise performance. Apparently this focusing of attention on the problem resulted in an improvement in the ratio.

The relatively low ratio for the Soviet Union compared with the United States is an interesting finding. There are certainly many reasons for expecting that the Soviet economic system should be wasteful of inventories, but the difference found is surprisingly large. Is it possible that the results arise from the methods and concepts used, and may not really reflect inventory inefficiency? There are several possible questions.

1. Are there differences in the valuation of inventories in the two economies? Generally speaking Soviet inventories are priced at current prices; when prices are changed, inventories are revalued immediately at the new prices. Most inventories in U. S. manufacturing are valued on the principle of "cost or market value, whichever is lower." This practice results in undervalued inventories when prices are rising, but in periods of falling or steady prices is no different from the Soviet principle. About 15 per cent of inventories in U. S. manufacturing are valued on the LIFO basis,[6] which also results in undervaluation when

[6] *Business Statistics, 1957 Edition*, p. 204.

prices are rising, and overvaluation when prices are falling. These peculiarities of the American valuation methods may be part of the explanation for the relatively high figures in the postwar period in the United States, but it would probably not influence the ratios for the 'thirties at all.

2. What is the effect of the difference in scope of the "industry" concept in the two economies? It will be remembered that the Soviet concept of industry includes mining and power generation in addition to the branches covered in the American figures. In both these branches the ratio of output to inventories is higher than in manufacturing,[7] so that the figures in Table 2 are higher than figures for Soviet manufacturing alone would be.

3. It is possible that some of the difference between the U. S. and Soviet ratio may be explained by differences in the branch structure of industry. Inventory-output ratios vary as between individual branches of industry, and so differences in branch composition would result in different averages, even if U. S. and Soviet ratios were identical in each branch separately. Further study of this question is needed, but is beyond the scope of this article.

4. There may be systematic biases in the reporting of inventories in each economy. In the Soviet Union increments in goods in process are counted as output in some industries, and so there may be some pressure on plant managers to exaggerate inventories in these branches. In the United States there may be an incentive to underestimate inventories in order to reduce profits and so tax liability. Verification of these possibilities would be a formidable problem, but it does not seem likely that any such biases could be quantitatively important.

5. Because we have used a gross concept of output, the ratio of output to inventories will be sensitive to differences in the degree of integration or fragmentation in the industrial structure. Can it be that the higher ratio of output to inventories in the United States is the result of more double-counting in the U. S. measure of output? This is a very elusive question, but a rough answer can be gotten by comparing the ratio of output gross of materials, fuel and power to output net of these inputs in both economies. This method won't answer the question precisely. Some materials inputs are from enterprises outside the industrial sector, and so the ratio of net to gross will be affected not only by the degree of integration in industry, but also by the composition of output (e.g., as between industry processing agricultural raw materials and other industry) and by relative prices of materials. But the result still should be suggestive.

For the United States the ratio of value added (i.e., value of output less materials, purchased energy and fuel) to gross output in manu-

[7] See for instance data cited in *Planovoe Khoziaistvo*, 1940 (5), p. 53.

facturing has stayed very close to 40 per cent during the whole period from 1929 to the present.[8] The Central Statistical Administration has published the statement on the cost structure of Soviet industrial output in 1955 shown in column (1) of Table 3.

On the basis of the information that in 1955 profits in industry were 8.2 per cent of costs, the distribution given in column (1) has been corrected to one for value of output as given in column (2). On the basis of the latter figures the ratio of value added to gross output in Soviet industry turns out to be about 33 per cent.[9] (The same result is obtained if this calculation is made for 1937.) So despite the qualifica-

TABLE 3.—STRUCTURE OF COST OF INDUSTRIAL OUTPUT AND OF VALUE OF OUTPUT
OF SOVIET INDUSTRY, 1955

	(1) Per Cent of Cost of Output	(2) Per Cent of Value of Output
Materials	66.1	61.1
Fuel	4.4	4.1
Energy	1.8	1.7
Amortization	3.4	3.1
Wages	21.2	19.6
Other	3.1	2.9
Profits	—	7.6
Total	100.0	100.0

Sources: The cost structure is taken from TsSU, *Promyshlennost' SSSR*, p. 29. The statement for the rate of profits is given in A. Zverev, "Gosudarstvennyi biudzhet vtorogo goda shestoi piatiletki," *Planovoe Khoziaistvo*, 1957 (3), p. 18.

tions indicated above about our method of calculating net output it certainly does not seem likely that the difference in the ratio of output to inventories in the two economies could be accounted for by a differential degree of grossness in the output measure. Indeed, such evidence as is available suggests that allowance for this factor implies even greater inefficiency in inventory use than was shown in the original calculations.

II. *Trade*

The term "trade" is in common usage a fairly nebulous term, and so it is necessary first to define this activity and to explain the concepts

[8] Data on value added in manufacturing can be found in U. S. Bureau of the Census, *Statistical Abstract of the United States: 1957*, Washington 1957, p. 783, and data on sales by manufacturers are found in *Business Statistics, 1957 Edition*, p. 14.

[9] It is not at all surprising to find that in the Soviet Union the ratio of gross to net output is greater than in the United States. There is great pressure in the Soviet economic system to induce administrators to resist integration of enterprises. The most important measure of performance for Soviet industry is gross value of output, and so there is a bias on the part of ministries and intermediate level organs to do as much double-counting as possible.

of stock and flow associated with it. The concept of trade which we have in mind is simple enough—it is the process of moving finished consumer goods from the producing enterprises to households. There is little difficulty in measuring the flow of this activity in any economy. It is given by retail trade turnover or retail sales, i.e., the flow of goods through the last stage of the process. The main problem is to decide just how broad to make the concept of retail sales. The Soviet and U. S. statistics seem to be roughly comparable in scope; in particular they both limit retail sales to sales of goods and omit sales of services. We have also excluded, for the purpose of the present study, sales of eating and drinking places, on the grounds that this is an activity in which the problem of inventory economy is not a matter of great importance.[10]

Ideally the concept of stocks involved in trade should be the total stock of finished goods held at any given moment by all organizations—retailers, wholesalers, and manufacturers—at all stages of trade activity. Unfortunately, however, it is difficult to clothe this ideal concept in statistical fact. The worst problem is in the U. S. statistics; they are simply not reconcilable with this concept. Much of the stock we are interested in measuring is held by wholesalers, but wholesale trade as defined in the Standard Industrial Classification includes not only the movement of goods from producers to households, but also the movement of goods from producer to producer. Data on U. S. wholesale trade generally conform to this definition. Another difficulty is in measuring the stocks of finished goods held by manufacturers up to the time they are moved on to some other organization on their way to households. This is a fairly important part of the total stock associated with trade, particularly in the United States, where retail trade organizations get their supplies to a significant extent directly from manufacturers without the intervention of wholesalers.[11] The amount of finished goods held by manufacturers for ultimate sale to households can be estimated only roughly, and then only for isolated years. Soviet statistical rubrics are more nearly congruent with our ideal concept. The Soviet definition

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[10] Unfortunately it is not possible also to exclude the stocks associated with this portion of trade, but it is assumed that they would be small. Moreover, since the ratio of sales of eating and drinking places to total retail sales is not widely different in the two countries, there should be no differential distortion because of this omission. The share of sales of eating and drinking places in all retail sales has been about 10 to 20 per cent in the Soviet Union and about 10 per cent in the United States. See TsSU, *Sovetskaia torgovlia, statisticheskii sbornik,* Moscow 1956, p. 20, and *Statistical Abstract of the United States: 1957,* p. 844.

[11] Sales by wholesalers to retailers amounted to only 59 per cent of retail sales in 1935, 53 per cent in 1939, and 50 per cent in 1948. U. S. Bureau of the Census, *U. S. Census of Business: 1948,* Vol. IV: *Wholesale Trade—General Statistics,* Washington 1952. Similar information for 1954 has not yet been released. A large part of the difference lies in the retail trade mark-up, of course, but even allowing for this, there still remains a considerable gap to be covered by direct purchases from manufacturers.

of wholesale trade involves only the movement of goods to retailers, and
Soviet data on stocks in wholesale trade also include most inventories
of finished goods held by manufacturers to be passed on to retail
trade.[12] So it is necessary only to add the Soviet figures for stocks in
wholesale trade to stocks in retail trade to obtain the desired total.

TABLE 4.—INVENTORIES, SALES AND SALES-INVENTORY RATIOS IN RETAIL TRADE IN
THE UNITED STATES AND U.S.S.R., 1932–1935

Year	United States (billion dollars)		Sales-Inventory Ratio	U.S.S.R. (billion rubles)		Sales-Inventory Ratio
	Inventories	Sales		Inventories	Sales	
1932				2.4	35.5	14.8
1935				8.8	74.5	8.5
1936				11.7	98.8	8.4
1937				13.6	115.8	8.5
1938	6.6	34.9	5.3	12.9	127.4	9.9
1939	6.9	38.5	5.6	12.9	149.3	11.6
1940	7.6	42.6	5.6	16.4	152.2	9.3
1941	9.8	50.7	5.2	7.3	131.4	18.0
1942	10.0	51.5	5.1	5.7	59.6	10.5
1943	9.5	56.0	5.9	10.0	63.3	6.3
1944	9.5	61.9	6.5	13.7	90.8	6.6
1945	9.9	69.7	7.0	19.1	127.0	6.6
1946	14.8	91.9	6.2	27.8	198.4	7.1
1947	17.6	108.9	6.2	38.7	262.7	6.8
1948	19.8	119.8	6.1	59.5	264.7	4.4
1949	19.1	120.2	6.3	72.3	289.6	4.1
1950	24.1	133.1	5.5	64.1	312.2	4.9
1951	26.5	146.0	5.5	76.9	329.6	4.3
1952	27.0	151.4	5.6	93.2	340.2	3.6
1953	28.4	157.4	5.5	94.3	373.6	4.0
1954	27.6	157.5	5.7	90.0	421.9	4.7
1955				98.9	443.1	4.5

Sources: The U. S. data are based on Business Statistics, 1957 Edition, and on U. S.
Department of Commerce, Office of Business Economics, National Income, 1954 Edition,
Washington 1954. Data for the Soviet Union are from Sovetskaia torgovlia. Detailed citations
and explanations of adjustments made are to be found in the mimeographed appendix.

Unfortunately, however, such data on stocks in Soviet wholesale trade
are available for only a limited number of years.

Because of these data limitations we shall for the moment consider
the ratio of retail sales to stocks held by retail trade organizations
alone. Data of this type are available for both economies for a fairly

[12] The definition of wholesale trade in TsUNKhU, Slovar'-spravochnik po sotsial'no-
ekonomicheskoi statistike, Gosplanizdat, Moscow 1944, p. 164, is "turnover of an inter-
mediate character concerned with the sale of consumption goods to trade organizations."
For a statement that stocks held by manufacturers are included in the data on wholesale
trade see N. N. Riauzov and N. P. Titel'baum Statistica sovetskoi torgovli, Moscow 1956,
p. 91.

long series of years, and so this approach is very useful in investigating changes over time in both economies. Subsequently we will try to make some estimates of total stocks involved in trade for a few years to see what modifications might have to be made in our conclusions if we were able to use the ideal concepts. The data for inventories and sales in retail trade organizations in the United States and Soviet Union for a series of years are presented in Table 4. There are two important points we want to discuss with regard to these data: (1) changes over time in the Soviet Union and (2) the difference between the ratios for the United States and the Soviet Union.

Changes over time in the Soviet Union. The most striking thing about the series for the Soviet Union is the considerable decline in the sales-inventory ratio. From an average of perhaps 9 in the prewar years, it has dropped to something in the neighborhood of 4 in the postwar years. In other words it now takes roughly twice as big a stock of goods to support a given flow of retail sales as it did in the 'thirties. This is all the more surprising when we note that in the case of the United States there has been virtually no change over the entire period, and indeed even very little variation from year to year. What explanation can there be for this startling change?

In view of the magnitude of the change, it is hard to believe that it can be the outcome of changes in inventory efficiency and one is tempted to find some alternative explanation, such as some institutional change involving pricing or valuation practices. If, for instance, inventories in the prewar period had been valued net of the very high turnover taxes, and gross of these taxes in the postwar period, this could go a long way toward explaining the decline in the ratio. But there appears to be no evidence to suggest that any such change has taken place. There seems to be no doubt that the stocks are now valued at prices gross of turnover tax, but this apparently has always been the case. The tax has always been paid before the goods ever came into the hands of retail trade organizations, so that the inventories in trade were valued at prices gross of the turnover tax.[13] Furthermore, it is clear from the textbooks on accounting in trade that although the retail trade organizations value stocks both in terms of retail prices and at cost to themselves, no attempt is made to distinguish the value of these goods at prices net of turnover tax. Therefore it would not be possible for them even to report their stocks in prices net of turnover tax.[14]

Another factor which might have some effect on the sales-inventory

[13] ". . . the practice in the thirties was to have the tax on industrial commodities paid by the factory, and on agricultural commodities by the procurement organization." F. D. Holzman, *Soviet Taxation*, Cambridge, Mass. 1955, p. 90.

[14] See for instance, N. I. Il'in, *Bukhgalterskii uchet v sovetskoi torgovle*, Moscow 1954, Ch. 5.

ratio would be changes in the structure of trade. There are marked differences in the ratios for different kinds of trade, so that a shift in the composition of trade might lead to a change in the average ratio for trade as a whole. For instance if we distinguish only trade in food products and trade in other products, it turns out that the sales-inventory ratio for the latter is of the order of three times the ratio for the former, both in the USSR and in the United States.[15] But as will be shown subsequently, there has been virtually no shift in the structure of trade as between these two categories, and the sales-inventory ratio has deteriorated more or less uniformly for both categories.

Some of the decline in the sales-inventory ratio in Soviet retail trade is accounted for by a change in organization which has resulted in a smaller fraction of total trade inventories being held by wholesalers, and a higher proportion by the retail trade network. In 1937 retail trade organizations held something like 60 per cent of all stocks and in 1940, about 56 per cent, as against something like 80 per cent in the postwar period.[16] If we reallocate the total postwar stocks between wholesale and retail trade in the postwar period to approximate the prewar distribution this would bring the sales-inventory ratio up to somewhere in the neighborhood of 5 instead of the actual 4. But this is still considerably below the prewar ratio, and so this factor does not go very far towards explaining the deterioration. There appears really to have been a significant change in the sales-inventory ratio for all kinds of trade and for trade activity as a whole.

We could interpret this in two possible ways. (1) There may have been an actual deterioration in the technical efficiency with which the goods are moved through the channels of trade, i.e., longer hauls, more "dead stock," excessive intermediate handling of goods, and less frequent shipments, for example. (2) A more plausible explanation would be that the change represents an improvement in the quality of Soviet trade. It seems quite possible that the very high ratios of the prewar period reflect inadequately filled pipelines, intermittently empty shelves, and consequently great inconvenience to the consumer rather than anything that could be called rational economy of inventories. The postwar figures would represent a situation in which there has been a less uneven flow of goods through the system, and more nearly constant availability of goods. If this is indeed the explanation, it is an interesting fact in itself, and an important point to keep in mind when the difference in the ratios for the United States and the Soviet Union is considered.

Comparison of U. S. and Soviet ratios. The comparative inventory

[15] *Sovetskaia torgovlia,* pp. 88-91 and *Business Statistics, 1957 Edition,* pp. 45-50.

[16] *Sovetskaia torgovlia,* p. 79.

intensity in Soviet and U. S. trade depends on the period we are considering. The U. S. ratio has remained more or less constant at about 5.5 over the period while the Soviet ratio has declined from 9 to 4. If the interpretation suggested above for the decline in the Soviet ratio is accepted, the implication is that the postwar Soviet figure represents a qualitative state of trade more nearly comparable to that in the United States as far as shortages, customer convenience, and so on is concerned. If this is the case, then it appears that the Soviet trade system is significantly less economical of inventories than is trade in the United States. There are, however, two additional considerations to be investigated, namely (1) difference in the organization of trade and (2) difference in the composition of trade in the two economies.

TABLE 5.—RATIO OF RETAIL SALES TO TOTAL STOCKS IN TRADE, SOVIET UNION, SELECTED YEARS

Year	Total Stocks in Trade (billion rubles)	Sales Inventory Ratio
1937	22.5	5.1
1940	29.3	5.2
1950	96.3	3.2
1951	111.7	3.0
1952	128.8	2.6
1953	123.9	3.0
1954	110.9	3.8
1955	124.6	3.6

Source: Sovetskaia torgovlia, pp. 79; and Table 4.

1. Our own economic history has been marked by radical changes over time in the institutional organization of the distribution process, with consequent shifts in the relative importance of the different links in the system as inventory holders. It is conceivable that such a difference between U. S. and Soviet institutions should be part of the explanation for the comparatively high inventory-sales ratio of Soviet retail enterprises, and our view of inventories should be broadened to take in all the stocks of finished goods held for eventual sale to consumers to see whether this changes the result of the comparison.

Data on the value of stocks in all links of the Soviet trade network are available for several years. These are presented in Table 5 with the associated sales-inventory ratio for each year. Although the statistical source is somewhat vague as to the meaning of these figures, it is evident from discussions of trade statistics in other Soviet sources that they do not include goods held for sale to industrial users,[17] they do

[17] The definition of wholesale trade in TsUNKhU, *Slovar'-spravochnik po sotsial'no-ekonomicheskoi statistike*, Moscow 1944, p. 164 is "turnover of an intermediate character concerned with the sale of consumption goods to trade organizations."

include goods held by all the organizations standing between the producer and the consumer, including even the finished goods held in warehouses of industrial producers.[18] Furthermore, it is fairly certain that they are valued in retail prices.[19]

Figures for total stocks of goods held for ultimate sale to consumers at all stages of the trade system in the United States are much more difficult to obtain. However, on the basis of data in the Census of Business and the Census of Manufactures for certain years, it is possible to make estimates of the concepts we want. As stated earlier, wholesale trade statistics in the United States include not only the handling of goods destined for consumers, but also goods destined for producers. So the first step is to separate out from wholesale trade inventories those stocks which are held for sale to consumers. The Census of Business for both 1939 and 1948 gives a breakdown of wholesale trade sales by kind of wholesaler and by class of customer. It also gives inventories held by each kind of wholesaler. Our procedure is to allocate wholesalers' inventories to "trade" as we have defined it in proportion to the percentage of the wholesalers' sales which went to retailers. The result is an estimate of the stocks held by wholesalers for sale to households rather than to producers. There is certainly no reason for supposing that this procedure will give exact results, but it is the best that can be done under the circumstances.

The next step involves making an estimate of the stocks of finished goods held by maufacturers for purposes of trade. The 1939 and 1947 Census of Manufactures gives end-of-year inventories of finished goods held by manufacturers, broken down by branch of industry. An estimate of inventories held for consumers is obtained by adding up the finished-goods inventories in those branches of industry whose output is destined mainly for consumers. Again any such allocation is bound to involve many errors, but still it will provide a rough approximation of the desired concept. (Unfortunately the 1947 Census of Manufactures inventories figures are for January 1, 1948, rather than for Janu-

[18] The main sales agencies of the branches of industry producing consumer goods are now part of the Ministry of Trade, but even when they were administratively under the control of the industrial commissariats, they were still included for statistical purposes in wholesale trade. *Slovar'-spravochnik*, p. 164. Moreover, according to N. N. Riauzov and N. P. Titel'baum, *Statistika sovetskoi torgovli*, Moscow 1956, p. 91, warehouses attached to industrial plants also report their inventory holdings for inclusion in the totals of wholesale trade stocks.

[19] There is no explicit statement in the source to this effect, but on the basis of the following evidence, it seems fairly certain that the figures are in retail prices. In *Sovetskaia torgovlia*, p. 79, there is a table which gives the value of stocks in wholesale trade at the end of 1955 as 25.7 BR, and also states that these stocks would support 20 days of retail trade turnover. Taking 5.47 per cent (i.e., 20 divided by 365) of the retail trade turnover for 1955, we get a figure of 27.5 BR. Though it is not clear just why there should be this small discrepancy, the figures are sufficiently close to each other to suggest that the wholesale trade stocks are valued at prices very close to retail prices.

ary 1, 1949, which is the date of the Census of Business figures for wholesale trade inventories.)

The next step is to convert these stocks into retail prices. As they stand they are valued at cost to the manufacturer or wholesaler. A rough estimate of the mark-up for wholesale trade is obtained by determining the ratio of income generated in wholesale trade to wholesale sales. This turns out to be about 15 per cent,[20] and this, together with the retail trade mark-up of 25 per cent estimated earlier gives an overall conversion factor of 1.44. In figuring this mark-up no allowance has been made for the difference between the cost of goods held by manu-

TABLE 6.—RATIO OF RETAIL SALES TO TOTAL STOCKS IN TRADE, UNITED STATES, 1939 AND 1948

(billion dollars)

	1939	1948
Inventories held by manufacturers	1.5	3.8[a]
Inventories held by wholesalers	2.0	4.4
Total	3.5	8.2
Total at retail prices	5.0	11.7
Inventories held by retailers	6.9	19.8
Total	11.9	31.5
Retail sales	38.5	119.8
Sales-inventory ratio	3.2	3.1

[a] First of year.

Sources: The data for inventories held by manufacturers are from the U. S. Bureau of the Census, Census of Manufactures, 1939, Vol. I, Statistics by Subjects, p. 354, and Census of Manufactures, 1947, Vol. I, General Summary, p. 15. The data for stocks held by wholesalers were computed from data in U. S. Bureau of Census, U. S. Census of Business; 1948, Vol. IV: Wholesale Trade—General Statistics, Washington 1952, p. 22, and Statistical Abstract, 1957, p. 856. The figures for inventories in retail trade and for retail sales are taken from Table 4. The details of the calculations are available from the author on request.

facturers, and the price at which they are sold. But this is probably a small omission. Much of the manufacturers' stock will be sold directly to retailers, and probably the wholesale trade mark-up will be an adequate expression of the difference in these cases. Only in the case where a manufacturer sells to a wholesaler will there be a difference not accounted for. Then, finally inventories in retail trade outlets are added (already marked up to retail prices in Table 4) and then the ratio of retail sales to inventories held at all stages of the distribution system can be calculated. These calculations are summarized in Table 6.

Comparing the results of Tables 5 and 6, the conclusion seems to be

[20] Estimates of income originating in wholesale trade are found in National Income, 1954 Edition, pp. 176-77. Sales of wholesale trade are given in Business Statistics, 1957 Edition, p. 13. For the years for which these series are both available, i.e., 1939 to the present, the ratio of income to sales has been fairly constant, varying between 13 and 16 per cent.

that there is a considerable organizational difference between Soviet and American trade, with a larger proportion of all inventories held by organizations other than retailers in the American than in the Soviet economy. When correction has been made for this difference, the result is sales-inventory ratios which are not far different for the two economies, at least in the postwar period.

2. The second qualification to be considered is the difference in the composition of trade in the two economies. One of the most notable contrasts between trade in the United States and in the Soviet Union is the relative importance of food versus nonfood trade. Trade in food products has characteristically accounted for half or more of total Soviet trade, whereas in the United States the share has been just slightly above one-fourth.[21] Since the sales-inventory ratio differs considerably between food trade and other trade, the figures of Table 4 therefore do not give a true picture of relative inventory efficiency in the two economies. The quantitative effect of this difference on the aggregate ratios is significant. Both in the United States and in the Soviet Union the sales-inventory ratio for food trade is approximately three times that for nonfood trade.[22] If we weight the U. S. sales-inventory ratios for food and nonfood trade in accordance with the relative importance of these two kinds of trade in the Soviet Union, the average ratio would be a little over 7. Thus it appears from this calculation that the Soviet economy takes roughly half again as much inventory to stock a given flow of goods to consumers as is the case in the United States.

No exact quantitative reconciliation of the effects of these two sorts of difference between Soviet and U. S. trade will be essayed here, but the net effect would be more or less as follows. Allowance for the differences in institutional structure eliminates the difference in the ratio indicated in Table 4, where stocks in retail units only were considered. But on the basis of the difference in the composition of trade it is finally concluded that the "structural" ratios underlying the average in the Soviet Union are significantly lower than in the United States.

III. *Conclusion* ✕⟋

Despite the theoretical difficulties and the data problems involved in trying to compare Soviet and U. S. inventory-output ratios, it is highly

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[21] See *Sovetskaia torgovlia*, pp. 52-55, and *Statistical Abstract of the United States: 1957*, p. 844.

[22] For the Soviet Union the sales-inventory ratio for food trade was 2.4 times the sales-inventory ratio in nonfood trade in 1950, 2.8 times in 1954, and 3.5 times in 1955. *Sovetskaia torgovlia*, pp. 48, 50, 88, and 90. In the United States these same ratios were 2.6 in 1951, 2.8 in 1953, and 2.9 in 1954. *Business Statistics, 1957 Edition*, pp. 45-49.

likely that the Soviet system requires higher stocks in relation to flows than does the U. S. economy. This conclusion contradicts Soviet statements on the subject, but with regard to industry, at least, is not at all inconsistent with what is known about the operation of the economy. In particular the poor operation of the supply system makes hoarding almost a rule of economic rationality for the Soviet factory manager.

For anyone who has shopped both in Soviet and American stores, the conclusion in the case of trade is harder to credit. The typical Soviet retail store appears to be very inadequately stocked with goods compared to an American supermarket or department store. But the statistical basis for the conclusion in respect to trade is direct and precise enough to leave little room for doubting it. The explanation must lie in such weaknesses as excessive links in the distribution system, improper distribution of inventories throughout the system, the accumulation of unsaleable stocks, and slowness in transportation.

The implications of these high inventory requirements are not hard to see. The difference between the American ratios and the Soviet ratios represents a tying up of resources in the Soviet economy that could well be used elsewhere. And from another point of view, one particularly relevant to the Soviet case, they represent a burden on economic growth. The excessive inventory increments associated with expanding production are greater than they need to be, and represent a deduction from resources available for investment in fixed capital. It remains only to describe this burden roughly in quantitative terms. It was estimated in Table 1 that the inventories in industry on January 1, 1956 amounted to 227 billion rubles. If the rate of turnover of Soviet inventories were raised to the American level, i.e., if the ratio of output to inventories were raised from say 4 to 6, the inventory requirement in industry would be reduced by a third, i.e., about 75 billion rubles. The magnitude of this waste is seen by measuring it against the total gross investment in the Soviet economy of 150 billion rubles in 1955. At the same time, of course, this wastefulness represents a reserve for the Soviet system: to the extent that these inventories could be freed, they would constitute a source of future growth.

THE FIRM IN ILLYRIA: MARKET SYNDICALISM

By BENJAMIN WARD*

The discussion of the feasibility of socialism has long been closed with apparently quite general agreement that an economy will not inevitably collapse as a result of nationalization of the means, of production. On the theoretical side the clinching argument was probably made by Barone shortly after the controversy began [2]. Probably the best known of the arguments on the other side of the question, that of Mises [15], was published twelve years after Barone's paper and gave rise to a new set of arguments, among them those of Taylor, Lange and Lerner [11] [12]. Lange in fact explicitly (though perhaps with a touch of irony) developed market socialism as a counterexample for Mises' assertions.

Today one might be inclined to take market socialism as something more than a theoretical counterexample. But as a serious proposal for social reform it leaves some important questions unanswered. For example the problem of the emergence of a bureaucracy in whose hands the economic power is largely concentrated was raised by Lange himself. Another unanswered question has to do with the behavioral response of decision-makers to such directives as the rules for determining output and changing price. Will the rules be simply obeyed or will various means of simulating compliance while serving other ends be developed?

These two questions are of special interest today as one watches some Eastern European countries groping toward a less centralized form of economic organization, and as one watches Western European socialists struggle with the implications for democracy (and efficiency) of further nationalization. In the present paper a few of the implications of one possible alternative form of industrial organization are explored. In this model the means of production are nationalized and the factories turned over to the general management of elected committees of workers who are free to set price and output policy in their own material self-interest. The nature of the resulting price and output

decisions are investigated and compared with those obtained in the competitive capitalist (or market socialist) model.

The assumptions of the model bear a close resemblance to the legal status of the industrial firm in Yugoslavia in recent years. Consequently some of the organizational arrangements of a "market syndicalist" economy can be described most conveniently by citing laws on the statute books in Yugoslavia, as is done in Section I. Toward the end of the paper some comments are made as to the extent of deviations of firm behavior in Yugoslavia from that of the theorems of our model. It seems that Illyria is in fact an alternative to the existing system in Yugoslavia as well as to those in Western and the rest of Eastern Europe.

I. *Legal Aspects of a Market Socialist Economy*

The legal framework of the Yugoslav economy has been undergoing such rapid and repeated overhaul during the past seven years that it is difficult to pin down the provisions that are relevant at any one point in time. In what follows reference is mainly to the year 1954. Of first importance in releasing the firm from its former Stalinist constraints was the new planning system.[1] Federal and republican plans no longer prescribed output norms for firms and industries. Figures in the central plan represented generalized expectations rather than explicit norms. The firm itself in its own "independent" plan set its own goals for the year and even then was not penalized for failure to fulfill these targets.

The firm was not only empowered to set its own rate of production but was also made responsible for its sales. The compulsory distribution plan was abolished [17, No. 17, 1952, item 169], and the firm was permitted to enter freely into contracts for the sale of its output and the purchase of raw materials. Prices had gradually been released from central control until here too the firm had the right to price its own products "on the basis of market conditions."[2] Price controls still existed in 1954 but only for a narrow range of commodities.[3]

With output norms no longer available for the purpose, the new criterion of successful performance of the firm became profitability, that is to say, the ability to earn enough in revenue to cover costs at

[1] [17, No. 58, 1951]. The 1954 plan is published in [17, No. 13, 1954, item 146].

[2] This had been carried out for certain classes of firms and industries during 1950-51 (for example price-setting in the case of textile products by the price bureaus of the Ministry for Domestic Trade was abolished by a decree published in [17, No. 48, 1951, item 454]). The general statute appears in [17, No. 32, 1952, item 382].

[3] Prices of some industrial raw materials (e.g., pig and cast iron and sawn timber) were fixed by decree during 1954 [17, No. 20, item 221; No. 26, item 295; No. 32, item 407]. The Federal Price Office was re-established early in 1955 [17, No. 22, 1955, item 225] but there was no significant increase in the number of controlled prices at the time.

the existing market prices. The term "revenue" means roughly what it would mean to an American businessman. However, the term "costs" requires some discussion in view of certain aspects of the wage system and of the fact that the state retained ownership of the means of production.

Labor cost was defined by law [17, No. 52, 1953, item 439] and was based on the average level of skill of workers in the industry. Industries were divided into eight groups on this basis and the labor cost per worker-month set for each group. For example, a coal mining concern, falling in Group III, was assigned a calculated (*obračunski*) wage of 8,100 dinars per worker-month. If the firm employed a staff of 100 for a full month of work, labor cost would be 810,000 dinars for the month. The calculated wage was not the same thing as the contractual wage, which was the basis on which a worker was hired. This latter could be set freely by the firm. The distinction was that the contractual wage was *not* an accounting cost and was set by the firm, while the calculated wage *was* an accounting cost and was set by the state.

Secondly, there was the problem of charging the firm for its use of the state-owned land, plant and equipment. Ground rent was to be charged industrial firms at the same rate as that charged on the "largest class of arable land in the district" [17, No. 53, 1953, item 456]. The latter was set on a cadastral basis in accord with the yield of the land. Capital, that is to say plant and equipment, including the more expensive tools, was revalued during 1953 on a basis which does not seem to have been described explicitly in the press.[4] The social (i.e., federal) plan was then to set each year the rate to be paid to the state as interest on fixed capital. A standard rate of depreciation on the various types of equipment was prescribed by the state and payments sufficient to maintain the value of the equipment were also charged as costs against the firm [16, pp. 128-32, 165-67].

Under this system then, costs would be the sum of material costs, "regular contributions" to the state (i.e., the interest charge on fixed capital, ground rent and the excise tax on sales, where levied—a relatively insignificant item in 1954), the calculated wage fund and interest on short-term credit outstanding. Profits, i.e., revenue less costs so-defined, became the measure of success of the firm.

To lend point to this change in perspective, a bankruptcy law was promulgated.[5] Several types of receivership were defined, but in general it was provided that a firm became bankrupt if it was no longer able to

[4] D. Misić [14] says that capital was to be valued at its "real present value, taking account both of its economic obsolescence and the extent to which it is worn out."

[5] [17, No. 51, 1953, item 425]. An earlier law [17, No. 57, 1951, item 545] is much less specific and does not define the conditions under which a decree of bankruptcy against a firm will be passed.

make its regular payments to the state and to pay wages out of its revenues at the rate guaranteed by the state.[6] Included was a provision permitting reorganization of the firm after writing down existing debts, provided the creditors were agreeable.

The new organization was designed to increase the efficiency of the economic system via competition among firms. As Vice-President Kardelj put it, ". . . stimulative elements . . . appear above all through the interest of the enterprise in achieving, through free competition with other enterprises on the market, the best results as regards quality and quantity of goods, lower costs of production and good marketing" [10, p. 135]. The firm's incentive to participate in this competition with its fellows stemmed from two sources: workers' management and a profits-sharing scheme. The former had been established in 1950 [17, No. 43, 1950] and provided for an elected council of workers in the firm which was to serve a general policy-making function. The council approved the independent plan of the firm and the wage schedule and was empowered to issue directives regarding execution of the plan and the management of the firm. These were binding upon the firm's director, providing they did not conflict with existing laws and decrees. Day-to-day supervision of operations was entrusted to the management board (*upravni odbor*),[7] a subcommittee of the workers' council which also prepared drafts of the plan and the wage schedule for the approval of the workers' council. Differential wages within the firm were thus set by the workers themselves under this law and the later planning law, the chief constraint being that no wage rate could be set below that in the state minimum wage law.[8]

The calculated wage rates were supposed to be set at levels which would add up to 90 per cent of the total contractual wages at the planned production rate and sales price. If the workers were to receive the contract wage then it was necessary for the firm to make a profit on its operation [1, p. 44]. Furthermore, any profits achieved by the firm were placed at the disposal of the workers' council to be used either for investment or rationalization or to be paid out as a wage supplement in proportion to the contract wage received by each worker,[9] though a

[6] In 1954 the state guaranteed up to 80 per cent of the calculated wage fund of the firm. A firm could apply to the state bank for a loan to cover up to 90 per cent of this fund, but the bank could refuse the loan if it thought the chances of repayment were not good. Guarantee of the loan by the local government (*narodni odbor* or "people's committee") was often required [17, No. 5, 1954, item 57].

[7] The director was a member ex officio of the management board.

[8] [17, No. 7, 1952, item 108] [17, No. 56, 1953, item 484]. Worker skills were classified in [17, No. 57, 1950, item 508], and minimum compensation fixed for each grade.

[9] There are several qualifications to this statement. Some portion of the profits was to be used for the building up of a reserve fund and the local government received a share as well [1, p. 44].

steeply progressive profits tax was levied on this supplementary wage
fund.

While the Yugoslav economic system thus involves a considerable
measure of autonomy for the firm, it should not be thought that inde-
pendence of the sort possessed within the legal framework of capitalism
has been acquired by the Yugoslav firm. The state reserves the right to
intervene directly to alter any decisions of which it disapproves.[10] Such
intervention could occur legally as a result of new decrees of the gov-
ernment or by means of the exertion of influence via the trade unions,
the League of Communists, or the local governments, rights whose legal
sanction was often based on the right of approval of the firm's de-
cisions.[11] But intervention was now to be viewed as the exception rather
than the rule.[12]

II. *The Competitive Firm: The One Output-One Variable Input Case*

The Illyrian firm operates in an environment rather similar to the
legal environment within which the Yuglosav firm operates. In Illyria
however there will be no intervention by the state in the firm's decision-
making process, nor does minimum wage legislation exist. The worker-
managers are free to set firm policy under the influence of the profit
incentive.

The firm to be considered in this section operates in a purely com-
petitive market. Decision-making is concerned with the short run and
is viewed as static in nature; that is, the worker-managers are inter-
ested in maximizing their individual incomes over a given period of
time. The services available to the firm are labor, which is a homogene-
ous input, and a fixed plant, which is owned by the state and operated
by the workers. The firm must pay a tax in the form of interest on the
replacement cost of the plant. Ground rent, depreciation, working capi-
tal and other taxes will be ignored. The state sets the calculated wage
rate w, but this is done merely to provide an accounting definition of
labor cost and does not determine in fact the level of wages.[13] The
workers never plough their profits back into the firm, but in each period

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[10] An official statement in vindication of the use of this right by Vice-President Kardelj
can be found in [10, p. 133].

[11] The people's committee had the right of approval of the firm's independent plan (see
planning law cited above, footnote 1) and the trade unions had some special rights of
intervention in the hire-fire decision [17, No. 26, 1952, item 306].

[12] Such action is termed "administrative intervention" by Yugoslav economists and is
asserted to have been ubiquitous under the previous Stalinist form of economic organiza-
tion. A principal reason for establishing the new system was to make such actions unneces-
sary. See for example [13, pp. 95-100, 113 ff., 131-32, 224 ff.] [18, pp. 238 ff.].

[13] In Yugoslavia the setting of the calculated wage performs an important function in
determining the portion of the firm's wage bill that comes under the progressive surplus
profits tax, but we are ignoring this tax in the Illyrian case.

distribute the whole amount of profits as a wage bonus. In our firm, which employs a single skill-type of worker, the distribution is made equally to each employee.

A production function will describe the technical conditions under which the firm may transform the homogeneous factor labor, x, into a salable product, y:

(1) $$y = f(x).$$

Over the range of the variables under consideration the marginal product of labor will be assumed to be positive but declining as output increases. Labor input will be measured in terms of the number of workers employed. By assuming that labor input can be changed only by varying the number of laborers the possibility of overtime work by the existing staff is eliminated. This is done so as to avoid introducing the marginal disutility of labor as an important constraint.[14] It is also assumed that there is no discrimination among workers, and that a decision to lay off workers on profit-maximizing grounds would not be affected by the fact that the result would be to create unemployment.[15]

The sole source of income to the firm is from the sale of its product at the parametric price p. Two costs are incurred in production: labor cost which is valued at the calculated wage w per worker,[16] and the fixed charge for the use of capital R. Profit of course is the difference between revenue and cost. The worker-managers, acting in their own material self-interest, are not necessarily interested in maximizing profits as their capitalist counterparts, the stockholders or entrepreneurs, would be. Each worker is interested in maximizing his own wage income. The workers as a group, corresponding to the group of stockholders in capitalism, are interested in adopting policies which will maximize

(2) $$S = w + \frac{\pi}{x}$$

where π represents profits.

The last term of equation (2) can be divided into two parts since

[14] The Yugoslav wage law cited above provides that time-and-a-half be paid for overtime work, but that such work cannot be paid for unless prior authorization has been obtained from the local government. Apparently there was a tendency to hog the work which, reasonably enough, was frowned upon by the authorities in a labor surplus economy.

[15] The management board has the final decision in the matter of hiring and firing in the Yugoslav firm (with the exception noted above, fn. 11). If it is assumed that the board is composed of workers of relatively long tenure in their employment in the firm, so that they would not be personally affected by a decision to reduce output, aside from the favorable effect on their own income, this assumption may seem reasonable.

[16] All workers and employees, including those whose wage cost to the firm would ordinarily be considered as overhead, are included in the wage cost wx, as a matter of convenience.

average profits per worker consists of the difference between average revenue per worker, U, and average cost per worker, K. The firm will then choose that output which will make the positive difference between U and K a maximum. This would be the output at which

(3) $$dU/dy = dK/dy.$$

This is the Illyrian equivalent of the capitalist condition that price will equal marginal cost under rational management, or of the market socialist rule that managers act so as to set marginal cost equal to price. The Illyrian condition states that wages per worker (or, what amounts to the same thing, profits per worker) are maximized if the competitive firm chooses the output at which marginal revenue-per-worker equals marginal cost-per-worker.[17] This condition has more in common with the capitalist "rule" than with the Lange-Lerner rule. For the Illyrian rule represents the *result* of behavior of a specified kind (wage-maximizing behavior), as does the neoclassical rule (profit-maximizing behavior). In the market socialist economy of the Lange-Lerner type however, the managers are *directed* by the state to act in a certain way, the rule not being connected explicitly with the motivations of the managers.

Equilibrium for the Illyrian competitive firm is described graphically in Figure 1, where the values of U and K are plotted against x. The solution is not altered by making x rather than y the formal choice variable. U has its maximum value at the point at which marginal and average product are equal[18] and declines as the number of workers is either increased or decreased from this value. K, representing average costs per worker, is equal to

$$w + \frac{R}{x}.$$

This curve is a rectangular hyperbola asymptotic to $x = 0$, $K = w$. Profits per worker reach a maximum when the difference between U

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[17] Marginal revenue-per-worker, it will be noticed, is not the same thing as marginal revenue per worker. The former measures the change in average revenue per worker brought about by a small change in output, while the latter measures the average marginal revenue per worker. In symbols, marginal revenue-per-worker is:

$$\frac{d(py/x)}{dy} = p \cdot \frac{x - yx'}{x^2}$$

while marginal revenue per worker is:

$$\frac{d(py)/dy}{x} = \frac{p}{x}.$$

[18] From the preceding footnote it can be seen that marginal revenue-per-worker will be zero when x/y equals x'. The shape of the production function ensures that this will be a maximum value for U.

and K is greatest, which is the value of x for which the slopes of U and K are equal. This is point b of Figure 1.

What is the meaning of this equilibrium? How does it compare with the equilibrium position of the traditional firm? We may consider first the effects of changes in the parameters on the Illyrian firm's behavior, and then contrast the equilibrium positions of Illyrian and capitalist firms under similar technological and market conditions.
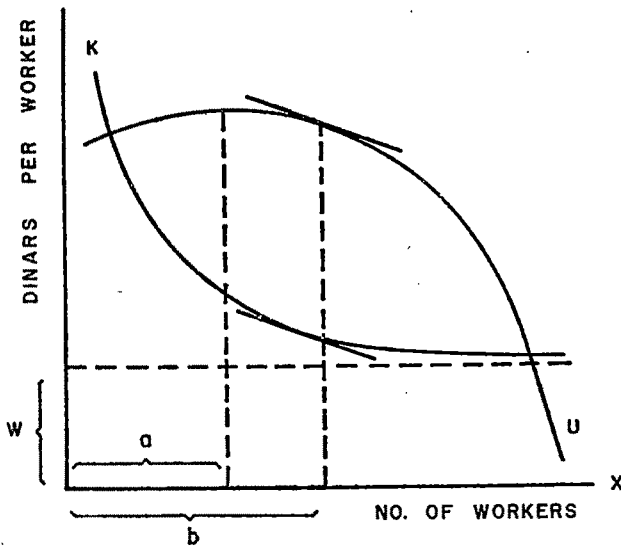
FIGURE 1

Referring to Figure 2, suppose that the firm is in equilibrium producing, under revenue and cost conditions represented by U_1 and K_1, an output corresponding to the level of employment a. The state now raises the interest rate, so that R is increased. This shifts the cost curve up to K_2. But at the output corresponding to a, curve K_2 is steeper than is U_1.[19] That is to say, at employment level a the rate of decrease of average cost per worker is greater than the rate of decrease of average revenue per worker. Consequently it will be to the workers' advantage to raise output until average cost and average revenue per worker are decreasing at the same rate. In Figure 2 this is represented by employment level b where the slopes of U_1 and K_2 are equal. This result can be generalized into the theorem: *A change in the fixed costs of the competitive Illyrian firm leads to a change in output in the same direction.*

[19] Since $K = w + (R/x)$, $dK/dx = - R/x^2$. Therefore, if $R_2 > R_1$, $|dK_2/dx| > |dK_1/dx|$ at $x = a$.

Further increases in R would lead to further increases in output. If K_3 were the relevant cost curve the firm would be earning zero profits. Even if R were increased beyond this point output would continue to increase, as the worker-managers strove to minimize losses. Under these circumstances the workers would be receiving less than the calculated wage w. So long as no better alternatives were available elsewhere the workers would continue to work in the given firm despite this fact, under our assumptions.[20] Decreases in R of course have the opposite effect. At $R = 0$, the cost function becomes $K_4 = w$, and output would
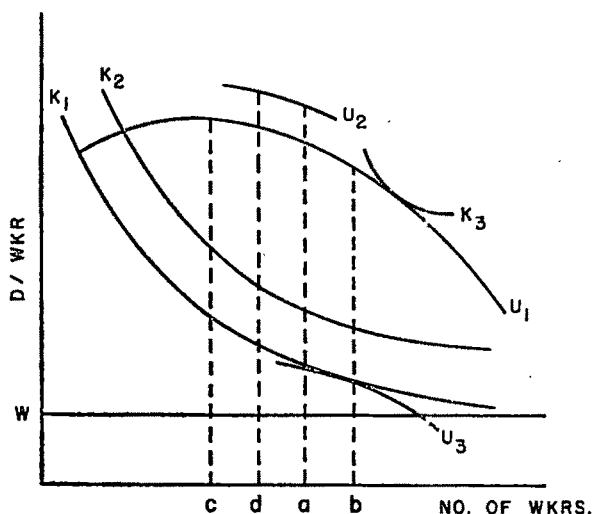
FIGURE 2

be at the level corresponding to the maximum value of U_1. A negative interest rate would convert K into a hyperbola asymptotic to the same lines as before but located below w on Figure 2. Employment would be less than c and the competitive Illyrian firm would be in equilibrium with average costs falling.[21]

Price changes may be considered in a similar way. Suppose that an increase in demand for the industry's product leads to an increase in the market price p of our firm, which is currently in equilibrium at

[20] In Yugoslavia wages up to 80 per cent of the calculated wage are guaranteed by the government. If this were true of Illyria, then at outputs beyond that which yielded $0.8w$ to the workers the maximization criterion would cease to apply. Continued operation at such a level would eventually lead to bankruptcy.

[21] As in capitalism this would only be true over the range in which marginal product was declining. Beyond that range the second-order condition for equilibrium would not be satisfied, so that if a solution existed it would not be a maximum. It may also be noted that over this range of values the supply curve would be positively sloped.

employment level a of Figure 2. This will shift U_1 upwards to position U_2. But at the current employment level U_2 will be steeper than K_1.[22] That is, at a the rate of decrease of average revenue per worker is greater than the rate of decrease of average cost per worker. Output and employment will contract until these rates are again equal as at employment level d. Our theorem is: *A change in price to the competitive Illyrian firm leads to a change in output in the opposite direction.*

The lower limit to a price-induced output contraction is, roughly speaking, at employment level c where average and marginal product are equal. If falling price were to shift the revenue curve down to U_3 a zero profits position would have been reached. The remarks above regarding operations at a loss would of course apply equally if falling price rather than rising fixed costs were the cause of the losses.[23]
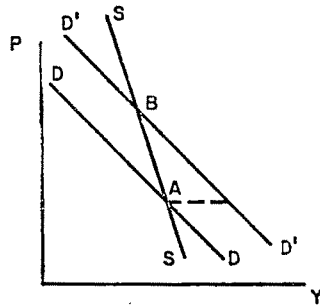
FIGURE 3

Under the usually hypothesized market and technological conditions the Illyrian competitive firm possesses a negatively sloped supply curve. This does not mean however that Illyrian competitive markets are inherently unstable. For example, Figure 3 depicts the industry supply

[22] $dU/dx = (p/x)[y' - (y/x)]$. Hence if $p_2 > p_1$ then $|dU_2/dx| > |dU_1/dx|$ at $x = a$.

[23] The effects of changes in p and R can perhaps be seen more clearly by considering the equilibrium condition:

$$(4) \qquad \frac{dS}{dx} = \frac{p(xy' - y) + R}{x^2} = 0$$

or

$$(5) \qquad \frac{y}{x} - y' = \frac{R}{px}.$$

Thus the right-hand term of (5) measures the difference between average and marginal product in equilibrium, which will be positive (decreasing average product) if R is positive. But the difference between average and marginal product is a monotonic increasing function of output beyond the point of maximum average product (at which point the difference is nil). So, from equation (5), if R is increased, the difference between average and marginal product, and hence equilibrium output, will be increased. On the other hand an increase in p means a decrease in the difference between average and marginal product, and hence a decrease in equilibrium output.

and demand curves in such a market. If demand were to shift from DD to $D'D'$ point A would no longer be an equilibrium position. If this is a "price-adjusting" market in the usual sense, the adjusting mechanism is such that the direction of movement of price over time has the same sign as the amount of excess demand. In the diagram excess demand is now positive, so price increases and eventually equilibrium is restored.

On the other hand, if the demand curve has a steeper slope than the supply curve the adjusting mechanism described above will lead away from equilibrium and the market will be unstable. To be assured of stability this possibility must be avoided, which means that some further constraint must be imposed on the structure of the firm specified above.[24] The problem of instability is most likely to arise when product demand is relatively inelastic, or when marginal product is relatively large and declining slowly as output increases.

If the state changes the calculated wage w there is no change in any of the variables relevant to the firm. The K function $(cf.$ Figure 1) shifts vertically up or down as a result. The income of the workers is unchanged, though relatively more income is in the form of profits (if w is reduced) and relatively less in the form of wages.[25]

The Illyrian equilibrium can now be contrasted with its capitalist counterpart. Consider two firms, one in Illyria, the other in a capitalist country. They have identical production functions and are operating in purely competitive markets. In addition, market prices are equal in both cases, as are fixed costs, and the Illyrian calculated wage w_I equals the going capitalist wage w_C. In Figure 4 the U and K functions describe the revenue and cost positions of the Illyrian firm under alternative levels of employment. The rates of change are also drawn in. At the intersection of the latter the Illyrian firm is in equilibrium, producing the output corresponding to employment x_I.

In describing the equilibrium of the capitalist firm it will first be noted that U also expresses the value of the average product of the capitalist firm under our assumptions, since $U = py/x$. The capitalist value-of-the-marginal-product function bears the usual relation to U, and the capitalist output is found at the point x_C where VMP equals the wage, since output y_C is a single-valued function of labor input.[26]

In the diagram the capitalist output exceeds that of the Illyrian firm. But this need not be the case. For example, by increasing w_C it would be possible to reduce the equilibrium output level of this firm to the Illyrian level or even below. Under our assumptions a necessary and

[24] See the Mathematical Appendix.

[25] See note 13 above.

[26] We are assuming that the capitalist firm too can vary only the number of workers employed and not the hours of work.

sufficient condition that the outputs of the two firms be equal is that the equilibrium marginal products be equal. The capitalist value of the marginal product is equal to w_c. In Illyria the value of the marginal product is equal to the "full" wage, i.e., the calculated wage plus the profits share to each worker.[27] Therefore the Illyrian full wage equals the capitalist wage and equality of outputs implies zero profits.[28]
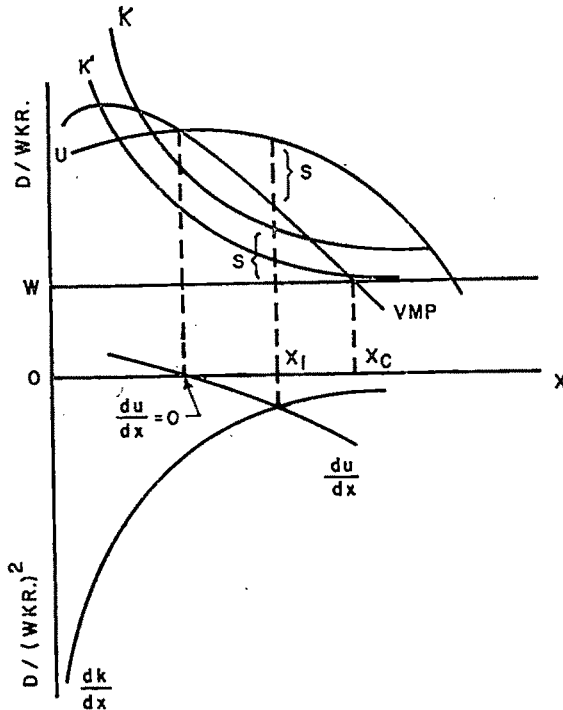
FIGURE 4

Thus the Illyrian firm is capable of producing in the short run at a level equal to or even greater than that of its capitalist counterpart. And the state can affect output decisions of the firm via its ability to alter the parameter R. If it is willing to use the fixed tax for capital use as an instrument of policy in attaining desired levels of output, and consequently is willing to make discriminatory charges on this basis, it may create an environment in which it is in the material interests of the worker-managers to produce at the competitive capitalist output, or at some other preferred rate. Alternatively, if the industry were in long-

[27] From equations (2) and (5), $S = (py - R)/x = py'$ in equilibrium.

[28] We assumed at the start that $w_I = w_c$. Since the value of w_I really does not make any difference, a more significant statement would be: equality of outputs implies equal wages.

run equilibrium in both countries and demand, labor force, etc., conditions were identical, both firms would produce the same output.

Finally, the case of constant average product y/x may be noted. In capitalism this means one of three things: (1) if $VMP > w$ the firm produces at capacity; (2) if $VMP < w$ the firm produces nothing; and (3) if $VMP = w$ output is indeterminate. In the Illyrian case this means that U is a horizontal line. The maximum positive, or minimum negative, difference between U and K consequently is at infinity whatever the position of U on the diagram. The Illyrian firm produces at capacity when marginal and average product are equal.

III. *The Case of Two Variable Inputs*

In Illyria a single class of inputs, labor, is singled out for special treatment. The distinctive features of Illyrian behavior stem entirely from this fact. By extending our previous model to include the use by the firm of a variable nonlabor input, the special position of labor in the firm can be brought out more clearly. The production function will now have the two arguments,

$$(6) \qquad\qquad y = f(x, z).$$

If the usual assumptions of positive marginal products and diminishing returns to the factors are made, the equilibrium condition for labor use will correspond to that in Section II, i.e., the value of the marginal product of labor will be equal to the full wage. For the nonlabor input however the value of the marginal product will be equal to the price v of the input.[20] The workers react to changes in nonlabor inputs in the same manner as do capitalists: they will increase their use of the factor as long as it contributes more to revenue than to cost. On the other hand they seem to use a different criterion in evaluating labor use. An additional laborer must contribute more to revenue per worker than to cost per worker in order for him to be employed. In fact, *only* the la... criterion is being employed in the model. It simply happens that the capitalist and Illyrian criteria lead to the same behavior with regard to nonlabor inputs. Whenever one of these factors contributes more to revenue than to cost it also contributes more to revenue per worker than to cost per worker. As a result the equilibrium conditions are the same. However the two criteria do not lead to the same behavior when it comes to labor use. Because each laborer gets a share of the profits it does not follow that an additional worker who contributes more to revenue than to cost will necessarily also contribute more to revenue per worker than to cost per worker. As a result the equilibrium conditions for labor use are not the same in the two regimes.

An analysis of the effects of changes in the parameters R and p leads

[20] See the Mathematical Appendix for derivations in the two-variable-input case.

to less clear results in the two-input case than it did in Section II: In the case of a change in fixed costs the analysis may be illustrated by means of the factor allocation diagrams of Figure 5. The curves in 5A are drawn on the assumption of a fixed input of factor z and those in 5B on the assumption of a fixed level of employment. From an initial position of equilibrium in which x_1 of x and z_1 of z are being used, fixed cost is increased. This shifts K_1 upward to K_2, increasing labor input from x_1 to x_2, and consequently tending to increase output. However, there is now an additional effect which must be taken into account: namely the effect of the increase in labor use on the marginal product of the nonlabor input. If the latter is unaffected or increases, shifting VMP_1 upwards to VMP_2, the increase in output is either unaffected or
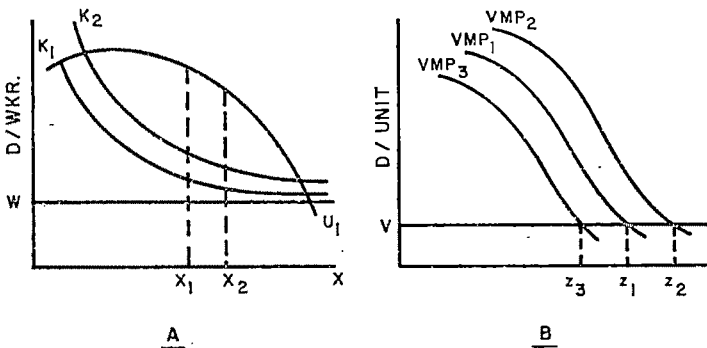
FIGURE 5

magnified. However, if VMP is reduced by the increased labor use the amount of z used decreases, and the output effect of the increase in fixed cost is indeterminate by means of qualitative analysis alone. The latter, however, is a rather unlikely eventuality, since in the short run more labor will generally not decrease the usefulness of the other variable factors, and conversely. Consequently a change in fixed cost in the multifactor case will also tend to lead to a change in output in the same direction.

A more serious indeterminacy appears in the analysis of price changes. Without a good deal more information it is not possible to state the effect on output of a change in price. The possibility of a positively inclined supply curve emerges clearly however, and some presumption that the danger of instability, resulting from a negatively inclined and relatively elastic supply curve, has diminished. Whether or not a negatively sloped supply curve will result in the multifactor case depends on the relative importance of labor in the bill of inputs.[30]

Similarly, changes in the parameter v, the supply price of the non-

[30] Cf. equation (20) in the Appendix.

labor input z, have indeterminate effects on output. This is also true in the case of analysis of the capitalist firm with the same amount of information, though information sufficient to remove the indeterminacy in one case may not be sufficient in the other.

The statements made in Section I comparing competitive capitalism with competition in Illyria generally apply in the somewhat more complicated two-variable-input case. We will consider here the problem of comparative factor allocation. As before our two firms have identical production functions and are operating under identical market conditions so that:

$$p^I = p^C$$
$$w^I = w^C$$
$$v^I = v^C$$
$$R^I = R^C$$

the superscripts standing for "Illyria" and "Capitalism" respectively.

The situation is described in Figure 6 in which isoquants Q_i which are identical for both firms are drawn. Let us assume first that the capitalist firm is producing output Q_1. BB is the factor-cost line based on the values of w and v, so that the capitalist firm is in equilibrium at a factor mix represented by point N. Let us assume further that the capitalist firm is earning a profit at this level of operation. At the same output the Illyrian firm would be earning a profit too. But it would not be in equilibrium at point N. This is because BB is not the relevant factor cost line for the Illyrian firm. Since in Illyria the value of the marginal product of labor is equated to the full wage, i.e., including the profits share, BA, representing a larger wage "cost," is the relevant one for the Illyrian allocation decision. The Illyrian firm is in equilibrium then at point M, producing less output and using less labor than its capitalist counterpart.

Suppose now that market price falls to the zero profits point. Capitalist output and factor mix contract along Y^o, say to point L. Illyrian output and factor mix contract along Y^I, but also to point L, since the zero-profits full wage is equal to w. If price should fall further so that both firms are incurring losses the full wage will then be less than w. For example, under conditions which would lead the capitalist firm to produce at H, the Illyrian firm would produce at J. The Illyrian firm would produce more than the capitalist firm and would use more labor, so as to spread the losses around among as many of the worker-managers as possible.

The Y^I line, like Y^o, is positively sloped in the diagram, indicating that supply responds positively to an increase in price. It is perfectly

possible for Y^I to have a negative slope under suitable cost and technological conditions, but it will still intersect Y^o at the zero-profits point.[31]

As a final aspect of the multiple-input case we may consider a firm which is highly automated so that labor does not enter significantly into the short-run production function as a variable input. In this case fac-
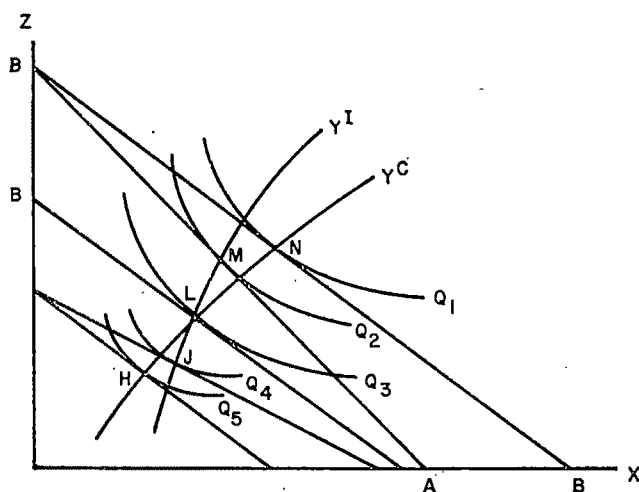
FIGURE 6

tor use and output are determined by the usual equilibrium conditions of capitalism. That is, with a fixed labor force any addition to profits is also an addition to profits per worker. Such a firm would behave in exactly the same way as its capitalist counterpart, equating marginal cost to price and the marginal value products to the fixed input prices.

IV. Market Imperfections

Illyria must pay a price for its decentralized pricing system in the form of imperfect markets. For the worker-managers no less than their capitalist counterparts have an incentive to profit from the negatively inclined demand schedule that must in many instances confront them. The whole congeries of market types from monopoly to monopolistic competition, including the usual forms of collusion, could emerge under Illyrian conditions. Alterations in the previous models required to take

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[31] Figure 6 may also be used to contrast other comparative static changes. For example, an increase in w will increase the slope of BB without affecting that of BA. This will tend to move the capitalist equilibrium position at N closer to the Illyrian at M. An increase in R on the other hand will tend to make BA less steep without affecting the slope of BB. This will tend to move the Illyrian equilibrium position at M closer to the capitalist at N. When the equilibria coincide in either case profits will be zero.

account of such market imperfections in most instances are not extensive. For example, the monopoly solution in the single-variable-input case can be discussed by means of Figure 2 (p. 574) if the U function is reinterpreted to take account of the fact that price is now a variable rather than a parameter of the system. That is, any point on U would now represent the average revenue that would accrue to the monopoly from selling the output the x workers are capable of producing at the price offered for that quantity by buyers. The result is that factors are used up to the point at which the marginal revenue product equals the (assumed perfectly elastic) supply price in the case of nonlabor inputs, and to the point at which marginal revenue product equals the "full" wage for labor inputs.[32] Output will be less than in competition under the usual conditions.

There is one factor market which exhibits a special rigidity in Illyria; namely the labor market. The situation can be illustrated by assuming away all the customary forms of rigidity in the labor market such as trade unions, barriers to training, imperfect knowledge, etc. If labor is a homogeneous factor well informed and concerned to better its material position as far as possible, we may assume the supply to depend solely on the wage offered. If profits figures are published or otherwise available it is the full wage that will determine the offer of labor power. Suppose that from an initial position of equilibrium the supply schedule for labor shifts up, leading to excess demand. Ignoring the influence of varying employment levels on product demand, the marginal firms are now unable to obtain labor at the going wage rate. But they are also unable to offer higher wages, since the wage rate was already at a maximum. As a result of the rigidity of the wage offer there will be no forces set in motion in the short run to correct a disequilibrium, and the shortage of workers will in itself tend to depress the wage paid by the marginal firms, since profits can no longer be maximized.

If excess supply of labor should develop, a similar rigidity would occur. Workers are willing to offer themselves at lower wage rates, but these rates cannot legally be paid. Consequently the excess supply will persist. Only entry or departure by some firms would be capable of changing the situation; i.e., by changes in the quantities demanded in each case rather than by alterations in the market price of labor.

If profit rates were kept secret it might be possible to create a supply function in which the nominal wage w_1 was the independent variable. In this case, when excess demand appeared the state could raise the nominal rate sufficiently to attract the needed number of workers into the market. This change in the wage would not affect the full wage or

[32] See Appendix, note 2.

the product market positions.[33] That is, it would affect only the supply of labor, not the demand. However it would be rather difficult to keep all information regarding profits from the labor market when all recipients of profits or losses were also workers.

V. *Concluding Remarks*

1. The zero-profit output of the competitive Illyrian and competitive capitalist firms will be identical, given the same market and technological conditions for the two firms. This suggests that in the long run the Illyrian conditions under competition could lead to an optimal allocation of resources wherever the capitalist competitive regime would. However we have not discussed conditions of entry in Illyria. Entry could occur either by creation of new firms by the state, or by expansion of existing firms, or by some provision for individual or decentralized group initiative in starting new enterprises. All three possibilities exist in Yugoslavia but it is not possible to discuss them here. It will merely be noted that there is likely to be strong resistance by the Illyrian worker-managers to ploughing back profits, since this would involve a reduction in the current profits share. This will be true if relatively low-income entrepreneurs are likely to be more myopic than relatively high-income ones. Something additional to worker self-interest might well be necessary in the Illyrian environment to ensure entry equivalent to that under capitalism.

2. Market imperfections stemming from the ability of the seller (or buyer) to influence the market price by varying his rate of output lead generally to a lower level of output and a higher price than the competitive rates, in Illyria as in capitalism. This has been a persistent problem in Yugoslavia from the beginning of the new system [7, p. 16] [4, 1953, 2, 443-44] [4, 1954, 3, 841-42] and has led to the promulgation of an "antitrust" law.[34] The state's broad rights of intervention might seem to offer more favorable opportunities for controlling such behavior, but persistent complaints in the Yugoslav press suggest that control of monopoly has not been notably successful.[35] Of course Yugoslavia, being a rather small country with an underdeveloped industrial sector and communications network, and with a balance-of-payments

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[33] However, with a progressive profits tax, as exists in Yugoslavia, the full wage will be reduced, *ceteris paribus*, by a decrease in w, since a larger proportion of the wage bill would become taxable; and of course the full wage would increase with an increase in the calculated wage.

[34] [17, No. 56, 1953, item 483, esp. Article 74]. The language is fully as vague as that of the Sherman Act, among other things forbidding firms from doing anything which leads to a "monopoly position in the market."

[35] See however [4, 1953, 2, 1034] for a description of the refusal of a Yugoslav court to uphold a contract which was in restraint of trade.

problem which has led to a large number of restrictions on competitive imports, has considerable initial disadvantages to overcome in developing a purely competitive market.

3. A special stability problem arises in Illyria, since firms may react to a price change by altering the rate of output in the opposite direction (negatively sloped supply schedule), a situation which is more likely to occur the more important labor is as an input. Since a good deal of new plant has been installed in Yugoslavia since 1952, this would be a difficult hypothesis to test. Indeed, our model does not tell us anything at all about what to expect if there is in fact instability in a market. In addition it is probable that in a large number of Yugoslav firms policy decisions are made by the director without much reference to the wage-maximizing desires of the workers. Though directors also share in the profits, it is likely that other motives also exist for them which might lead to different behavior as regards price and output and input policy.[36]

4. The beginning student of economics would undoubtedly be delighted to learn that in Illyria an increase in fixed costs would really lead to an increase in output so as to spread out the increased burden over a larger number of units. I have seen no evidence to indicate that the Yugoslav authorities have varied the interest rate on existing capital for purposes of output control, though discrimination has been practiced which is consistent with such an aim.[37]

5. The labor market possesses a rigidity which prevents adjustment of supply and demand to restore a displacement from equilibrium. It is true that in Yugoslavia there has been some excess supply of labor at least since the institution of the new economic system in 1952 [4, 1953, 2, 182-83] [9], though this can be explained as well by other factors operating generally in a labor market in an underdeveloped country (and by the successive droughts) as by means of this theorem. In this excess-supply situation some incentive probably exists for the firm to practice a form of illegal discriminaton which is related to the specific rigidity described above. This could be done by means of an illegal contract to hire workers who would agree to work for the contractual wage rate for unskilled workers and renounce their right to profits.[38]

[36] See [19] for a discussion of the relative influence of director, management board and workers' council within the firm. For example, directors are often in close contact with local government officials. One possible result of ties of this kind might be profit-maximizing behavior, as mentioned in note 38 below.

[37] [16, pp. 132-33]. In the 1954 plan, for example, the standard rate was 6 per cent, but in some types of construction no interest at all was charged, while elsewhere a rate of 2 per cent was charged. It is certainly true that the state was specially interested in increasing output in the favored areas. It does not necessarily follow however that the planners had the mechanism operative in our model in mind in granting the favors.

[38] A phenomenon known as the "dead brigades" (*mrtvi brigadi*) may have been an instance of this [4, 1953, 2, 1034] [9, p. 487]. For example the coal mining concern men-

Finally, outside pressure by various groups, but especially by the people's committees, the organs of local government, seems to have had considerable effect on decision-making by the firm in Yugoslavia. We will note only one example. The people's committees possess the right to a portion of the profits of every firm within their territory. This has led them to influence the firms' policies and to take other steps to siphon funds from the firms into the treasury of the local government.[39] The actions of local governments and other administrative bodies, plus fears of administrative intervention, undoubtedly result in significant differences in the behavior of firms in Yugoslavia and Illyria. These factors, plus the relatively large role the director seems to play in *de facto* decision-making within the Yugoslav firm, provide the chief limitations to the application of the above analysis to current operations in Yugoslavia. The model's relevance to Yugoslavia may be somewhat increased if it is assumed that the legal framework is descriptive of an ultimate purpose on the part of the Yugoslav leadership.

In summary, market syndicalism differs from market socialism in that in the former (1) both price and output decisions are decentralized to the level of the firm; (2) the workers employed in each firm control policy making; and (3) material interest is the governing incentive. Some of the arguments in favor of market syndicalism as a nonbureaucratic alternative to other forms of socialism would bear a striking resemblance to those of the economic liberal when attacking some tendencies of contemporary capitalism; and conversely for arguments against market syndicalism. However it would not be seemly to discuss these issues until some other properties of the Illyrian economy, such as the investment decision, multiple market stability and macroeconomics, have been investigated.

MATHEMATICAL APPENDIX

1. *The Condition for Stability in a Single Competitive Illyrian Market under the Assumptions of Section II.*

From equations (2) and (3) in the text

(7)
$$S = \frac{py}{x} - w - \frac{R}{x}$$

tioned on p. 568 above might hire an unskilled worker for 6000 dinars per month, which would add 8100 dinars to the calculated wage fund, i.e., to labor cost in the accounting sense. This would substantially reduce accounting profits and hence the amount of taxation under the steeply progressive profits tax law. If the firm expected to make a fairly high level of profits, this could be to the monetary advantage of the "in-group" workers even if the newly hired worker performed no work at all. From the above descriptions it seems that the dead brigades in fact had little to do. In some cases the dead brigades were in fact "dead souls," fictitious employees.

[39] [8, p. 159] [5]. The people's committee would be interested in the level of profits per se, rather than in profits per worker, so that within the framework of our model such disagreements over price-output policy could arise.

so that

(8)
$$\frac{dS}{dy} = \frac{p(x - yx') + Rx'}{x^2} = 0$$

where $x' \equiv dx/dy$. The equilibrium condition $S' = px - pyx' + Rx' = 0$ may be differentiated with respect to the parameter p:

$$\frac{\partial S'}{\partial p}\bigg|_R = \frac{dS'}{dy}\frac{\partial y}{\partial p}\bigg|_R + \frac{\partial S'}{\partial p}\bigg|_{y,R} \equiv 0$$

$$= (-pyx'' + Rx'')(\partial y/\partial p) + (x - yx') \equiv 0$$

or the slope of the firm's supply function:

(9)
$$\frac{\partial y}{\partial p} = \frac{yx' - x}{x''(R - py)}.$$

But $(R - py) = -pxy'$, since the equilibrium is preserved along the supply function. So supply elasticity:

(10)
$$\eta_S \equiv \frac{p}{y}\frac{\partial y}{\partial p} = \frac{x'}{xx''}\left(\frac{x}{y} - x'\right) < 0,$$

except over the relatively unimportant range in which average product is equal to or less than marginal product. Note that

$$x'' \equiv \frac{d^2x}{dy^2} = \frac{y''}{(-y')^3} > 0, \quad \text{where} \quad y'' \equiv \frac{d^2y}{dx^2}.$$

We are assuming a price-adjusting market in which the existence of excess demand leads to price increases over time in the case of excess demand and of decreases in the case of excess supply. Such a market will be stable when the supply curve is negatively sloped provided $\eta_D > \eta_S$. If we assume that production functions of all firms are identical, elasticity is invariant under the summation from firm to industry supply function. Thinking then in terms of industry demand and firm supply conditions, we have:

(11)
$$\eta_D > \eta_S = \frac{x'}{xx''}\left(\frac{x}{y} - x'\right)$$

as a necessary and sufficient condition for stability.

2. Monopoly with One Variable Input.

Equation (7) applies in this case, except that p is now a variable. Assume

(12)
$$p = g(y, \alpha)$$

such that $\partial p/\partial y < 0$ and α, a shift parameter, is defined so that $\partial p/\partial \alpha > 0$ and further that $\partial p/\partial y$ remains invariant under the shift. Differentiating (7) with respect to y and solving for the first order condition for a maximum $(y' \equiv 1/x')$:

(13)
$$y'(p + p'y) = (py - R)/x, \quad \text{where} \quad p' = \frac{\partial p}{\partial y}\bigg|_\alpha.$$

Equation (13) may be differentiated with respect to R and α, respectively, and solved for:

$$\frac{\partial y}{\partial R} = - \frac{x'}{2p'x + p''xy - x''(py - R)}$$

and

$$\frac{\partial y}{\partial \alpha} = \frac{(\partial p/\partial \alpha)(x'y - x) - xy\left(\dfrac{\partial^2 p}{\partial y \partial \alpha}\right)}{2p'x + p''xy - x''(py - R)} .$$

Knowledge of signs tells us that, as long as the demand curve is linear or convex to the origin, a change in R leads to a change in y in the same direction. With a similar demand curve, an upward shift in demand of the hypothesized kind will lead to a decrease in output if the firm is operating beyond the point of maximum average product, but an increase in output if average product is still increasing.

3. The Two-Variable-Input Case.

$$(14) \qquad \qquad S = \frac{1}{x} \left[py - (wx + vz + R) \right]$$

and

$$(15) \qquad y = f(x, z), \qquad y_x > 0, \qquad y_z > 0,$$
$$y_{xx} < 0, \qquad y_{zz} < 0, \quad \text{and} \quad y_{xx}y_{zz} - y_{xz}^2 > 0.$$

Applying the first-order conditions for a maximum,

$$(16) \qquad \partial S/\partial x = (1/x^2)[p(xy_x - y) + vz + R] = 0$$

and

$$(17) \qquad \partial S/\partial z = (1/x)(py_z - v) = 0$$

or

$$(18) \qquad \qquad py_x = \frac{py - (vz + R)}{x}$$

and

$$(19) \qquad \qquad py_z = v.$$

Further differentiation of (16) and (17) gives, at the equilibrium position at which (18) and (19) are satisfied,

$$\partial^2 S/\partial x^2 = py_{xx}/x < 0,$$
$$\partial^2 S/\partial z^2 = py_{zz}/x < 0,$$
$$\partial^2 S/\partial x \partial z = py_{xz}/x$$

and

$$\frac{\partial^2 S}{\partial x^2} \frac{\partial^2 S}{\partial z^2} - \left(\frac{\partial^2 S}{\partial x \partial z}\right)^2 = \frac{p^2}{x^2} (y_{xx}y_{zz} - y_{xz}^2) > 0.$$

Therefore equations (18) and (19) determine a maximum. Both the latter equations can be differentiated with respect to R and with respect to p and solved for:

$$\frac{\partial x}{\partial R} = \frac{\begin{vmatrix} -1 & (pxy_{xz}) \\ 0 & (py_{zz}) \\ y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}}{p^2x \begin{vmatrix} y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}} > 0,$$

$$\frac{\partial z}{\partial K} = \frac{\begin{vmatrix} (pxy_{xz}) & -1 \\ (py_{zz}) & 0 \\ y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}}{p^2x \begin{vmatrix} y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}} \gtrless 0,$$

$$\frac{\partial x}{\partial p} = \frac{\begin{vmatrix} (y - xy_x) & (pxy_{xz}) \\ (-y_z) & (py_{zz}) \end{vmatrix}}{p^2x \begin{vmatrix} y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}} \gtrless 0 \qquad \frac{\partial z}{\partial p} = \frac{\begin{vmatrix} (pxy_{xx}) & (y - xy_x) \\ (py_{xz}) & (-y_z) \end{vmatrix}}{p^2x \begin{vmatrix} y_{xx} & y_{xz} \\ y_{xz} & y_{zz} \end{vmatrix}} \gtrless 0.$$

The slope of the supply function at the equilibrium point,

$$\frac{\partial y}{\partial p} = y_x \frac{\partial x}{\partial p} + y_z \frac{\partial z}{\partial p}$$

$$(20) \qquad = \frac{[y_{zz}y_x(y - xy_x) - xy_z{}^2y_{xx} + y_{zz}(xy_xy_z - yy_z + xy_z)]}{px(y_{xx}y_{zz} - y_{xz}{}^2)} \gtrless 0.$$

The denominator is positive, but the numerator is undetermined in sign with the specified information.

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PROGRESSIVE TAXATION IN AN
INFLATIONARY ECONOMY

By E. J. Mishan and L. A. Dicks-Mireaux*

The impact of progressive taxation on variations in aggregate income is of general interest for at least two reasons. First, as a consequence of the postwar inflation, there has been a growing awareness in all sections of the community of the burden of income taxation. Second, it is of importance to economic theorists, in particular to those who hope to influence policy, to have some idea of the magnitude of the so-called "built-in stability" effect of progressive taxation when confronted with large variations in aggregate income as well as small.

Though some useful work has been done with the aim of estimating the amount of built-in stability, the methods used have been such as to limit the findings to strictly comparable aggregate incomes[1]; or in those cases where an extension of the same methods to widely varying incomes was conceivable in principle, in practice it would have proved too laborious to undertake.[2] The particular virtue of the method pre-

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[1] For instance, R. A. Musgrave and M. H. Miller [3] suggest a coefficient of built-in stability (or what they call "built-in flexibility"), α, defined as $1 - \dfrac{\Delta Y}{\Delta Y_a}$, where ΔY is the resultant increment of income in response to an autonomous increase of investment with the given structure of taxes, while ΔY_a is the hypothetical increment of income in the absence of any income tax. Thus "α is the fraction of change in income which is prevented because of built-in flexibility." They separate two factors, (i) the average level of tax rates r, and (ii) the income elasticity of taxes, $E = \dfrac{\Delta T}{\Delta Y} \cdot \dfrac{Y}{T}$, in order to indicate the possibility of increasing α by raising E while maintaining or lowering r. With a current r estimated at about .2, and an E estimated at about 1.5, α is little over one-third at the existing (1948) yield levels.

For limited movements of income from current levels, the marginal tax arrived at by their methods is satisfactory enough. The estimates for E and r, however, would not be suitable for measuring changes in income-tax revenue for appreciable change in aggregate income even if we could neglect changes wrought by alterations in the income structure. For instance, granted that tax rates remain unaltered, the onset of a mild but persistent inflation would shift income earners into successively higher tax brackets with the result that the marginal tax would advance faster than income.

[2] For instance, J. A. Pechman [4] has calculated the total of personal income taxes which the population would have paid in the years 1948-52 at the 1953 tax rates given the income distributions for each of these years. His method is to distribute the total of taxable income of each income group among the relevant tax brackets, the amounts in each being

sented in this paper is that, if the assumptions about tax characteristics are accepted, the calculation of total income-tax yields for any variation in aggregate income becomes a relatively simple procedure. Not only can we compare, for example, the growth in tax yields for different rates of inflation, but international comparisons of progressiveness and built-in stability become feasible. While we shall confine our attention to the U.S. economy, employing the tax structure existing in 1953, the data used, though accurate enough to warrant our conclusions, are of subsidiary importance only, serving simply to illustrate the method.

First, a word about the usefulness of comparing tax yields for widely varying levels of aggregate income on the basis of the tax structure of a particular year. If an inflation is prolonged, a government may lower its rates of income taxation. In fact during the postwar period income-tax rates were lowered both in the United States and the United Kingdom, though not sufficiently to preclude the governments of these countries from collecting increased revenue. Furthermore, if a government did maintain the level of taxes or did not appreciably lower them during a period of prolonged inflation, the tax yield might not increase in legitimate proportion as a consequence of growing tax evasion by those sections of the community more advantageously placed for this practice. Yet the question to be answered here is not the less interesting because of these and similar considerations. Indeed, the pressure on governments to reduce taxation during an inflationary period, and the growing temptation of nonemployed income earners to evade it, cannot be properly appreciated without some notion of what we may call the *virtual* growth of the tax yield—the growth of the tax yield that would occur if all means directed toward mitigating its incidence were held in abeyance.

In a first calculation we shall include only personal income taxes. Both corporation and indirect taxes will, however, be included in a second calculation in which the amount of all taxes other than income taxes will be assumed to remain proportional to aggregate money income.[3]

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taxed at the 1953 bracket rate. His estimate of built-in stability, T/Y was between 16 and 19 per cent over the years 1948-53.

If this method were to be used to estimate tax yields of income in future years assumptions about the income structure and the possibly changing tax characteristics of income earners in each income group would be necessary. Pechman does not attempt this however; he estimates only what built-in stability was. On the other hand, if Pechman's methods were used to estimate built-in stability for projected aggregate income levels, it would require a prohibitive amount of calculation for aggregate incomes associated with even a limited range of rates of inflation or deflation over a few years.

[3] It might occur to the reader that if we experiment with unchanged tax rates for personal incomes we should extend the experiment to all other taxes. Yet if, instead of using

We shall also want to measure what we call the tax-income ratio, defined as the ratio of the revenue which the government would collect, if all taxes in question were duly paid, to the aggregate of personal incomes of the entire population. Its calculation, however, will depend upon the postulates we make in order to isolate the problem and upon the simplifications necessary to make the calculations manageable.

In regard to the postulates, while we are chiefly interested in the effects on taxation of varying rates of inflation, an increase of money income occasioned solely by a rise in productivity will bear the same additional income tax as an equal increase of money income which is the result purely of a rise in the general level of prices, provided always that the trend in income distribution is common to both contingencies. We are therefore under no obligation at the outset to state whether the growth of money income is the result of one factor or of the other or of both of them. This is an advantage, for with the results we reach on the basis of an expanding money income we may estimate the portion of the national income which ought to accrue to the government for various combinations of inflation and productivity.

No growth or decline of the population, natural or migratory, is allowed for. For the measurement of the tax-income ratio this restriction is not at all serious. If we supposed, as we might without incredulity, that the distribution of income of any additional population was similar to that of the initial population an allowance for additional population would not affect this ratio.

As a first approximation we shall assume that any increment in the total money income of the population is divided in equal proportion among them: each income-earner, that is, experiences the same percentage increase in his money income. Needless to say, there have been significant changes in the structure of gross incomes over the postwar period—broadly speaking, relative gains were made by unskilled and semiskilled workers, relative losses by salaried professionals—but even if this trend should continue further, any endeavour to allow for it may be considered a refinement not to be pursued here.

Section I develops a general method of deriving the required equation, aggregate income-tax yield as a function of aggregate income. In Section II the adoption of a particular form of tax function is shown to result in a further simplification of work. Section III gives the estimates of tax yields for a wide range of aggregate income levels using the 1953 tax rates.

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our obviously tentative conjecture about the time-path of all other taxes, we were to break them down into several categories (say, specific excise taxes, ad valorem excise taxes, corporate taxes, and capital taxes, with perhaps some further subdivisions) it would be unlikely to alter the general impressions which emerge from the tables which include all taxes.

I. *The Method of Calculation*

Ideally, if we suppose total money income to have risen by, say, 10 per cent we should, following our first approximation, add 10 per cent to the income of each income-earner in the given population. The larger tax that each income-earner has to pay may be easily calculated by referring to the appropriate tax tables and the total of taxes so calculated may be expressed as a percentage of the now larger aggregate money income. Though straightforward enough, this method is hardly practicable in a community comprising more than a few income-earners. For the size of populations we are dealing with we have no choice but to divide them into income groups. The smaller the range of each

TABLE 1.—PERSONAL INCOMES AND INCOME TAX LIABILITY, 1953

Income Group ($ thousands per year)	Number of Taxpayers	Adjusted Gross Income ($ millions)	Income Tax Liability
0–2	6,780,709	9,281	752
2–3	7,021,539	17,640	1,683
3–5	15,868,939	63,051	6,418
5–10	12,490,576	81,747	10,443
10–20	1,506,940	19,702	3,592
20–50	414,989	12,037	3,353
50–100	60,260	3,994	1,646
100–500	15,153	2,153	1,227
500 & over	517	528	319
Total	44,159,622	210,372	29,431

Source: Estimates provided by courtesy of the National Bureau of Economic Research.

income group the more accurate will be our calculations albeit the more tedious the work involved. Actually, our choice is limited by the official tax returns which suffice to illustrate the method and yield statistically significant results. Table 1 contains the basic data for our calculations.

We may well suspect that errors in the tax returns will be mostly errors of omission and that, therefore, the tax that would have been collected by an omniscient government would be a larger proportion of the aggregate income than the proportion actually collected. Notwithstanding this source of discrepancy we may accept the table as a basis for calculation on the assumption that the percentage error between the "true" tax and the collected tax remains constant. Interested as we are in the relative changes of the aggregate tax and tax-income ratio, the significance of the discrepancy is slight.

The first step in deriving a relationship between aggregate income and aggregate income tax is to discover the relation between individual earnings and individual income tax,

(1) $t = \phi(y)$,

where t is the individual tax and y the individual income. Since the relevant circumstances of individual taxpayers vary widely we shall use the information in the table to estimate this relation for an *average* taxpayer and then subject all taxpayers to it. We assume that if, say, each of the number of taxpayers within a certain income group had his income increased by the same percentage thereby entering a higher-income group, the *average* tax now to be paid will be the same as that paid by the previous members of that income group, who have now like-wise moved up in the income scale.[4] .
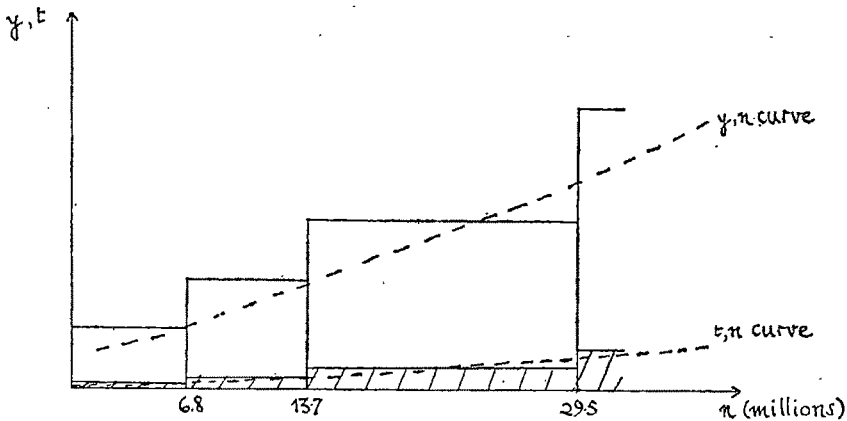
FIGURE 1

LOGARITHMIC SCALES ·

Next we arrange the taxpaying groups from the smallest on the left to the largest on the right along the horizontal axis of Figure 1 which measures the number of taxpaying individuals n. Along the vertical axis we measure personal income, y, and income tax, t. For reasons of space we include only the three lowest-income groups. The shaded rectangles represent the tax paid by the group whose income is represented by the larger rectangle (average income *times* number of income-earners in group). On our definition, then, the existing tax-income ratio may be reckoned as the sum of the shaded rectangles over the sum of the outer rectangles, expressed as a percentage. ·

[4] Empirical justification for this assumption may be established by discovering identical average tax yields of equal income groups for two or more years a little apart in time, years in which the tax structure was the same or closely similar. Unfortunately, we have been unable to discover among the U.S. data two such comparable years, so that this assumption stands, at worst, along with the assumption of an equiproportional rise of income for each member of the earning population, as an approximation which we believe to be not so far removed from the true state of things as to impart, in a calculation of this kind, a significant bias to our results.

Suppose now that there is a rise in aggregate money income of 10 per cent. The height of each outer rectangle is raised by 10 per cent while the height of the corresponding tax-rectangle is adjusted by reference to our tax-income function, equation (1) above. The area of the sum of the adjusted tax rectangles divided by the area representing the sum of the new income rectangles is the index of the new tax-income ratio.

Though this procedure is not impracticable and clearly illustrates the principle employed here, it would be wearisome to follow this procedure if we wished to measure the tax burden for varying rates of growth of aggregate income. In order therefore to devise a more general formulation we shall assume continuity in the range of incomes—which is not unreasonable considering the large numbers involved—and, in Figure 1, impose a continuous income-population curve and a continuous tax-population curve, indicated by the dotted y, n and t, n curves respectively, to replace the discontinuous curves traced out by the outer and inner rectangles. In fact we need derive statistically only the income-population curve,

$$(2) \qquad\qquad y = f(n)$$

where n is the nth individual in the population so arranged. The tax-population curve follows directly from the simple transformation,

$$(3) \qquad\qquad t = \phi[f(n)].$$

If now the y, n curve of Figure 1 were to shift upward in equal proportion along its entire length, the t, n curve would follow it upward at a determinate rate and, as we might guess, at a faster rate than the y, n curve. The response of the tax area to such an increase in the income area of our figure is, of course, the basic relationship that we seek,

$$(4) \qquad\qquad T = \Psi(Y),$$

where T is the aggregate of personal income taxes and Y the aggregate of personal incomes of the community.[5]

[5] The mathematical derivation of (4) presents no difficulties. We have two equations to be estimated statistically, (1) and (2). From these two equations we can deduce (3). Initial aggregate income, Y_0, is equal to

$$\int_0^N f(n)dn,$$

where N is the entire population of income earners. Holding the population of income earners, N, constant, any aggregate income, Y, is equal to

$$\int_0^N \lambda f(n)dn,$$

If there is now a rate of growth of money income over time, say 100 r per cent per annum, the magnitude of the aggregate money income after x years is Y_x, where

$$Y_x = Y_0(1 + r)^x.$$

Since for any Y_x we can now calculate the corresponding aggregate tax, T_x, from equation (4), the tax-income ratio T_x/Y_x is given by the ratio

$$\Psi[Y_0(1 + r)^x]/Y_0(1 + r)^x,$$

where Y_0 is a datum, Ψ has been calculated, and r and x take on chosen values.

II. A More Simplified Method

We have demonstrated a general method of calculating aggregate tax and the tax-income ratio corresponding to any size of aggregate money income for a tax-income function and an income-population function of any form, provided only that they are continuous and single valued. Though not difficult in principle it does entail the statistical estimation of these two functions and the derivation from them of a third function, (3). Unless the fit of the two statistical curves is unusually good the error in the third function may be disturbing.[6]

A great simplification of work and a reduction of error follow if the tax function is cast into the exponential form $t = By^\beta$ for then we require neither the statistically derived income-population function, equation (2), nor the derived tax-population function, equation (3). Given only the initial aggregate income tax—and irrespective of the initial population, the initial aggregate income, or its distribution (provided only that the distribution remains unaltered during the income-changes) —a given percentage increase of that aggregate income associated with the initial aggregate tax in question will uniquely determine the increment in the aggregate tax.

This rather surprising proposition is made evident in the appendix, but it is also illustrated in Figure 2 in which the vertical axis measures

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λ being the ratio Y/Y_0. Finally, the aggregate income tax corresponding to any aggregate income is T, where

$$T = \int_0^N \phi[\lambda f(n)]dn.$$

And since N is held constant, T becomes a function of λ, or Y/Y_0. Y_0 being constant, T becomes a function of Y, as in (4).

[6] To illustrate, in using this general method with the 1953 data we obtained very satisfactory fits for the two statistical functions. Notwithstanding this, the computed initial aggregate tax,

$$T_0 = \int_0^N \phi[f(n)]dn,$$

gave a figure some 10 per cent above that of the known aggregate tax of 1953.
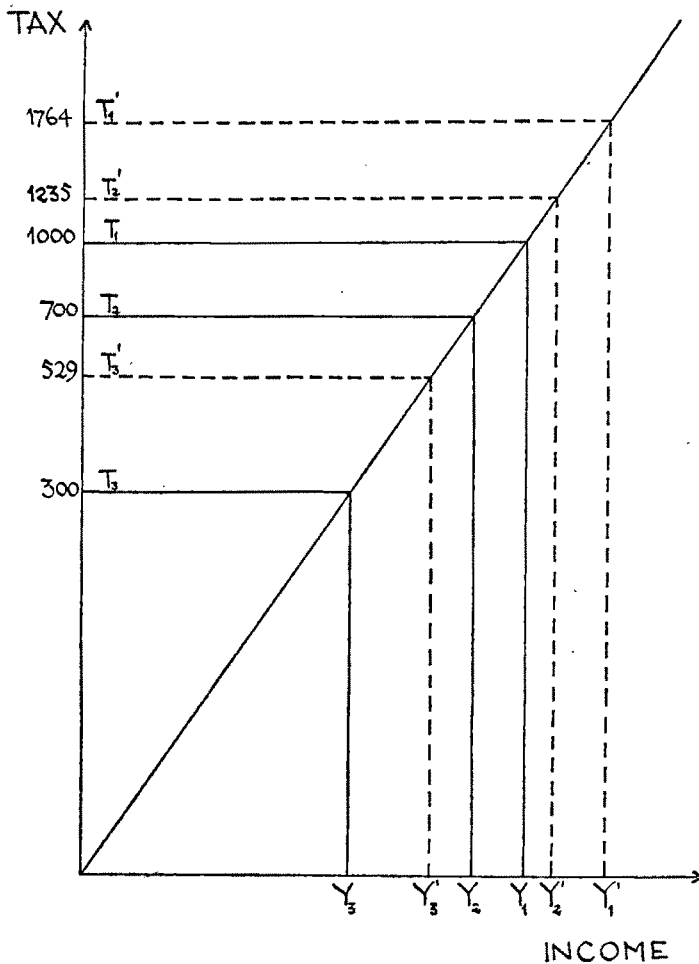
FIGURE 2

the logarithm of the tax and the horizontal axis the logarithm of the individual's income. The tax function, $t = By^\beta$ appears therefore as a straight line in which log B is the vertical intercept (not shown for convenience of presentation) and β the slope of the straight-line curve. For simplicity of exposition log B is taken as equal to $-2\frac{1}{2}$, and β as equal to 1.4. As a matter of convenience, the axes passing through the origin are omitted in Figure 2, only the relevant part of the curve appearing.

We can now suppose two distinct communities, A and B, each paying the same aggregate income tax of 1,000, and each subjected to the

tax function in Figure 2. Community A contains only one individual who, consequently, pays the whole of the 1,000 tax which is represented by the height of T_1 from the origin. The income corresponding to this tax is 8,483, and is represented by the distance from the origin to Y_1. In the other community there are two income-earners, one paying 700 and the other paying 300, these amounts being indicated by T_2 and T_3 respectively. Their corresponding incomes, Y_2 and Y_3, are therefore 6,575 and 3,590 respectively. It should be noticed that aggregate income in B is larger than that in A, that population in B is larger than that in A, but aggregate tax, 1,000 is the same for both communities.

We now increase aggregate income in A and in B by 50 per cent. Since the increase is equiproportional, the new incomes will be 12,725, 9,863, and 5,385 for Y_1', Y_2', Y_3', respectively, and the new taxes corresponding to them, T_1', T_2', and T_3', can be read from the curve in Figure 2, and will be 1,764, 1,235 and 529. The *increment* in tax in both communities is therefore 764, or 76.4 per cent.

III. *Estimates of Tax Yields*

It is indeed fortunate that a tax function of the exponential form turns out to be such an excellent fit[7] for with the simplified aggregate tax function $T_x = \lambda^\beta T_0$, or, specifically using the 1953 data, $T_x = \lambda^{1.42483} T_0$, we may say simply that a change of the initial aggregate income by the ratio λ results in a change in the corresponding initial aggregate income tax by the ratio $\lambda^{1.42483}$. In other words a 1 per cent increase or decrease[8] of aggregate income is accompanied by, approximately, a 1.428 per cent increase or decrease in corresponding aggregate income tax, and a 10 per cent increase or decrease of aggregate income is accompanied by, approximately, a 14.54 per cent increase or decrease in corresponding aggregate income tax. With such a formula the cal-

[7] The specific individual tax function, equation (1), for 1953 turns out to be $t = .0315 y^{1.42483}$, the coefficient of correlation being .9983.

[8] Pure deflation is, of course, symmetrical with pure inflation, and in such cases the formula is immediately applicable. On the other hand, a "recovery" or a "recession" characterized by sizable variation in employment violates the assumption that during the change in aggregate income the population remains constant. More important, however, it violates the assumption that the distribution of incomes remains unchanged when aggregate income varies. For example, during a recovery, newly employed individuals whose incomes come into the lowest tax brackets make a relatively small contribution to aggregate income tax. However, our assumption of an equiproportional spread of any increment of aggregate income over the pre-existing distribution of incomes issues in a greater tax yield than in fact is collected. Parallel reasoning for a recession reveals our assumption to issue in a reduction of income taxation which would be in excess of that experienced. Hence, in so far as a change in the level of prices is accompanied by changes in employment in the same direction, our method overstates the change in aggregate income taxation and to that extent exaggerates built-in stability.

TABLE 2.—AGGREGATE REAL PERSONAL INCOME TAXATION

(billions of dollars)

Year	0 (1953)	1	2	3	5	10	15
Aggregate real personal income[a]	286	295	303	313	332	384	446
Virtual per annum yield[b] of aggregate real personal income tax at 1953 tax rates with per cent price-level increase per annum of:							
—10		32.3	32.2	32.1	32.0	31.6	31.2
— 5		33.1	33.8	34.4	35.9	39.7	44.0
— 3		33.4	34.4	35.4	37.5	43.4	50.2
0	32.4	33.8	35.8	36.8	40.0	49.4	61.0
1		33.9	35.6	37.2	40.9	51.5	65.0
2		34.1	35.9	37.7	41.7	53.7	69.2
3		34.2	36.2	38.2	42.6	56.0	73.6
5		34.5	36.7	39.1	44.4	60.8	83.2
10		35.2	38.2	41.5	49.0	74.0	111.9

[a] An increase of real income of 3 per cent p.a. is assumed throughout this table and all succeeding tables. Income includes transfer payments which in 1953 amounted to $14.3 billion.

[b] We apply our aggregate tax function, $T_x = \lambda^{1.42483} T_0$, as estimated to the data given in the *Survey of Current Business*, July 1956.

Source: For 1953 data, *Survey of Current Business*, July 1956.

TABLE 3.—AGGREGATE REAL TAXATION

(billions of dollars)

Year	0 (1953)	1	2	3	5	10	15
Aggregate real personal income[a]	286	295	303	313	332	384	446
Virtual per annum yield[b] of all taxes (in real terms) at 1953 tax rates with per cent price-level increase per annum of:							
—10		96.8	98.7	100.6	104.6	115.7	128.7
— 5		97.6	100.2	102.9	108.5	123.9	141.5
— 3		97.9	100.8	103.8	110.1	127.5	147.8
0	95.0	98.3	101.7	105.2	112.6	133.5	158.5
1		98.4	102.0	105.7	113.5	135.7	162.5
2		98.6	102.3	106.1	114.3	137.9	166.7
3		98.7	102.6	106.6	115.2	140.1	171.2
5		99.0	103.2	107.6	117.0	144.9	180.7
10		99.7	104.7	109.9	121.6	158.2	209.5

[a] As in Table 2.

[b] All taxes other than personal income taxes are assumed to increase in the same proportion as aggregate money income. (See text and footnote 10.)

Source: See Table 2.

TABLE 4.—ANNUAL TAX-INCOME RATIO (PERSONAL INCOME TAXES ONLY)

(percentages)

Year	0 (1953)	1	2	3	5	10	15
Annual percentage price increase of:							
−10		11.0	10.6	10.3	9.7	8.2	7.0
− 5		11.2	11.1	11.0	10.8	10.3	9.9
− 3		11.3	11.3	11.3	11.3	11.3	11.3
0	11.3	11.5	11.6	11.8	12.1	12.9	13.7
1		11.5	11.7	11.9	12.3	13.4	14.6
2		11.6	11.8	12.1	12.6	14.0	15.5
3		11.6	11.9	12.2	12.9	14.6	16.5
5		11.7	12.1	12.5	13.4	15.8	18.7
10		12.0	12.6	13.3	14.8	19.3	25.1

Source: Table 2.

culation of change in aggregate income tax, in real or money terms, for varying rates of productivity combined with varying rates of inflation or deflation becomes a relatively effortless operation.[9] Nevertheless, in order to convey an impression of the magnitudes involved we have made some estimates, which appear in Tables 2 to 5. Such estimates and any conclusions drawn from them will of course depend upon the particular characteristics of the tax structure in the United States during 1953.

In the tables we assume productivity to increase steadily over the future at 3 per cent per annum. Thus, as may be gathered from Table 2, it would require roughly a 3 per cent per annum decline in the price level to maintain aggregate money income unchanged over time. This, then, is the rate of deflation necessary if the *proportion* of income to be paid in as income tax is to remain constant (though, of course, *real* income tax being paid is increasing at the rate of 3 per cent per annum). If the price level does not alter over time, money income increases at the same rate as productivity—3 per cent per annum on our assumption—and the proportion of tax collected rises. In that

[9] In this connection it will be appreciated that our form of the exponential tax function, $t = By^\beta$, implies that tax is collected from all individual incomes no matter how small. The figures from which the function is derived are, we should remember, average figures. If, for example, the group averaging $2,000 pay on the average a tax of $10 per person, many in the group are in fact not paying any tax at all. Nonetheless, if, owing to an equiproportional rise in the incomes of the community, the old group is replaced by a new group with average incomes of $2,000 we assume the same average tax of $10 per person, though again a similar proportion of the group pays no tax at all. It is the averaging principle that enables us to include the lowest-income group in the tax function, and the imposition of continuity in the tax function that enables us to reach down to the lowest income in the community—a hypothetical zero income.

TABLE 5.—ANNUAL TAX-INCOME RATIO (ALL TAXES)

(percentages)

Year	0 (1953)	1	2	3	5	10	15
Annual percentage price increase of:							
−10		32.9	32.5	32.2	31.5	30.1	28.9
− 5		33.1	33.0	32.9	32.7	32.2	31.8
− 3		33.2	33.2	33.2	33.2	33.2	33.2
0	33.2	33.4	33.5	33.7	34.0	34.7	35.6
1		33.4	33.6	33.8	34.2	35.3	36.5
2		33.5	33.7	34.0	34.5	35.9	37.4
3		33.5	33.8	34.1	34.7	36.5	38.4
5		33.6	34.0	34.4	35.3	37.7	40.6
10		33.8	34.5	35.2	36.7	41.2	47.0

Source: Table 3.

case the aggregate money, and real, tax increases faster than 3 per cent per annum; in fact it begins to increase at about 4.3 per cent per annum. For a moderate inflation of, say, 3 per cent per annum, the initial per annum increase of real aggregate tax collected is in the region of 5.6 per cent. In 12 years or so, aggregate real tax would have doubled. It would double in a little over ten years if the price increase per annum averaged about 5 per cent.

A visual impression of Table 2 is given in Figure 3. Aggregate real income and aggregate real tax are measured vertically on a logarithmic scale whereas time, in years, is measured horizontally on a natural scale. The several lines beginning at T_0 and sloping upward to the right trace the paths through time of per annum aggregate tax for different rates of inflation. For instance, the vertical line 5-Y_5 measures aggregate real income per annum 5 years from the base year. The intersection through it of, say, the 10 per cent line (at the point T) provides the measure of per annum aggregate real tax, 5-T, at the same point in time, the fifth year, when prices have been rising steadily at the rate of 10 per cent per annum.

It need hardly be said that this diagram could easily be modified to have the distance O-Y_0 equal to the distance 15-Y_{15}. Changes in real aggregate income and real aggregate tax could no longer be shown on such a diagram but the comparison of the tax-income ratio as defined would be immediately apparent. Though we have not included this modified diagram, the tax-income ratio figures for varying rates of inflation have been distilled from the information given in Table 2, and appear in Table 4. It may surprise some to observe that, with tax rates unchanged, it would require 15 years of inflation, averaging 10 per

BILLIONS OF DOLLARS : LOGARITHMIC SCALE
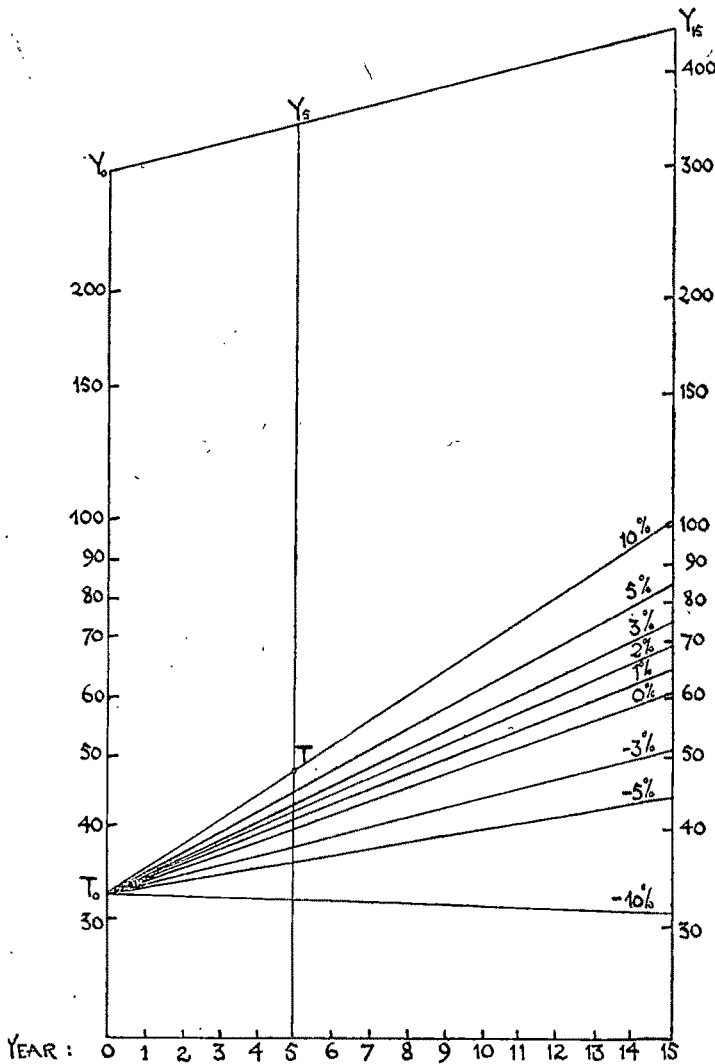
FIGURE 3

cent per annum, to raise the 1953 tax-income ratio of a little over 11 per cent to a tax-income ratio of just over 25 per cent per annum.

Finally, in order to impart some idea of the variations in total taxes we introduce a conjecture about the likely course of all taxes other than personal income taxes—the one already suggested, that they remain proportional to aggregate money income over time. The figure we use

is 21.9 per cent, the ratio of all other taxes to personal incomes in 1953.[10] Tables 3 and 5 give figures for total real tax and the total tax-income ratio respectively under the same conditions as those in Tables 2 and 4.

The tables afford an impression which makes detailed comment unnecessary. The tax-income ratio certainly increases with inflation but not as fast as might have been expected by those acquainted with the wonders that can be performed with compound interest.[11] Even with prices rising continuously at an average rate of 10 per cent per annum, 13 years or so are required before the burden of tax is doubled for the community as a whole, though for the higher ranges of income it will increase faster than this.

As for the disinflationary effect of a growth in aggregate real tax, it is plain that though this source of stability is not negligible economists who have put some emphasis on this factor[12] may find the magnitudes a little disappointing. Certainly we are impelled to the conclusion that a passive fiscal policy—maintaining tax rates constant with

[10] The ratio for the successive years, 1950-1955 inclusive, is 21.3, 22, 20.8, 21.9, 21, and 22.6 per cent. This assumption is not inconsistent with the regressive character of these other taxes taken together as a group as estimated by R. A. Musgrave [5, p. 198]. If each income bracket receives an equiproportional increase in income, in accordance with our assumption, then—provided the income distribution remains the same—aggregate income increases in the same proportion and so also does the amount of all other taxes when calculated as a proportion of income, notwithstanding that this proportion is larger for lower income brackets than it is for higher ones.

[11] Though fast enough to call into question the relevance of the arguments put forward by E. D. Domar [1]. Domar shows that, granted constant prices, a percentage growth of national income, r per annum, accompanied by continuous government borrowing at a percentage rate, α per annum, will result in a growing debt burden which depends upon the rate of interest, i, paid on government bonds, and that this burden may be covered by a proportional tax (measured as total interest payments on the public debt divided by taxable income) which, though it grows over time, tends to a limit

$$\frac{i}{\dfrac{r}{\alpha} + i} \ .$$

Although this required tax rate turned out to be relatively low for any reasonable rate of government expenditure, the impression given of an additional burden to be met by taxes can be very misleading. Even with constant prices (or prices falling at an average rate of 3 per cent per annum) the government collects revenue at an ever-increasing rate and sufficient in magnitude to meet interest payments on any feasible rate of continuous government borrowing. While if only a limited rise in the level of prices over time is envisaged, a glance at Table 2 will suffice to assure the reader that the problem is hardly one of raising additional taxes but one of determining the disposal of growing revenues. If the government does not reduce taxes substantially and the level of prices does not fall continuously over the future—an unlikely contingency—either the government budget will tend to yield increasing surpluses or the government sector will tend to expand at the expense of the private sector of the economy.

[12] See, for instance, Milton Friedman's paper [2, esp. pp. 250-52].

unchanged (real) government outlays—would not by itself bring powerful pressure to bear on mild inflationary tendencies until after the lapse of quite a few years. Built-in stability is built small into the system, not large.

MATHEMATICAL APPENDIX

The object of this short analysis is to set out formally some of the preceding argument and to illustrate the methods of calculation used.

1. *The tax-income function and the distribution of incomes.* In the first instance we shall consider the most general case, assuming that the tax-income function and the distribution function of incomes may be described continuously. Thus:

$$(1) \qquad\qquad t = \phi(y)$$

where t is the tax liability on the gross income y. And:

$$(2) \qquad\qquad y = f(n)$$

where y is the gross income of the nth income-earner, ranked in order of magnitude of income.

Substituting for y in equation (1) gives:

$$(3) \qquad\qquad t = \phi[f(n)].$$

2. *Aggregate tax and income.* If now all income-earners pay some tax, n may vary from 0 to N in both the tax and income functions. N is the total income-earning population. Thus aggregate taxation may be written:

$$(4) \qquad\qquad T_0 = \int_0^N \phi[f(n)]dn$$

corresponding to an aggregate income of

$$(5) \qquad\qquad Y_0 = \int_0^N f(n)dn.$$

3. *The effect of a rise in money incomes.* If now money incomes rise at a given rate and all incomes are affected alike we are able to measure the change in aggregate taxation, assuming that the tax-income function remains unchanged. Thus if a given y may now be written λy where $\lambda > 1$, the tax liability on each of the new incomes becomes:

$$(6) \qquad\qquad t = \phi(\lambda y)$$

and since the distribution of incomes remains unchanged,

$$(7) \qquad\qquad t = \phi[\lambda f(n)].$$

Aggregate taxation now becomes:

$$(8) \qquad\qquad T_\lambda = \int_0^N \phi[\lambda f(n)]dn$$

and the corresponding aggregate income is:

$$(9) \qquad Y_\lambda = \int_0^N \lambda f(n) dn$$

$$= \lambda Y_0.$$

4. It has been assumed throughout that all income-earners pay some tax. If we consider the tax schedule in any detail this assumption appears too restrictive. But it should be remembered that we are concerned with a function relating *average* tax to *average* income. This function by its very continuity implies that tax is paid from the lowest to the highest income.

5. If the tax-income function takes the exponential form:

$$(10) \qquad t = By^\beta$$

where β is the tax-income elasticity and is constant for all incomes, and B is some other constant, we can show that aggregate taxation is independent of the distribution of incomes.

Thus equations (4) and (8) may now be written:

$$(11) \qquad T_0 = \int_0^N B[f(n)]^\beta dn$$

and

$$(12) \qquad T_\lambda = \int_0^N B\lambda^\beta [f(n)]^\beta dn.$$

Subsequent taxation may therefore be expressed in terms of initial taxation as:

$$(13) \qquad T_\lambda = \lambda^\beta T_0$$

which is independent of the distribution of incomes.

All that need be estimated by statistical methods is the average tax-income elasticity β.

6. *The effect of annual increases in prices and productivity.* If prices and productivity are assumed to rise at annual rates of $100p$ and $100s$ per cent respectively then from equation (13) the aggregate money tax after x years may be written as:

$$(14) \qquad T_x = [(1 + p)(1 + s)]^{\beta x} T_0$$

where T_0 is initial aggregate tax in year 0. Real aggregate tax becomes:

$$(15) \qquad T_x' = [(1 + p)^{\beta-1}(1 + s)^\beta]^x \cdot T_0.$$

The aggregate tax-income ratio may be written as:

$$\frac{T_x}{Y_x} = \frac{[(1 + p)(1 + s)]^{\beta x} T_0}{[(1 + p)(1 + s)]^x Y_0}$$

where Y_0 is initial aggregate income in year 0.

Therefore the aggregate tax-income ratio after x years becomes:

(16) $$\frac{T_x}{Y_x} = [(1+p)(1+s)]^{(\beta-1)x} \cdot \frac{T_0}{Y_0}.$$

7. *The statistical determination of β.* The coefficient β was determined by the method of least squares from the regression of log t upon log y. The average tax liabilities and incomes were calculated from Table 1 of the text. The variables were weighted according to the relative tax liability in each income group, because in estimating the tax-income function we can only approximate to some "ideal" function. There will be divergencies, in general, between observed and estimated taxes. Since we cannot attain the ideal we should, as it were, shift some of the inevitable error—the divergencies between estimated and observed values—away from the more important points on the function to the less important ones. If, to take an extreme example, 90 per cent of the aggregate tax were derived from the $5,000 to $10,000 group we should be concerned to achieve a closer correlation between observed and estimated values over this range even though, as a consequence, the functions will fit less well elsewhere.

The estimated equation in linear logarithmic form was:

$$\log t = -2.49801 + 1.42483 \log y$$

$$(0.12507) \quad (0.03121)$$

where the appropriate standard error of each estimated coefficient is given in brackets. The correlation coefficient for log t and log y was 0.9983 so that the fit can be considered to be a good one. For the purpose of all calculations β was taken as 1.42483.

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THE SIZE DISTRIBUTION OF BUSINESS FIRMS

By Herbert A. Simon and Charles P. Bonini[*]

The distribution of business firms by size has received considerable attention from economists interested in the phenomena of competition and oligopoly and in the issues of government regulation to which these phenomena are relevant. That the size distribution of firms (whether within a single industry or in a whole economy) is almost always highly skewed, and that its upper tail resembles the Pareto distribution has often been observed, but has not been related very much to economic theory. Attempts at economic explanation of the observed facts about concentration of industry have almost always assumed that the basic causal mechanism was the shape of the long-run average cost curve; but there has been little discussion of why this mechanism should produce, even occasionally, the particular highly skewed distributions that are observed.

In Part I we shall discuss the adequacy of explanations of the size distribution based on the static cost curve. In Part II we shall propose an alternative theory based on a stochastic model of the growth process. In Part III we shall examine the empirical data in the light of the model. In Part IV we shall examine the implications of our analysis for public policy. In Part V we shall comment on some of the needs for empirical and theoretical research in this area.

I. *Economic Theory of the Size of Firms*

Economic theory has little to say about the distribution of firm sizes. In general, we are led to expect a U-shaped long-run cost curve or planning curve for a firm. But the scale corresponding to minimum costs need not be the same for different firms, even in the same industry. If we employ the concept of economic rent, we can say that firms will have the same minimum cost, but varying outputs at this cost [8, pp. 123-127]. If this is the case, the cost curve yields no prediction about the distribution of firms by size and no explanation as to why the observed distributions approximate the Pareto distribution.

Some theorizing has been concerned with long-run increasing, decreasing and constant cost curves for firms.[10, esp. pp. 210-17]. But the theorizers have hesitated to draw conclusions about the observed size distributions. In some cases, the theory is indeterminate about the distribution, as in the case of constant costs [10, p. 211]. In others, the theorists point out that "industry" is such a vague and arbitrary term that comparing the sizes of different firms is like comparing oranges and apples. Differences in the size of markets for firms and the idea that firms are moving towards the equilibrium of the cost curve but haven't reached it are also mentioned as reasons why firms widely varying in size can survive in the same industry.

All these factors make static cost theory both irrelevant for understanding the size distributions of firms in the real world and empirically vacuous. And yet these distributions show such a regular and docile conformity to the Pareto distribution that we would expect some mechanism to be at work to account for the observed regularity.

In the previous discussion (as in much of the literature on this topic) our comments about the long-run cost curve have been completely a priori. Recently, J. S. Bain [2] has made a careful analysis of all the available information on the cost curves of firms and plants in a substantial number of industries, using both published and original data that he obtained by questionnaire. His data show that plant cost curves (ignoring the problem of intra-industry specialization) generally are J-shaped. Below some critical scale unit costs rise rapidly. Above the critical scale, costs vary only slightly with size of firm. Moreover, in only a very few industries (the typewriter industry is perhaps the most striking example) does the critical scale represent a substantial percentage of the total market. These facts correspond well with beliefs about these matters that are widely held by businessmen.

We can say, then, that the characteristic cost curve for the firm shows virtually constant returns to scale for sizes above some critical minimum—S_m. Under these circumstances, the static analysis may predict the minimum size of firm in an industry with a known value of S_m, but it will not predict the size distribution of firms.

II. Stochastic Models of Firm Size

In the context of a different theoretical framework, our limited knowledge of the shape of the long-run cost curve derived from static analysis might lead to much stronger predictions. This is, in fact, the case.

We postulate that size has no effect upon the expected percentage growth of a firm. We shall formalize this into the assumption (Gibrat's law of proportionate effect) that the distribution of percentage changes

in size, over a year, of the firms in a given size class is the same for all size classes. That is to say, we assume that a firm randomly selected from those with a billion dollars in assets has the same probability of growing, say, 20 per cent, as a firm randomly selected from those with a million dollars in assets.

There are two reasons why this is a plausible assumption on economic grounds. First, it agrees with the empirical findings, as we shall discuss more fully at a later point. Secondly, if, as we have postulated, there exists approximately constant returns to scale (above a critical minimum size of firm) it is natural to expect the firms in each size-class to have the same chance on the average of increasing or decreasing in size in proportion to their present size.

Before discussing the model in detail, we should like to comment on the numerous related models that have been proposed in recent years for explaining various skewed distributions of economic variables— including income [3], wealth [11], sizes of firms [7], and sizes of labor unions [6]. It has often been noted that many economic variates—and not only firm size—have frequency distributions with highly skewed upper tails. In the past, these distributions have been most often approximated by the log-normal distribution or the Pareto curve—sometimes with quite good fit.

Now many of the simple and commonly used statistical distributions can be generated from simple stochastic models—the normal distribution, the Poisson, the exponential, and so on. A stochastic process (e.g., the simplest random walk [4, pp. 279-307]) that will generate the normal distribution of a variate will, of course, when applied to the logarithm of the variate, generate the log-normal. But in applying the assumptions to the logarithm of the variate, we have, in effect, assumed the law of proportionate effect.

We can state the same point in a different way. If we incorporate the law of proportionate effect in the transition matrix of a stochastic process, then, for any reasonable range of assumptions, the resulting steady-state distribution of the process will be a highly skewed distribution, much like the skewed distributions that have been so often observed for economic variates. In fact, by introducing some simple variations into the assumptions of the stochastic model—but retaining the law of proportionate effect as a central feature of it—we can generate the log-normal distribution, the Pareto distribution, the Yule distribution, Fisher's log distribution, and others [9, pp. 425-27]—all bearing a family resemblance through their skewness. Contrariwise, we generally get quite different steady-state distributions from stochastic processes that do not embody the law of proportionate effect, or some approximation to it.

For the moment, we prefer to emphasize the generic similarities rather than the specific differences among the various stochastic processes that incorporate the law of proportionate effect. The log-normal and the Pareto distribution have been most often discussed in the literature; our own investigations, and Champernowne's, have led more often to the class of distributions we have called the Yule distribution.

Let us assume that there is a minimum size, S_m, of firm in an industry. Let us assume that for firms above this size, unit costs are constant. Individual firms in the industry will grow (or shrink) at varying rates, depending on such factors as (a) profit, (b) dividend policy, (c) new investment, and (d) mergers. These factors, in turn, may depend on the efficiency of the individual firm, exclusive access to particular factors of production, consumer brand preference, the growth or decline of the particular industry products in which it specializes, and numerous other conditions. The operation of all these forces will generate a probability distribution for the changes in size of firms of a given size. Our first basic assumption (the law of proportionate effect) is that this probability distribution is the same for all size classes of firms that are well above S_m. Our second basic assumption is that new firms are being "born" in the smallest-size class at a relatively constant rate.

It has been shown elsewhere that under these assumptions the Yule distribution will be the steady-state distribution of the process [9, pp. 427-30]. Let $f(s)ds$ be the probability density of firms of size s. Then the Yule distribution is given by:

(1) $f(s) = K\mathrm{B}(s, \rho + 1)$,

where $\mathrm{B}(s, \rho + 1)$ is the Beta function of s and $(\rho + 1)$, K is a normalizing constant, and ρ is a parameter. It is easy to show that as $s \rightarrow \infty$,

(2) $f(s) \rightarrow Ms^{-(\rho+1)}$,

which is the Pareto distribution. Hence the Pareto distribution approximates the Yule distribution in the upper tail.

The details of the derivation of the Yule distribution need not be repeated here [9, pp. 427-35]. What distinguishes the Yule distribution from the log-normal is not the first assumption—the law of proportionate effect—but the second—the assumption of a constant "birth rate" for new firms.[1] If we assume a random walk of the firms already in the system at the beginning of the time interval under consideration, with zero mean change in size, we obtain the log-normal. If we assume

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[1] The otherwise excellent study by Aitchison and Brown [1, p. 109] is in error in supposing that Champernowne's model of income distribution would have yielded the log-normal instead of the Yule distribution if Champernowne had taken a continuous rather than a discrete model. The real difference between the models lies in the assumptions about boundary conditions.

a random walk, but with a steady introduction of new firms[2] from below, we obtain the Yule distribution.

The parameter ρ, of the Yule distribution has a simple interpretation. Let G be the net growth of assets of all firms in an industry during some period, and let g be that part of the net growth attributable to new firms—firms that have reached the minimum size during the period. Then, it can be shown that:

$$(3) \qquad \rho = \frac{1}{1 - g/G} = \frac{1}{1 - \alpha}, \quad \text{where} \quad \alpha = g/G.$$

Thus, if $g/G = .10$—new firms account for 10 per cent of the growth in assets in the industry—we will have $\rho = 1/(1 - .1) = 1.11$. In the limit, as the contribution of new firms to total growth approaches zero, ρ approaches 1. Although it is assumed in the derivation that α be a constant, a slow change in α can be expected to modify the steady-state distribution only slightly.

III. *The Empirical Data*

Since published empirical data on the distributions of firms by size are numerous and monotonously similar, we will limit ourselves to some illustrative figures. Whether sales, assets, numbers of employees, value added, or profits are used as the size measure, the observed distributions always belong to the class of highly skewed distributions that include the log-normal and the Yule. This is true of the data for individual industries and for all industries taken together. It holds for sizes of plants as well as of firms.[3]

The log-normal function has most often been fitted to the data, and generally fits quite well. It has usually been noticed, however, that the observed frequencies exceed the theoretical in the upper tail, and that the Pareto distribution fits better than the log-normal in that region. This observation suggests that the stochastic mechanism proposed in the previous section is the appropriate one, and that the data should be fitted with the Yule distribution.

We have fitted straight lines to the logarithms of the cumulative distributions for the British data of Hart and Prais, and for the data on large American firms in 1955 published in *Fortune* [5], obtaining

[2] They need not be new-born, merely small. That is, we may assume some arbitrary lower size limit and regard any firm that reaches this size as "new-born." In this case the equilibrium distribution will hold only for firms above the minimum.

[3] It is the ubiquitousness of these functions in size distributions of firms, as well as in distributions of wealth, incomes, city populations and a host of other, more or less unrelated, phenomena that argues most persuasively for their common base in some kind of weak probabilistic hypothesis.

good fits in both cases.[4] In the British case, we get $\rho = 1.11$, in the American $\rho = 1.23$. On the basis of these parameters, we would infer that a little less than one-fifth (18.7 per cent) of the growth in assets of the American firms was accounted for by new firms, and about one-tenth (9.9 per cent) in the British case.

It is not necessary, of course, to make indirect inferences of this sort from the steady-state distributions. Data are now available, both in Britain and the United States, that allow us to follow the changes in size of individual firms, and to construct the transition matrices from one time period to another. Hart and Prais have published such transition matrices for British business units for the periods 1885-96, 1896-1907, 1907-24, 1924-39, and 1939-50 [7, Tables 3, 4, 5, 6, 7]. From the matrices, they have been able to test directly the first assumption underlying the stochastic processes we are considering—the law of proportionate effect. They found that the frequency distributions of percentage changes in size of small, medium, and large firms, respectively, were quite similar—approximating to normal distributions with the same means and standard deviations. We found the same to be the case with the transition matrix for the 500 largest U. S. industrial corporations from 1954 to 1955 and 1954 to 1956.

A simple, direct way to test the law of proportionate effect is to construct on a logarithmic scale the scatter diagram of firm sizes for the beginning and end of the time interval in question. If the regression line has a slope of 45 degrees and if the plot is homoscedastic, the law of proportionate effect holds and the first assumption underlying the stochastic models holds. A plot of the U. S. data shows these conditions to be well satisfied for the 1955-56 period.

In addition, as an independent check of our parameter ρ, we calculated for the American firms for the years 1954-56 the quantities G (net growth in assets—for all firms above the $200 million category) and g (the part of this growth due to new firms—those entering the $200 million group). The figure obtained for g/G was 21.2 per cent which yields a ρ of 1.27. These may be compared with the respective indirect estimates, 18.7 per cent and $\rho = 1.23$, above. Thus we have obtained a close correspondence between the parameter obtained by fitting a steady-state distribution and that obtained by studying the growth of firms over time.

Thus far our data have encompassed an entire economy rather than a single industry. We justify applying the process to the whole economy on several grounds. First, the stochastic growth model we have de-

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[4] In the absence of better developed theories about goodness of fit of these skew distributions than we now have, we prefer not to make definite statements about "how good" the fits are.

scribed makes no reference to any feature of the cost curve, other than that costs are constant above some minimum point. Nothing in the model requires the firms in the sample to have the same cost curves. Second, if firms in various industries are distributed according to the Pareto curve with slopes close to 1 in each case, the composite curve for all industries will be a Pareto curve with slope close to 1. For these reasons, the arbitrariness of industry classification, and the heterogeneity of firms within industries do not create the same difficulties in applying the present theory as in applying classical cost theory to explain size distributions.

As an example of a distribution for a single industry, and because it represents an intrinsically interesting case in view of recent discussions

TABLE 1.—INGOT CAPACITIES OF TEN LEADING STEEL PRODUCERS

(Millions of Net Tons per Year, based on Capacity as of January 1, 1954)

Producer	Capacity	
	Actual[a]	Estimated
U. S. Steel	38.7	34.3
Bethlehem	18.5	17.1
Republic	10.3	11.3
Jones & Laughlin	6.2	8.5
National	6.0	6.8
Youngstown	5.5	5.2
Armco	4.9	4.8
Inland	4.7	4.2
Colorado Fuel & Iron	2.5	3.8
Wheeling	2.1	3.4
Total, 10 Companies	99.4	99.4

[a] *Source:* Actual from *Iron Age*, January 5, 1956, p. 289.

of mergers, we present in Table 1 a comparison of the actual ingot capacities of the ten leading steel producers with the theoretical capacities computed from the Yule distribution, with ρ taken at its limiting value, 1.

Perhaps the most interesting question for single industries is whether we can find any evidence of the minimum economic scale, S_m, from the size distributions. However, it is much more difficult to establish the minimum feasible size of firm than to establish the minimum feasible size of plant. The latter can often be estimated reasonably from engineering design considerations, and Bain found in most industries some consensus about this minimum scale for an efficient plant [2, Appendix B]. There is much less basis for estimating, and much less consensus about, the minimum scale for an efficient firm.

TABLE 2.—ESTIMATE OF MINIMUM FEASIBLE PLANT SIZE[a]

Industry	Bain Estimate as Per Cent of National Market	Estimate from Census Data by Yule Distribution as Per Cent of Total Value Added by Manufacture
Flour and milling	0.05 to 0.25	0.07 to 0.19
Footwear	no minimum	0.03 to 0.07
Canned fruits and vegetables	no minimum	0.06 to 0.11
Cement	0.4 to 0.7	0.14 to 0.54
Distilled liquors (except Brandy)	0.2 to 0.3	0.03 to 0.11
Petroleum refining	0.4 to 0.9	0.12 to 0.34
Meat packing	no minimum	0.3 to 0.7
Rubber tires and tubes	0.35 to 0.7	1.6 to 5.5
Rayon	1.0 to 3.0	0.14 to 0.37
Soap and glycerin	0.2 to 0.3	0.03 to 0.11
Cigarettes	1.0 or less	0.08 to 2.0
Fountain pens and mechanical pencils	1.3 to 2.5	0.06 to 0.16
Typewriters	5.0	5.7 to 14.1

[a] Minimum feasible plant size is that below which costs per unit rise substantially. The industries listed are those used by Bain, with seven omitted because of the inadequacy or incomparability of the data.

Sources: The Bain estimates were computed by multiplying his estimates of minimum efficient plant size [2, Table III, p. 72] by the fraction of their size that was encountered before costs rose substantially [2, Appendix B].

The estimates from Census Data were computed by plotting the cumulative number of firms from the 1947 *Census of Manfuacturers* against size in number of employees for the industries listed. Sharp breaks in the cumulative plot from a slope of −1 were taken as estimation points for the minimum feasible plant size and were converted to a percentage of total value added by manufacture from the same Census tables.

Taking Bain's estimates of the minimum efficient plant size, on the one hand, and Census of Manufacturers data on the size distribution of plants, on the other, we have made some preliminary attempts to compare for several industries the minimum efficient scales suggested by these two sets of data. The results are listed in Table 2. Our procedure was this: If there is a sharp increase in unit costs below some critical size, P_m, the number of plants in the industry below that size should be less than the number predicted from the Yule process. We plot cumulative numbers of plants against size on log paper, and look for sharp bends from a slope approximating −1 to a lower slope.

We have used census data for numbers of employees and converted these to per cent of total value added by manufacture. Our measure is thus comparable to Bain's which is based upon the percentage that a plant represents of total national market.

The reader can draw his own conclusions as to how far the two estimating procedures lead to similar results. Since we have made no more than preliminary explorations, we do not wish to push the point too

hard. It is clear, however, that the stochastic model provides some novel ways of interpreting the data on size distributions that may cast considerable light on the question of economies of scale. The argument runs as follows: If we take the stochastic model seriously, then any substantial deviation of the results from those predicted from the model is a reflection of some departure from the law of proportionate effect or from one of the other assumptions of the model. Having observed such a departure, we can then try to provide for it a reasonable economic interpretation.

In concluding this discussion of the data, we should like to emphasize a point made earlier—that the transition matrices may provide an even more valuable source of data about the process determining the sizes of firms and plants than the size distributions themselves. Since most of the empirical work to date has focused on the latter rather than the former, the reversal of emphasis initiated by the work of Champernowne, Hart and Prais, and others, is a very promising one.

IV. *Implications for Economic Policy*

In discussions of the degree of competition in individual industries, various measures of degree of concentration have been used. Few of these have other than an empirical basis, and the values that are obtained depend, in ways that are only partly understood, on methods of classification, cut-off points, and the like. Among the frequently used measures are Lorenz's and Gini's coefficients of concentration.

As Aitchison and Brown [1, pp. 111-16] argue, if we fit a distribution function to the observed data on the basis of a theoretical model, it is reasonable to base our measures of concentration on the parameters of the distribution function. Thus, they propose the standard deviation of the log-normal as an appropriate measure of dispersion, and show that the Lorenz and Gini coefficients can be expressed as functions of that statistic.

Similarly, if we use the Yule process to account for the distribution of firm sizes, our interpretation of the observed phenomena should be based on the estimated values of the parameters of the distribution. In the simplest case, the only one we have considered here, there is a single parameter, ρ. We have already provided an economic interpretation for this parameter in the previous section—it measures, in a certain sense, the rate of new entry into the industry. Hence, in this particular model, the concentration in an industry is not independently determined, but is a function of rate of new entry.

We may put the matter more generally. If firm sizes are determined by a stochastic process, then the appropriate way to think about public policy in this area is to consider the means by which the stochastic

process can be altered, and the consequences of employing these means. As a very simple example, if the rate of entry into the industry can be increased, this will automatically reduce the degree of concentration, as measured by the usual indices. Similarly, if, through tax policies or other means, a situation of sharply increasing costs is created in an industry, this situation should cause a departure of the equilibrium distribution from the Yule distribution in the direction of lower concentration.

A third, and more complicated, example is this: the amount of "mixing" that takes place—reordering of the ranks of firms in an industry —depends on the dispersion of the columns of the transition matrix. The same equilibrium distribution may be produced with various degrees of mixing, since the latter can vary independently of the law of proportionate effect. Public policy might be concerned with the amount of mobility rather than with the resulting degree of concentration. As a matter of fact, a measure of mobility (for firms or individuals) would appear to provide a better index of what we mean by "equality of opportunity" than do the usual measures of concentration.

The net effect of approaching the subject of industrial concentration in this way will be to make the classical theory of the firm much less relevant to the subject, but theories of economic development and growth much more relevant. When we have a collection of adaptive organisms placed in a relatively stable environment, we can often make strong predictions about the resulting state of affairs by assuming that the system will come into a position of stable, adaptive equilibrium. When, however, the environment itself is changing at a rate that is large compared with the adaptive speeds of the organisms, we can never expect to observe the system in the neighborhood of equilibrium, and we must invoke some substitute for the static equilibrium if we wish to predict behavior. Our main objective in this paper is to suggest the need for, and the availability of such a substitute with which to analyze the size distribution of firms.

V. Directions for Research

We have emphasized the tentative character of our results, and should like to suggest in conclusion some directions of research that look exceedingly promising:

1. We need to accumulate a body of knowledge about skew distribution functions and the processes that generate them that is comparable to the rich knowledge we possess about the normal, Poisson, exponential, and related distributions. We need to know more about the relations between the distributions and the 'generating processes, about efficient methods for estimating parameters, about the distributions of these

estimates, and about efficient methods for choosing among alternative hypotheses.

2. We need to develop stochastic models of economic growth that embody as much knowledge as we have, or can acquire, about the underlying processes.

3. We need to re-examine the corpus of economic data to see what part of it can profitably be explained or reinterpreted in terms of such economic models.

4. We need to re-examine those principles of public policy that are based on static equilibrium analysis to see what part of them will remain and what part will be altered as stochastic processes begin to play a larger role in our explanation of economic phenomena.

In this paper we have tried to suggest some of the directions in which inquiry may lead if it is guided by questions such as these.

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A SKEPTICAL NOTE ON THE CONSTANCY OF RELATIVE SHARES

By ROBERT M. SOLOW*

Ever since the investigations of Bowley and Douglas it has been widely believed that the share of the national income accruing to labor is one of the great constants of nature, like the velocity of light or the incest taboo. Keynes [12, p. 48] called it "a bit of a miracle." Even if it is sometimes observed that the pattern of distributive shares shows long-run shifts or short-run fluctuations, the former can be explained away and the latter neglected on principle. The residual belief remains that, apart from a slight (and questionable) upward trend and a countercyclical movement, the share of wages in the privately produced national income is unexpectedly stable. Much effort is devoted to exploiting and explaining this fact.

The object of this paper is to suggest that, like most miracles, this one may be an optical illusion. It is not clear what exactly is meant by the phrase: "The wage share in national income is relatively stable" or "historically almost constant." The literature does not abound in precise definitions, but obviously literal constancy is not in question. In any case, what I want to show is that for one internally consistent definition of "relatively stable," the wage share in the United States for the period 1929-1954 (or perhaps longer) has not been relatively stable.

If this contention is accepted, it is not without some general implications for economic theory. Beginning with Ricardo there have been sporadic revivals of interest in macroeconomic theories of distribution.[1] Now it is possible to have an aggregative distribution theory without believing in the historical constancy of relative shares, but the belief certainly reinforces the desire for such a theory. After all, a powerful macroeconomic fact seems to call for a macroeconomic explanation. It need not have one, but that is beside the point. As Kaldor says [9, p. 84]:

[1] I suppose the main contributors since Paul Douglas [4] have been M. Kalecki in [10] and, more recently [11, Ch. 2]; K. Boulding [2, Ch. 14]; N. Kaldor [9]; Kaldor's main argument was anticipated five years earlier by F. H. Hahn [6].

. . . no hypothesis as regards the forces determining distributive shares could be intellectually satisfying unless it succeeds in accounting for the relative stability of these shares in the advanced capitalist economies over the last 100 years or so, despite the phenomenal changes in the techniques of production, in the accumulation of capital relative to labor and in real income per head.

But if, in fact, relative stability of distributive shares is at least partially a mirage, one may feel freer to seek intellectual satisfaction elsewhere. There is still a lot to be explained.

I. *How to be Constant though Variable*

Table 1 shows the share of compensation of employees in a number of different aggregate income totals, so that the reader can see what kind of variability occurs, over the business cycle and over longer periods.

What does an economist mean when he says that the wage share has been relatively stable? Since he does not mean that it has been absolutely constant, he must mean that in some sense or other it has been more nearly constant than one would ordinarily expect.[2] The sentence already quoted from Kaldor suggests that since technique, real capital and real income per head have all changed "phenomenally," you would normally expect distributive shares to have changed "a lot," but they have only changed "a little" and this requires a special explanation. Not to split verbal hairs, it is evident that this is no definition at all. One must have some standard by which to judge whether some particular series of observations has fluctuated widely or narrowly.

Such standards of comparison can arise in a variety of ways. A tight theory may itself provide a benchmark. For example, the fraction of males among live births in a well-defined animal population is subject to statistical fluctuations from year to year. But the theory of sex determination, although perhaps not complete, gives some indication of how variable one ought normally expect the series to be. To say that the series is relatively stable could then simply mean that the observed variance is significantly less than the variance expected from the theory. Something like this does appear to be in the back of some authors' minds when they refer to the stability of the wage share. Take as a starting-point the neoclassical general equilibrium theory of distribution, which is formulated in terms of production functions, input-ratios, and the like. These quantities fluctuate over time. Ought not the pattern of distributive shares show comparable variability, according to the theory?

[2] A sporting colleague of mine once offered to bet that Vincent Impellitteri would get more votes for Mayor of New York City than most people expected.

TABLE 1.—SHARE OF COMPENSATION OF EMPLOYEES IN VARIOUS INCOME TOTALS,
1929–1955

Year	As Per Cent of National Income	As Per Cent of Privately Pro-duced Income	As Per Cent of Income Originating in Corporate Business	As Per Cent of Income Originating in Manufacturing
1929	58.2	55.6	74.6	74.2
1930	61.8	57.3	78.7	76.7
1931	66.5	63.2	87.9	88.0
1932	73.2	69.3	101.0	108.0
1933	73.4	69.5	101.6	104.7
1934	70.0	69.6	88.3	89.4
1935	65.3	60.8	83.8	82.6
1936	66.1	61.3	80.0	78.3
1937	65.1	61.0	79.9	78.7
1938	66.6	61.8	83.0	83.3
1939	66.1	61.6	80.9	79.9
1940	63.8	59.5	76.2	73.4
1941	61.9	57.6	72.7	69.0
1942	61.9	56.8	71.7	71.1
1943	64.4	57.6	72.2	73.4
1944	66.4	58.8	73.8	74.8
1945	68.0	59.8	77.0	77.3
1946	65.5	60.6	79.9	78.8
1947	65.3	61.7	77.5	75.9
1948	63.6	60.0	74.8	72.9
1949	65.2	61.2	75.7	73.5
1950	64.3	60.4	73.6	70.8
1951	65.1	60.8	74.0	71.1
1952	67.2	62.8	76.7	75.4
1953	68.9	64.5	78.5	77.5
1954	64.4	65.0	79.6	79.3
1955	68.9	64.7	77.4	76.5

Source: Department of Commerce, *Surv. Curr. Bus., National Income* Supplement, 1954, and July 1956.

But there is a world of difference between this case and the genetic illustration. The general equilibrium theory is in the first instance a microeconomic one. Between production functions and factor-ratios on the one hand, and aggregate distributive shares on the other lies a whole string of intermediate variables: elasticities of substitution, commodity-demand and factor-supply conditions, markets of different degrees of competitiveness and monopoly, far-from-neutral taxes. It is hard to believe that the theory offers any grip at all on the variability of relative shares as the data change—in fact this may be viewed by some as a symptom of its emptiness. A license to speculate, maybe, but hardly a firm standard. As a matter of speculation, the theory might be taken to imply that the aggregate shares come about through a kind of averaging process, in which many approximately independently changing

parameters intervene. From this view would follow an expectation of "relative stability," if anything.

A second possible source of a standard of variability is suggested by the analogy of statistical quality control. There the problem is also one of detecting "excessive" variability (or sometimes even deficient variability). But in the absence of some outside specification, the standard is usually given by the past behavior of the process itself. Clearly if the wage share had once oscillated between 50 and 80 per cent and now moved only in the range from 60 to 70 per cent, we could speak of relative stability. But it is not claimed that this is the case.

Third, the contrast between micro- and macroeconomic theories suggests that it might be possible to formulate an *internal* standard of variability. A hint in that direction is contained in a remark of Phelps Brown and Hart [14]: "Yet it still remains true that the changes in the share of wages in national incomes are not so great as we should expect when we look at the often wide swings of the corresponding shares within particular industries, and this relative stability also calls for explanation." Indeed it does; if the calorie contents of breakfast, lunch, and supper each varies widely, while the 24-hour total remains constant, we at once suspect a master hand at the controls. Similarly if wide swings within industries yield only narrow swings in the aggregate, this points to some specifically interindustrial or macroeconomic force.

But relative shares have denominators as well as numerators. However we subdivide the economy, the over-all share will be a weighted average, not a sum, of the respective shares for the subdivisions. This does not automatically entail that the over-all share will have a smaller variance than the sector shares. That all depends on the intersector correlations, i.e., on the macroeconomic forces. Note an interesting consequence: it is *negative* correlations between sectors which reduce the variance of the weighted average.

Here we have something empirically testable. Suppose, to take the simplest possible case, the economy is divided into k equal-sized sectors, in each of which the wage share is equally variable through time. Then if the sector shares fluctuate independently, the aggregate wage share will have a variance only $1/k$ times the common sector variance. If this were in fact the picture, it would be hard to claim that the relative stability of the aggregate shares required a specifically macroeconomic explanation. It might still be claimed that the aggregate share is more stable than it ought to be on this hypothesis, but now the explanation would have to be sought in the excessive stability of the individual sector shares. I suppose it could be plausibly argued that there are macroeconomic reasons for such microeconomic stability, but this is not the form that current theories take.

The more general case is no more complicated. Suppose there are k sectors, with shares $S_1, \ldots, S_k$ and weights in the aggregate $w_1, \ldots, w_k$. If the S_i represent the share of wages in the sector value-added, the w_i will represent the share of the sector value-added in the total. Let σ_i^2 be the variance of S_i through time, and let symbols without subscripts represent the aggregate share and its variance. Then in the null case of independence among sectors we would find:

$$(1) \qquad\qquad \sigma^2 = \sum_1^k w_i^2 \sigma_i^2,$$

and in any case we would have

$$(2) \qquad\qquad S = \sum_1^k w_i S_i.$$

Predominantly positive correlations among sectors will yield a larger σ^2 and negative correlations a smaller σ^2.

The value-added weights, however, are not constant from year to year. And on the face of it changes in the weights might be expected to be the main intersectoral force accounting for the relative stability of the aggregate share. If in fact the aggregate share fluctuated less than the sector shares would suggest, this might come about through countershifts in the weights: low-share sectors gaining in weight at the expense of high-share sectors when sector shares rise, and vice versa. There are good theoretical reasons why this might occur, but the fact is that it does not.

This subsidiary proposition is easily testable. It is only necessary to recompute the over-all shares using the observed sector shares but some fixed set of base-year weights. This has been done by Kalecki [11, p. 32] for U. S. manufacturing, 1879-1937, and by Edward F. Denison [3, p. 258] for the "ordinary business sector," 1929-1952. In both cases the fixed-weight series showed approximately the same amplitude of fluctuation as the observed series. The same conclusion can be read from the data to be analyzed below. Short- and long-run changes in the importance of the various sectors are important economic facts,[3] but they are not what accounts for the variance or lack of variance of the over-all shares. Thus in making use of formulas 1 and 2 I have in each case recalculated the averages using the value-added weights of a fixed base-year, usually somewhere in the middle of the period.

[3] James W. Beck [1] explicitly investigates short-run changes in over-all shares during the three periods 1930-32, 1941-43, 1950-53. Only in the second of these were weight-shifts a predominant factor. One wonders whether commodity substitution would not prove to be more important in a finer industry classification. John Dunlop, in his pioneering study [5, esp. pp. 163-91], also found weight-shifts to be a significant factor for the period 1929-34.

II. *Empirical Results*

The sector shares in Table 2 were calculated from the 1954 *National Income* Supplement to the *Survey of Current Business* (pp. 176-79). In each case they represent the ratio of "compensation of employees" to "national income originating."[4] The original data are reported for eleven sectors, not the seven used here. The four disappearing sectors are: Rest of the World; Government; Finance, Insurance, and Real Estate; and Services. The Rest of the World is a horse of a wholly different color. Government had to be dropped because our quaint

TABLE 2.—SHARE OF COMPENSATION OF EMPLOYEES IN INCOME ORIGINATING IN SELECTED SECTORS OF THE ECONOMY FOR SELECTED YEARS, 1929–1953

Sector	Weight	1929	1935	1937	1939	1941	1947	1951	1953	Variance
Agriculture, etc.	.113	.170	.134	.153	.185	.162	.170	.162	.206	.0004
Mining	.031	.751	.813	.715	.761	.705	.733	.704	.740	.0013
Contract Construction	.056	.667	.709	.704	.710	.733	.727	.759	.766	.0010
Manufacturing	.441	.742	.826	.787	.799	.690	.739	.711	.711	.0021
Wholesale and Retail Trade	.230	.702	.726	.691	.701	.624	.633	.650	.670	.0013
Transportation	.084	.725	.800	.812	.785	.717	.840	.805	.815	.0018
Communications and Public Utilities	.044	.541	.540	.560	.550	.543	.697	.619	.604	.0030
Total (Current Weights)		.647	.658	.656	.675	.613	.653	.631	.696	.0007
Fixed-Weight Total		.652	.702	.677	.683	.613	.666	.642	.678	.0008

accounting practices measure the value of its product by the compensation of its employees, so that by assumption no income is ever imputed to government-owned capital assets. I dropped the other two noncommodity-producing sectors on the grounds that the value-added concept is rather vague for them, and in many cases probably bears no remotely technological relation to conventional inputs. One could make a similar (but weaker) case for not including Trade, and one could argue that the imputation to wages in Agriculture may depend heavily on shifts between family and hired labor; but I have kept both in an effort to widen the coverage. The sector shares are shown for a selection of eight years between 1929 and 1953 but not for all. This was a perhaps unwise attempt to avoid the deep depression years and the war period.

[4] National income originating is a slightly more net concept than value added, since it excludes depreciation charges, indirect business taxes, and business transfers. Compensation of employees is the sum of wages, salaries, and the usual supplements. The figures no doubt exclude certain payments which logically ought to be imputed to labor, particularly part of the earnings of unincorporated enterprise. Cf. [3, p. 256] Probably the salary data also catch certain payments which function more like profits. I doubt that these "errors of observation" can influence the broad results substantially.

The table shows both the current-weighted over-all labor share and a fixed-weight series using the weights of 1941. In only one year does the use of fixed weights result in a change in the aggregate share of more than 2 percentage points, and the variability, as measured by the variance, is affected hardly at all. In part this is because the weights do not change radically, the main shift being a decrease in the relative weights of Agriculture and Transportation between 1929 and 1953, with Manufacturing gaining.

The last column shows the variance of each sector share and of the two aggregate-share series. The fixed-weight aggregate has a variance of .0008. If formula 1 is used to calculate a theoretical variance on the assumption that the sector shares moved independently in a statistical sense, it turns out to be .0005. This difference is almost certainly not statistically significant. We would have to conclude that the aggregate share varied just about as much as it would vary if the individual sector shares fluctuated independently, with positive and negative intercorrelations approximately offsetting each other. If anything, the aggregate share fluctuated a bit *more* than the hypothesis of independence would indicate. Anyone who believes that the aggregate share over this period was unexpectedly stable must believe the same of the sector shares and presumably seek the explanation there.

In Table 3, data from the Census of Manufacturing are analyzed in the same way. With the exception of 1941 and the substitution of 1954 for 1953, the same years are represented. Now the ratios give the share of wages only ("production workers' wages") in value added.[5] The fixed-weight average is calculated with weights equal to the 1947 fraction of each industry group in the aggregate value added. Once again the use of fixed weights makes only a negligible difference. In no year do the shares with fixed and current weights differ by as much as 1 per cent. The seven-year variance of the observed aggregate shares is .00028, and for the fixed-weight aggregate it is slightly increased to .00036.

But there is a striking difference between the behavior of the Manufacturing data and the wider Commerce figures. When a theoretical variance is calculated from formula 1, i.e., on the assumption that industry shares are statistically independent, it turns out to be only .00007. This is one-quarter of the observed share variance and one-fifth of the variance of the fixed-weight over-all share. And this substantial difference is in the "wrong" direction. The share of wages in manufacturing value-added fluctuates noticeably *more* than it would if the

[5] There are plenty of anomalies as between Table 2 and Table 3. Presumably they reflect the differences in concept between Census and Commerce data, as well as sheer observational error.

industry shares were mutually uncorrelated. This implies that there is predominantly positive intercorrelation among the wage shares in the separate industries. Instead of a special explanation of the relative stability of the over-all wage share in manufacturing, we appear to need just the reverse: an accounting for its tendency to fluctuate too much.

There are various ways of explaining the facts. Perhaps it is a fair

TABLE 3.—SHARE OF PRODUCTION WORKERS' WAGES IN VALUE ADDED, SELECTED MANUFACTURING INDUSTRY GROUPS, SELECTED YEARS, 1929–1934

Industry Group	Weight	1929	1935	1937	1939	1947	1951	1954	Variance
Food	.121	.268	.287	.291	.257	.285	.297	.281	.00019
Tobacco	.009	.238	.208	.215	.194	.273	.224	.222	.00064
Textile Mill	.072	.475	.575	.545	.499	.459	.540	.532	.00173
Apparel, etc.	.060	.355	.483	.483	.474	.454	.488	.490	.00233
Lumber	.034	.483	.541	.536	.502	.473	.493	.503	.00065
Furniture, etc.	.019	.422	.466	.470	.438	.475	.453	.454	.00020
Paper	.039	.359	.370	.360	.356	.352	.332	.362	.00014
Printing and Publishing	.057	.284	.287	.297	.279	.309	.339	.338	.00063
Chemicals	.072	.199	.206	.212	.189	.232	.212	.212	.00018
Petroleum and Coal	.027	.207	.237	.300	.256	.276	.265	.301	.00114
Rubber	.018	.385	.432	.465	.397	.472	.425	.407	.00109
Leather	.021	.464	.526	.528	.504	.473	.521	.509	.00066
Stone, Clay, Glass	.031	.417	.380	.389	.361	.431	.410	.392	.00056
Metals and Products	.158	.414	.450	.462	.427	.479	.424	.415	.00045
Nonelectrical Machinery	.105	.392	.446	.410	.380	.460	.438	.404	.00081
Electrical Machinery	.052	.341	.350	.369	.335	.423	.396	.357	.00109
Transportation Equipment	.079	.399	.497	.518	.494	.501	.477	.431	.00185
Miscellaneous	.028	.243	.370	.410	.372	.441	.434	.416	.00461
Total (Current Weights)		.358	.395	.402	.383	.407	.398	.382	.00028
Fixed-Weight Total		.357	.403	.406	.376	.408	.401	.389	.00036

idealization that the several industries buy their labor and capital inputs in the same or similar markets, so they can be imagined to face the same factor prices. If it is further assumed that each industry produces a single commodity with a technology describable by a smooth production function, then everything will depend on the distribution of elasticities of substitution among industries. If nearly all elasticities of substitution are on the same side of unity, then the wage shares will go up and down together in nearly all industries and there will be strong positive correlation. If elasticities of substitution are evenly divided on both sides of unity, there will be two groups of industries whose wage shares will move in opposed phase. Whether the net result is to increase

or reduce the variance of the aggregate wage share as compared with the hypothetical zero-correlation value will depend in a complicated way on the arrangement of weights and elasticities.

A special case occurs if each industry is imagined to produce a single commodity with a single fixed-proportions technique. Then every elasticity of substitution is zero and all wage shares move together. It is more interesting to recognize that each "industry" in Table 3 produces many commodities, some of which are complementary with each other in consumption and some of which are rival. Even if each commodity within an industry is produced by a single technique, it is no longer certain that the industry's wage share will rise and fall with the wage rate. The wage share for each commodity will rise with the wage rate, but those commodities whose production is labor-intensive will rise in price relative to others (assuming some degree of competition) and the intra-industry commodity-mix may shift in favor of capital-intensive commodities enough to decrease the wage share. The outcome depends in an easily calculable way on the factor proportions required by each technique and on the elasticities of substitution in consumption. If in addition commodities are producible with varying factor proportions, then once again the elasticities of substitution in production will play a role along with the other parameters [7, p. 8].

It must be admitted that none of this is very informative. It is all too static, too inattentive to technical change, too free with unknown and unknowable parameters—in a word, too neoclassical. It would be nice to have a single aggregative bulldozer principle with which to crash through the hedge of microeconomic interconnections and analogies. It is not inconceivable that the bulldozer may yet clank into view; but it is by no means inevitable either.

It is not clear how the newly popular widow's cruse theories (according to which the share of profits in income depends, given full employment, essentially on the rate of investment) can be made to apply on the somewhat disaggregative level to which my empirical results seem to force me. The stickiness of money wages, which forms the short-run side of Kaldor's theory [9, p. 95], may indeed have something to do with the results of Table 3, although that can hardly be the whole story. [The data next to be presented confirm the suggestion that Table 3's peculiarities are short-run in character.]

There are still other short-run facts that might help to explain the tendency of Table 3's industry shares to move together. An inclination to hoard skilled labor when output declines is one; the longer duration of collective bargaining agreements is another. In Table 4 the attempt is made to wash out some of the short-run effects by using decennial census data over a longer period of time. The layout is the same as that

of Table 3, but the coverage is necessarily poorer and the industrial breakdown cruder, because of changes in classification over the years. Broadly speaking, expectations are confirmed.

Once again, the use of fixed (1929) value-added weights results in only a slight increase in the variance of the aggregate wage share as compared with the observed totals. The variance of the observed totals is .0003, that of the fixed-weight totals is .0004. (Note that the difference between standard deviations, in natural units, is only the difference between ,017 and .020.) Moreover, a good part of this small increase is due to the single very high observed wage share in transportation equip-

TABLE 4.—SHARE OF PRODUCTION WORKERS' WAGES IN VALUE ADDED, SELECTED MANUFACTURING INDUSTRY GROUPS, SELECTED YEARS, 1899–1951

Industry Group	Weight	1899	1909	1919	1929	1939	1951	Variance
Food	.121	.223	.212	.291	.268	.257	.297	.001
Textiles	.150	.462	.449	.363	.420	.488	.515	.002
Metals, etc.	.320	.453	.456	.475	.395	.400	.424	.001
Lumber	.073	.452	.488	.495	.465	.470	.480	.0002
Leather	.027	.532	.480	.405	.464	.504	.521	.002
Paper and Printing	.109	.357	.332	.331	.304	.304	.336	.0004
Chemicals	.064	.223	.216	.265	.119	.189	.212	.001
Stone, Clay, Glass	.038	.548	.543	.486	.417	.361	.410	.006
Tobacco	.014	.284	.288	.234	.238	.194	.224	.001
Transportation Equipment	.086	.671	.474	.440	.399	.494	.477	.009
Total (Current Weights)		.412	.389	.395	.368	.370	.400	.0003
Fixed-Weight Total		.424	.404	.404	.367	.384	.409	.0004

ment in 1899, together with the fact that the weight of this industry increased from 1899 to 1929. It seems just possible that the character of the output of the industry was changing around the turn of the century. Although this effect does not appear to be very strong in the data here analyzed, I suspect that analysis on a finer commodity classification might well show that shifts in the composition of output do have an effect in reducing fluctuations in aggregate shares.

The theoretical variance, calculated from formula 1 on the assumption of the independence of industry shares, is .00025. This is less than the observed figure of .00040, but probably not significantly so. (The standard deviations are .016 and .020.) In any case the wide discrepancy found in Table 3 has all but disappeared. This confirms the belief that the positive association of industry shares in Table 3 was essentially short-run in nature. For long periods in manufacturing, and even for short periods in the grosser sector breakdown of Table 2, the data are compatible with the hypothesis that subgroup shares fluctuate approximately independently through time; or, more accur-

ately, that positive and negative intercorrelations approximately cancel out.

In general, the data we have examined suggest the following: if by the "historical constancy" of labor's share it is meant that the share of the total social product imputed to wages has shown a marked absence of fluctuation as compared with the fluctuations of its industrial components, then this belief is probably wrong. Whatever exceptional stability there has been in the pattern of relative shares appears attributable to the components. This in turn suggests that there is no need for a special theory to explain how a number of unruly microeconomic markets are willy-nilly squeezed into a tight-fitting size .65 straitjacket. A theory which wishes to produce the magic number among its consequences may have to say something about the component sectors among its premises.

III. *The Character of Trends*

There are still some interesting problems to be found among the sectors and in the aggregates. One such—and some economists would no doubt prefer to phrase the whole "historical constancy" question in these terms—is the mildness of the observable trends in the sector shares and in the aggregate relative shares. The history of western capitalism is supposed to be characterized by a long-run accumulation of capital relative to labor. We expect this trend to result in *some* trend in the distribution of the product. Why do we not observe a stronger one?

First, let us look at the orders of magnitude involved. No great accuracy is possible because of the difficulty of finding a reasonable measure of capital stock, because no two available time series are conceptually identical, and finally because of the imputation problem involved. Roughly speaking, during the first half of this century the capital/labor ratio for the private nonfarm sector rose by about 60 per cent. But most or all of the increase took place before 1929. Between 1929 and 1949 there was little change, possibly even a decline. In manufacturing the contours were broadly similar, although the initial increase in the capital/labor ratio during the period 1900-1929 was considerably greater.[6]

So far as distributive shares are concerned, it is generally accepted that there has been a slight tendency for the labor share to increase secularly. But before 1929 the trend was approximately horizontal[7]

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[6] I am leaving aside the period since 1949, which saw a new burst of net capital formation together with an approximately normal growth of the labor force.

[7] See for instance S. Kuznets [13, p. 86]. D. Gale Johnson's calculations [8, p. 178] show the labor share rising from 69.4 per cent in the decade 1900-1909 to 75.2 per cent

(with some short-run movements); between 1929 and 1949 there is a more pronounced upward tilt in the wage and salary share as Table 1 shows.

What lends mystery to this picture is that in the first quarter-century, when capital accumulates much more rapidly than the labor force grows, the distributive share picture shows little or no trend. But in the second quarter-century, when the growth of capital relative to labor slows down or ceases, the wage share begins to rise. It seems likely that the difference between the two periods may be tied up with a slightly higher rate of technical progress in the years since 1929.

But let us accept the notion that economic history shows us a strong tendency for capital to grow relative to labor. We are then led to expect a strong trend in relative shares. But which way? The neoclassical answer is that this depends on "the" elasticity of substitution, or rather on the distribution of elasticities of substitution on either side of unity.[8]

Here we run up against the same kind of verbal question that occupied us earlier. What is a "strong" trend in relative shares? And what constitutes an elasticity of substitution "substantially" different from unity in terms of common-sense expectations? And how different from unity need the elasticity of substitution be in order that it convert a strong trend in the capital/labor ratio into a strong trend in relative shares? For the case of a two-factor, constant-returns-to-scale production function, it is not hard to calculate that the elasticity of the labor

share with respect to the capital/labor ratio is $-S_K(1 - \dfrac{1}{\tau})$ where S_K

is the share of property in income and τ is the elasticity of substitution. Is an elasticity of substitution of $\frac{2}{3}$ substantially different from unity? It means that a 10 per cent change in the relative costs of capital and labor services will induce a 6.7 per cent change in the capital/labor ratio. If $\tau = \frac{2}{3}$ and $S_K = .30$, the elasticity of the labor share with respect to the capital/labor ratio is .15. Thus if the capital/labor ratio rises by 60 per cent (with $\tau = \frac{2}{3}$) the labor share should rise by 9 per cent. And since the labor share hovers around .70, this means a rise of about 6 or 7 percentage points. But this is just the order of magnitude observed!

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for 1940-1949, with nearly all the change coming after 1915-1924. Johnson's figures are for the whole economy and include, besides the direct compensation of employees, an allowance for the labor content of entrepreneurial earnings. The corresponding figures for compensation of employees are 55 per cent and 64.3 per cent. When restricted to the private sector, compensation of employees amounts to 53 per cent of privately produced income in 1900-1909, and 59 per cent in 1940-1949. When the allowance for entrepreneurial earnings is made on the private sector basis the figures are 68 per cent and 71.5 per cent.

[8] Remember that shifts in the weights of different sectors in the total appear not to count for very much.

I don't mean to conclude from this example that yet another problem evaporates. But before deciding that observation contradicts expectation, there is some point in deciding what it is we expect. In this case what needs precision is the notion of substitutability, and the problem is complicated further by the need to consider changes occurring over varying periods of time.

There are even more fundamental obstacles to a clear evaluation of the argument about trends. An unknown fraction of society's capital takes the form of the improvement of human abilities and skills. Casual observation suggests that this fraction has been increasing over time. Correspondingly an unknown fraction of what we call wages, even "production workers' wages," no doubt constitutes a rent on that human capital. So the true quantitative picture is far from clear. If it were possible to separate out the part of nominal wages and salaries which is really a return on investment, the share of property income in the total might be found to be steadily increasing. An alternative way of looking at it is to say that investment in education, training, public health, etc., has the effect of increasing the efficiency of the human agent, so that a measurement in man-hours underestimates the rate at which the labor force grows as properly measured in efficiency units. In this case it might be found that the accumulation of nonhuman capital does not proceed at a faster rate than the labor force grows. These are intrinsically difficult distinctions to draw empirically, but they hold much theoretical and practical importance.

There are of course still other discrepancies between the data we have and the analytical concepts to which we pretend they correspond. The problem of imputing to labor a proper share of the income of unincorporated enterprises has received some attention. But even in the corporate sector possibilities exist for converting what is "really" property income into nominal labor income, and vice versa, and there are often tax reasons for doing so. If this were a random effect in time it would do no great harm, but in fact it may behave more systematically than that.[9]

To complete the catalog of uncertainties about trends, I ought to mention the intrusion of technical change between the simple facts of factor ratios and factor rewards. About the incidence of historical changes in techniques little is known, and without this it is difficult to know what residual remains to be accounted for.

[9] Johnson [8, pp. 180-82] shows that some part of the apparent increase in the labor share is to be attributed to such statistical artifacts as the growing importance of government-produced income, all of which is conventionally imputed to labor, and the declining importance of agriculture and therewith of home-produced and home-consumed goods.

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ECONOMIC PROBLEMS IN AIR FORCE LOGISTICS

By HORST MENDERSHAUSEN*

Economic analysis has only lately been applied to military logistics. The supply and transportation problems of the armed forces used to be sufficiently small and specialized in peacetime to be left to military routine; and sufficiently overshadowed by economic mobilization problems in wartime to warrant neglect even then. This is no longer true. As instantaneous readiness for combat has come to be ever more important for the armed forces and as the national resources devoted to military logistics in peacetime have grown to respectable size, the allocation of resources between alternative uses and over time can no longer be handled by simple routines or the magic of command. The logistics problems of the modern military establishment are sufficiently permanent and complex to invite economic analysis. The purpose of the present article is to demonstrate this for a particular part of the field, the Air Force sector, in which a good deal of analytical work has been done.

In recent years, the United States has been spending nearly $20 billion annually on its Air Force. This stream of funds is channeled to certain military-political tasks and to certain organizations and weapons that may meet these tasks. The problem of how to make these fundamental allocations will not hold us here; but we shall be concerned with the efficiency of the Air Force in providing for the chosen tasks with the resources at its command.

Efficiency problems, costs and resource implications also enter into the selection of the tasks and the principal weapons themselves. There are no absolute military requirements just as there are no absolute standard-of-living requirements. But assuming that the objectives are properly defined and the means well chosen, the activity still may be "too expensive"—because it is inefficient. Conversely, because of inefficiency, the military program may skimp on important provisions and fail to meet worthy objectives.

This problem is the logistics problem par excellence. How can the Air Force maximize the military capabilities that given budget resources afford; or put differently, how can it minimize the cost of

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attaining stipulated capabilities? In what directions can improvements be found? How can Air Force practice be brought closer to an "optimum"? Logistics research is meant to answer questions of this sort.

I. *The Air Force Logistics System*

The Air Force logistics system comprises many institutions and activities ranging from industrial contractors to the combat units. Contractors' activities extend from production of weapons and parts to stockage, transport and maintenance work. Air Force depots deal with purchasing, repair and major maintenance work, and with the "wholesale functions" of stock-keeping and distribution. Air base logistics activities connect with contractors or Air Force depots, include stockage at the "retail" level and extend forward to the maintenance functions attached to squadrons, missile sites, etc. Transportation weaves through this entire system, carrying new materiel forward, reparable items backward, stock reallocations sideways, and projects outward into the carrying of combat units and their supplies to destinations. The communications system is a vital part of the whole.

The size of the logistics system escapes precise measurement because it resists precise definition. Etymologically, logistics is what it takes to "quarter troops"; translated into present-day organization and technology, this may mean the entire military budget, and more. But when military people speak of logistics today, they usually mean the functions of procurement and supply, maintenance, and transportation. The Air Force expenditures that can be more or less readily assigned to these functions, across all commands, are shown in Table 1. Even excluding investment expenditures outside the Air Materiel Command (*e.g.*, for installations), and expenditures on operational supplies (*e.g.*, fuel), motor pools—all of which might well be counted as logistics too —these expenditures take up about two-thirds of the Air Force budget.

In any sense, Air Force logistics is a vast enterprise. With many thousands of air vehicles to support, more than a million distinct articles ("line items") to supply, $14 billion worth of inventory to stock, and 15 continental and 5 overseas depots and about 250 air bases to manage, the Air Force logistics system matches in complexity any existing organization.

II. *A Typical Logistics Problem*

The typical logistics problem is a variant of the classical production problem. Stated in the "economy version" (given the output objective, minimize input),[1] the problem has the following shape: An operational

[1] In the "efficiency version," total input (e.g., the budget) is given, output to be maximized.

TABLE 1.—LOGISTICS FUNCTIONS IN THE AIR FORCE BUDGET, FISCAL YEAR 1956

Command	Expenditures on Functions[a] (millions of dollars)				
	Procurement and Supply	Maintenance	Transportation	Overhead AMC	Total Logistics Functions
Air Materiel Command					
Aircraft and related procurement	6,074	—	—	—	6,074
Other procurement	1,182	—	—	—	1,182
Operating expenditures	275	874	134	770[b]	2,053
Strategic Air Command	73	223	8	—	304
Tactical Air Command	19	59	3	—	81
Air Defense Command	27	100	3	—	130
Military Air Transport Service	25	120	44	—	189
Air Training Command	45	140	6	—	191
Air Research & Development Command	14	34	2	—	50
U. S. Air Force, Europe	32	71	10	—	113
Far East Air Force	33	66	5	—	104
Other Commands	43	85	10	—	138
All Commands, logistics functions	7,842	1,772	225	770	10,609
All Commands, other expenditures Investment expenditures, not AMC	—	—	—	—	1,340
Other operating expenditures, not AMC	—	—	—	—	4,761
Total Air Force expenditures	—	—	—	—	16,710

[a] Including military and civilian personnel expenditures, materiel and contract services consumed in performing these functions, and Headquarters services.

[b] Including $76 million for construction of AMC installations. Installation expenditures by other commands are not included in the expenditures on logistics functions; they appear under "Investment Expenditures, not AMC."

Source: U. S. Air-Force, AFC-100, Expense Account Report, 3 October 1956; and information obtained from the Bureau of the Budget.

program of the Air Force is to be fulfilled. Various resources must be combined to attain the objective. Units of each of these resources are available at certain costs. The resources can be combined in various ways which may be expressed in a logistics model (production function). The desired combination can be defined by a certain criterion, e.g., minimum total monetary cost.

Suppose the logistics system is given the task of supporting at minimum cost a program for a certain aircraft type. The program consists, let us say, of having a certain number of aircraft operationally ready,

that is, fully equipped and *not* out of commission for lack of parts or maintenance. The system may do three things: buy aircraft, supply spare parts, and provide personnel and equipment for maintenance work. With a larger number of aircraft procured, the program may be met despite the fact that some aircraft will be out of commission for parts (AOCP), for maintenance (AOCM), or not fully equipped (ANFE). With a more effective supply of spare parts, the program may be met through a reduction of the number of AOCP's or ANFE's; and with more effective maintenance, through the reduction of AOCM's. From the point of view of the military objective it is immaterial in which of the three ways the operationally ready aircraft are generated.

Assume, for simplicity, that supply and maintenance are each homogeneous activities, measurable in dollars, and that the cost of one unit of each of these three activities is independent of the amounts purchased. The analysis may begin by considering a certain number of aircraft obtainable through procurement. It may then look for the various combinations of supply and maintenance which will keep the number required by the program operationally ready. Suboptimization leads to the least-cost combination of supply and maintenance and its cost. The suboptimization is then repeated for other numbers of aircraft available.

Once we know the best combinations of supply and maintenance (logistics support) to use with each number of aircraft available, we may consider combinations of aircraft purchases and current support costs that can achieve the program. Finally, we may find the combination of aircraft, supply, and maintenance which costs the least.

The curve in Figure 1 suggests combinations of spare parts supply and maintenance with which the program can be achieved with a given number of available aircraft, A_1. (Since for one reason or another some aircraft will be out of commission for lack of support, A_1 will be larger than the number of operationally ready aircraft called for in the program). The logic of the isoquant is that supply and maintenance may be substituted for each other, for example, by reducing repair facilities and supplying whole assemblies rather than bits and pieces of spare parts.

The "budget line" shows the possible combinations of supply and maintenance which a budget, L_1, can buy. At point d, the rate at which we can substitute supply for maintenance and still support the program is equal to the rate at which we can make the substitution and still keep support costs constant. This offers the suboptimal allocation in the case of A_1 available aircraft; the program can best be supported by incurring M_1 cost of maintenance and S_1 cost of supply. In Figure 2 the suboptimization is extended to situations where fewer aircraft and smaller budgets are available.
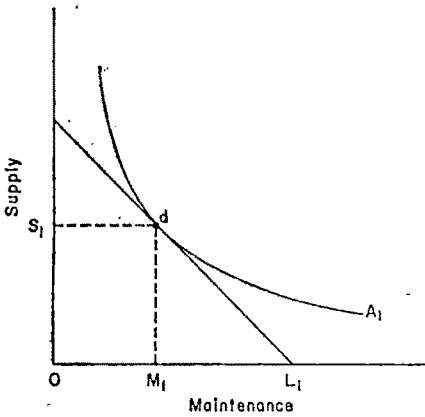
FIGURE 1
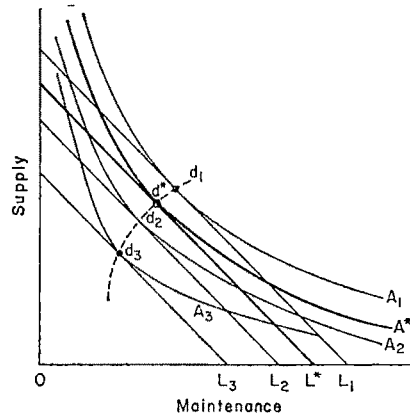
FIGURE 2
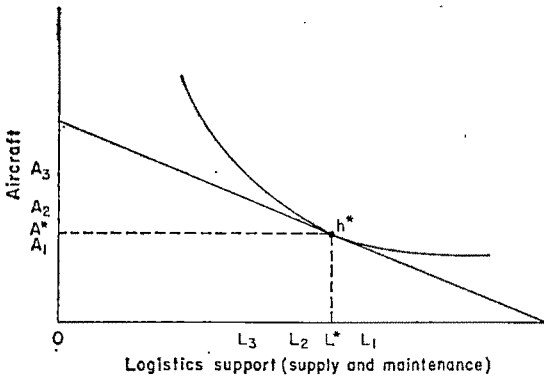
FIGURE 3

Figure 3 shows the allowable trade-offs between aircraft and the sum of supply and maintenance costs. For each number of aircraft the isoquant curve shows the minimum cost of logistics support which must be incurred if the program is to be achieved. The optimal combination of aircraft and logistics support depends on the relative costs of aircraft and logistics support. If aircraft cost, say, $2.5 million each, the "budget line" has a slope of minus 2/5; for, in order to keep the total budget constant, logistics support costs must be reduced by $2.5 million whenever the number of aircraft is increased by one. The optimal combination of inputs is found at point h^* where the rate at which we would be willing to substitute one input for another is just equal the rate at which we are able to do so.

The minimum budget necessary to support the program appears now

as L^*, plus \$2.5 million times A^*, that is to say, the cost of optimal logistics support, plus the cost of one aircraft times the optimal number of aircraft; and the optimum allocation of support funds between supply and maintenance appears in Figure 2 as the coordinates of point d^*.

So far, we have treated all the relations in this three-way trade-off problem as if they were certain; but in reality all of them are subject to uncertainty: the exact configuration of the aircraft, or of successive batches of the "same" aircraft that become available; the way they are used; the demand for spare parts that will appear in time; the demands on the maintenance establishment; its ability to do the required work in time; the real costs of making supply and maintenance support available; and thus the production surface itself, let alone the budget available for the aircraft and supporting activities. At the time when the problem calls for a solution, i.e., when procurement and support plans have to be laid, or when aircraft and logistics resources have to be allotted to squadrons, all of these factors are more or less uncertain.

Some uncertainties lend themselves to a probabilistic treatment. They relate to observable variables, the probability distribution of which can be estimated. The parameters of such a distribution will offer more information about the uncertain event than a sample average that may be known, and this information may lead to better management of the variables.

This kind of opportunity exists, for instance, in the field of demand for aircraft spare parts. Demand for such parts often occurs over time in a fashion well described by the Poisson probability distribution, or the negative polynomial distribution [Brown, 3]. Other opportunities may be found in the maintenance and transportation fields where the queuing of items awaiting repair or shipment and other factors lead to random fluctuations in repair and transport times respectively. An understanding of these random variations can pay off for the Air Force, just as insurance can for the man who knows that there is a risk of fire. Next to the basic economic calculus, probability thinking is most important in the analysis of logistics problems.

This is not to say that it can always be applied in full-blown quantitative fashion. Considering the range of uncertainties that beset the logistics business, only a small segment is accessible to precise probability calculations. Over what span of time a certain weapon will develop to its final configuration (if there is such a thing as a final configuration in this rapidly changing field); in what manner it will be used (intensity of flying practice, kind of mission, alerts, war, what kind of war); how the logistics support will be organized (under what

commands, by what techniques, in the Air Force or through private contractors)—all of these questions present alternative outcomes that defy precise probability calculation. This does not mean, of course, that the consequences of some of these uncertain events cannot be elucidated by study.

Indeed, the benefits that can be derived from economic analysis are often greatest where the outcomes are least calculable, that is to say, in planning for the future. Consider the basic economic principle that no output can be judged to be right in ignorance of the costs of its production. In military planning, it happens frequently that certain "outputs" are postulated as "needed" without regard to cost. The unknowns surrounding an entirely new weapon, such as a large ballistic missile, may be very great. Yet certain operational desiderata (e.g., readiness time, or rate of fire) may be laid down without regard to their implications in personnel, materiel or development time. Resource availabilities may be specified as well, but without regard to the limitations these resource quantities impose on operational capabilities in time. A fairly simple analysis of the technical and organizational features of missile operations, including the play of random factors, may develop some trade-offs between operational capabilities and resources and thus lead to a rational choice between attainable combinations.

III. Specific Logistics Problems

Looking for more efficient uses of resources is, of course, no monopoly of economists. Engineers, military officers, and other specialists are doing that too. But economists are accustomed to pursuing this search over a broader area, beyond the confines of a special technical field, limited only by the abstract concepts of input and output; this makes the economic approach useful in this field. Many of the trade-offs arise between separate functional areas. Each of these may be managed efficiently enough, and yet in combination they may not be. Because of the concern of all technicians with efficiency *within* their limited fields, and the tendency for intrafunctional efficiency to be mistaken for efficiency of the whole, there is a real need for an approach that cuts across the functions and looks for trade-offs between them. They all cost money. Some examples of such trade-offs will now be given.

A. Phasing-in a Weapon System

Modern weapons are complex and their useful lifetime is short. The Air Force is constantly adopting new engines, airframe features, fire control systems, and other details on existing "weapon systems," and

it is constantly developing and phasing-in entire new weapon systems.[2] The adoption of new details leads to "technical orders" that instruct the using organizations to change certain features on existing airplanes or other equipment, to remove certain pieces and to install new ones. The phasing-in of an entire new weapon system leads to conversion operations under which the using organizations, unit by unit, convert from an old weapon to the new. These change-overs pose considerable logistics problems, and their handling has a far-reaching influence on the effectiveness and the cost of the Air Force.

1. *Conversion.* The principal problem in conversion is to have all the adaptations in the system come off on time so that the tactical units receiving the new weapon can operate and service it, and simultaneously abandon the old weapon. Some resources may be transferable from the old system to the new, some parts, ground-handling equipment, hangars, crews, etc. Other resources—and they represent an ever-increasing proportion because of the growing intricacy of the weapons—are specific. A new assortment of parts, new engine starters, ramps, and so forth, are needed to handle the new aircraft. Maintenance crews have to be retrained, skills reproportioned, and all this needs to be done in time, so that the conversion causes the least possible loss in military effectiveness.

What are some of the conceivable trade-offs in this scheduling problem? The first trade-off may be between the number of new weapons and the size of maintenance support, supply support, or both. For instance, early in the phase-in it may be advantageous to give the bases more maintenance specialists and fewer aircraft than usual, because maintenance of the new equipment is the dominant problem. Similarly, it may be advantageous to give a new missile site greater stocks of propellants to allow for extra launch attempts on equipment that is still fairly unreliable, rather than giving the base more missiles to handle. A second trade-off may be between one item of logistics support and another, say, between the keeping of stocks of parts at bases and at depots, or between large parts inventories in the whole system and more efficient communication and transportation between bases, depots and contractors. A third may be between Air Force and contractor maintenance at the bases. The recognition of these logistics trade-offs may increase the effectiveness of the new weapons, and thus offset earlier delays in the development of the weapons.

2. *Telescoping of Development and Operations.* The development of

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[2] A weapon system is the combination of air vehicle, armament, ground support, etc., that is to perform a mission. The "phase-in" is the process in time by which the system becomes an operating part of the Air Force, the activation of units, delivery of equipment to them, etc.

a weapon and its introduction into the tactical units of the Air Force tend to overlap more and more. On the one hand, the novel and complex weapons have many "bugs" that it takes time to eliminate. On the other hand, the Air Force is anxious to profit early from the fruits of development and to equip its units with weapons that can match those of an enemy. The long development time of the "final" weapon and the desire to make the weapon operational as soon as possible tend to telescope development and operational use into each other. The gains from this telescoping are obvious, but so are the costs: combat weapons are tied up in elaborate modifications; aircraft and missiles, engines and parts are ordered into production that may be obsolete before they reach the operating units; contractors' plants are tooled up for models that will not be produced, and so forth.

The trade-offs are beset with particularly high uncertainties. Wishing to buy time, the Air Force may turn to regular production of an early untried design, and when "bugs" appear, saddle itself with the expense for repeated tool-ups of the producer and costly modification programs —and long out-of-commission times—for early delivered aircraft. Prolonged experimentation with a few prototypes, on the other hand, may save time by reducing the unknowns, reveal the potentialities of various designs and combinations, and reduce the costs of retooling and later modifications. The merits of the two approaches cannot be fully determined here [Klein, 8]. But certainly the second course puts a far more manageable burden on the logistics system than the first.

B. Supply Support

Uncertainties of a more homely sort pose interesting problems in supply support. The Air Force must predict its demand for fuel, engines, spare parts, etc., so as to provide the right material at the right place and at the right time. On the basis of these predictions it must buy and distribute material to depots and bases. The time lags of budgeting, procurement, paper work and shipping determine the time scale of the predictions.

For some things, demand is more predictable than for others. If a certain flying program is fixed—and adhered to—the demand for fuel is relatively predictable, and so is the demand for engines and components that have to be replaced according to "time change" rules. Even for aircraft spares, demand can be predicted with a variable degree of accuracy if certain precautions are taken, e.g., if system-wide rather than particular base demands, demands for substitutable items or otherwise grouped "families of parts" are considered, and provided, of course, that some relevant demand experience is available [Goldman,

6]. Failure rates may be calculated on an actuarial basis [Arnold *et al.*, 1]. But for a large number of aircraft spares, demand uncertainty remains very high, especially at base level, and among these can be found expensive "insurance type" items as well as a host of cheap bits and pieces.[3] In any event, of course, prediction rests on experience, well-kept records and sensible analysis.[4]

The real costs of bad demand predictions are shortages that condemn weapons to unreadiness, or excessive and obsolescent stocks, or excessive reliance on crash orders and the costly expediting of production or shipping. The actual out-of-pocket costs probably eat up several per cent of the Air Force budget.

There exist at least two promising ways for reducing the excessive costs of supply support. One consists of improvements of the way in which parts, particularly the more expensive components (with usually rather long production lead times), come into the Air Force system; the other of a more rational system of meeting supply and stockage requirements of parts, particularly of the cheaper variety. The first approach attacks the uncertainty problem from the flank, the second frontally.

Before saying more about these approaches, let us note that the Air Force made an important step in the direction of supply economy when it undertook in 1952-53 to sort out its equipment and the myriads of spare parts into cost categories. Holding costs, obsolescence risk, etc., obviously pose very different problems for nuts and bolts on the one hand, and for cockpit canopies, whole wing assemblies or landing gear units, on the other. Accounting should be more careful, procurement and distribution more circumstantial for expensive than for cheap items. By forming three cost categories of items, and setting high-value items aside for special management, the Air Force started on the right way.[5] Along this way, it may now combine item cost with demand char-

[3] Some people expect that demand for missile spares will on the whole be more predictable than demand for aircraft spares, because parts replacement on the missiles may be governed to a greater extent by time-change rules, and operating conditions may be more uniform than for manned aircraft. This remains to be proven.

[4] The statistical data collected in the Air Force offer much room for improvement. Even where the prospects for prediction are fairly good, the basis is often weak because records are unintelligently organized, or prematurely destroyed, sampling methods are faulty or not used, and so forth.

[5] The "Hi-Valu" program of the Air Force, launched in the fall of 1952, provided for special procurement and inventory procedures for items of more than $10 unit cost which, following an item-ranking by expenditure (quantity times unit cost), constitute about one-half of the value of a particular procurement contract.

Since 1955, such items have come to be known as "Category I (Hi-Valu)." They make up about 2 per cent of the total number of airborne spares, and 64 per cent of their total procurement value [12, pp. 40-41]. All other items costing more than $10 apiece are called Category II items. Category III items cost less than $10 apiece.

acteristics and other variables and thus make the treatment given to
different types of items in supply more discriminating from an eco-
nomic point of view.

1. *Deferred Procurement.* The flanking attack on demand uncer-
tainty accepts the fact that demand is virtually unpredictable for the
early part of a weapon system's phase-in. But it rejects the necessity
for the Air Force to rely on the unavoidably inaccurate early estimates
and to buy quantities of expensive spare parts accordingly, several
years in advance.[6] It seeks to meet uncertainty with flexibility.

The rationale of this approach is that buffer stocks of spares are
now employed at contractors' plants to smooth the production process,
and that these stocks, if somewhat enlarged and properly managed,
could cover replacement demands arising from the first few test and
operational units of a new weapon. Reparable "carcasses" of parts,
taken off the whole weapon, could be returned to factory or depot for
repair and then be set aside to form the first Air-Force-owned stock of
the parts. As this process rolls on, the Air Force would accumulate a
small stock of its own, and gain the demand experience needed to make
a reasonable estimate for major procurement. The major procurement
would be postponed until well into the phase-in of the operational air-
craft or missile.

This approach involves, of course, certain new costs, but these costs
seem to be much smaller than the savings on inventories and obsoles-
cence, at least for the relatively expensive articles [Petersen, 11].
Net savings may range from 30 to 60 per cent of the total cost of
these items under the present supply system and amount to hundreds
of millions of dollars. Air Force and aircraft manufacturers have
shown much interest in the approach, and some new weapons are
going to be supported in this fashion.

2. *Improved Requirements and Distribution Calculations.* While
logistics studies show that the Air Force tends to err by investing too
much money, and too soon, in expensive spares, they indicate that it
tends to be too parsimonious in stocking the bases with cheap parts
[Karr, 7]. It costs relatively little to lean over backward in stocking
low-cost items in depth if the supply process is managed properly. On
the other hand, the keeping of ample stocks can avoid numerous in-
stances of aircraft being put out of commission for lack of bits and
pieces. The uncertainty of demand for cheap parts can be met most

[6] A typical weapon phase-in stretches over 2 to 4 years. Production lead times on major
components range usually from 1 to 3 years. Under current procedures, 2 to 3 years may
easily pass from the first major spares contract to the appearance of the first sizable batch
of demand data from operational aircraft. In the absence of reliable data, Air Force pro-
curement officers tend to reduce the risk of parts shortages for new weapons by interpret-
ing the engineering estimates of support requirements rather liberally.

economically head-on, i.e., by ample insurance in the form of stocks.

The supply method for the cheaper parts can be improved by balancing holding costs against reorder costs in computing economical order quantities, and by balancing savings in shortage costs against costs of adding units to stock or pipeline in computing economical reorder levels. Several important variables enter into the estimation of these costs: the costs of storing, interest and obsolescence enter into holding costs; the costs of priority measures (and other factors) into shortage costs. The theory of inventory control provides the key to these improvements [2, 4]. The results, for practical application in Air Force logistics, may take the form of tabulations of economical order quantities and reorder levels for certain demand rates, unit prices, pipeline times and shortage costs; or corresponding "programs" for electronic computers [Ferguson and Fisher 5; 13]. At the cost of somewhat larger base stocks of low-cost items, this approach promises to reduce considerably the shortage costs of the supply system. It applies to the calculation of spares requirements for the Air Force as a whole, and to the allocation of spares to bases and depots.

3. *Centralized, Automatic Control.* Controlling the Air Force supply process is quite a formidable task. Many decisions are being made day-in and day-out to order, ship, process and reroute materiel and to stock and release items at the many supply installations. A vast flow of paper accompanies the flow of materiel and the production of the statistical data needed for decision making. This information and decision process poses interesting technical and economic problems. It is cumbersome and expensive despite improvements that have been brought about through organizational devices and through the introduction of business machines. The "paper mill" is still slow, information fragmented, and clerical errors abound.

Electronic computers with large "memories" are capable of controlling inventories far more effectively. They can make many of the routine supply decisions simultaneously and produce many of the data. Data processing centers may obviate the labor of many clerks on Air Force installations, turn out automatic shipping orders and other messages, and assemble statistics that afford a virtually complete and instantaneous view of the inventory and flow of materiel. Combined with high-speed communication, these devices can lead to a revolution of the logistics process.

The technology of such a control system is now in view, and parts of it have been put to practical tests [Nelson and Tupac, 10]. But important choices remain to be made in which economic analysis will have some part. Considering the organizational changes and large investments in equipment that are involved, how can the benefits of the sys-

tem be measured in terms of effectiveness and cost savings? How far should centralization and automatization be pushed? Undoubtedly, the technological possibilities of the system can be exploited to introduce new economic considerations into military supply management. Up to now, some of these considerations could be applied only in very rough form (e.g., item cost, through the setting up of a few cost categories), and others not at all (e.g., shortage costs and holding costs).

C. Investment in Airlift

Lest the reader feel that economy in logistics is only a matter of spare-parts management, the last example will deal with a very different subject. This is a problem of a rational investment program over time. The aim of the investment is the continual availability of a sufficient capacity for long-range military airlift at minimum cost.

Like all weapons, military airlift is subject to the forces of changing requirements, technological change and obsolescence of existing equipment. Foreseeable demands on airlift vary with changing requirements for peacetime transport capacity by the Air Force itself, and for wartime transport by Air Force and Army. Turboprop and turbojet-powered aircraft of greater range and speed have become available for procurement. The existing Military Air Transport Service (MATS) fleet, while purchased and paid for, continues to exact a price in terms of upkeep and especially manpower, and it may be too small.

But perhaps more than other weapons, airlift offers a refuge to non-economic thinking. Calculations of military economy seem to be less necessary for airlift because transport aircraft, unlike bombers and fighters, do not enter into direct competition with enemy forces; and calculations of a business nature (ton or passenger miles versus costs) do not seem to apply because the Military Air Transport Service is not an airline. Its main purpose is training for emergency use. This leads to some danger that the Air Force will have to get along with relatively costly and increasingly ineffectual types of equipment, because there is no accepted rationale for investment in airlift.

To escape this impasse, a method is needed to compare the relevant costs of alternative air transport fleets over time and to select from various attainable sequences of fleets the one that promises to meet postulated requirements at lowest cost. Such a method has been developed and applied along the following lines [McGuire, 9].

Alternative air transport fleets were considered, consisting of numerous combinations of designed and tested aircraft types. The productivities of each aircraft type were measured in terms of passenger and cargo-carrying capacity, speed, range, runway and loading characteristics; and comparative rates of substitution were established among the

competing types. Alternative fleets were then formed by selecting possible combinations of numbers of existing, phasing-in and phasing-out aircraft, over a specified period ahead, say, 10 years, subject to the current availability of the aircraft in Air Force inventories and to the production facilities, existing or buildable, for additional aircraft over that period.

All of the selected fleets had to meet the output requirements of a foreseeable maximum wartime transport load, *e.g.*, the requirements attending the deployment of a mixed task force for limited war to a foreign theatre of operations, in addition to meeting the normal and continuing resupply requirements of the U.S. Air Force. They had to be suitable to meet these requirements in terms of a specific route structure and desirable service frequencies.

On the cost side, all new resource outlays to create and operate these fleets in peacetime were considered. No sunk costs were allowed, and no wartime operating costs taken into account. The relevant costs, in other words, consisted of new outlays to produce that state of peacetime readiness required for emergency use. The various selected sequences of air transport fleets were then compared with respect to their cost in new money and in manpower. Manpower requirements were considered separately, because the Air Force usually operates not only under monetary budget ceilings but also under manpower ceilings. One of the fleets involved a minimum of new procurement and others, procurement of various sizes and kinds.

The study showed the marked diseconomies in money and manpower that would flow from following a "least-procurement" policy. It also showed that an economical fleet would employ a large number of newly procured transport aircraft.

IV. *Laboratory Simulation*

Logistics research applies economic and statistical thinking to the problems of a peculiar industry. In many ways, it resembles economic research in other areas. But some special features of the "industry" have led to the development of a new technique as a part of research: laboratory simulation of the management process. It consists of a reproduction on a reduced scale of the Air Force milieu and the activities which are the object of inquiry, and of the testing of alternative policies and procedures within this framework.

The first logistics simulation laboratory, which was set up at the RAND Corporation in Santa Monica in 1956-57, has several aims. It is an extension of research in the direction of designing logistics management procedures, testing their feasibility and comparing their effectiveness and cost with other procedures. It makes it possible to bring

under study the interactions of Air Force commands, command eche-
lons and functions which cannot be covered by any formula. It is also
an educational device for theoreticians and practitioners in logistics.

In its first application, the laboratory was used to design a logistics
system for a hypothetical Air Defense fighter aircraft. This system
incorporated the results of three studies mentioned above, *i.e.*, deferred
procurement of high-value spares, economical procurement and distri-
bution of other parts, and centralized electronic data processing. Par-
allel to this system, a laboratory replica of current Air Force logistics
was created to serve the same task. Both systems went through several
"years" of operation, from phase-in to phase-out of the fighter, on a
number of "bases" and under a realistic command structure.[7] In the
course of this run, significant differences between the two systems were
found; the new system was at least as effective as the old, and at much
lower cost. In the future, the laboratory may be used to study other
facets of Air Force logistics and test other ideas.

This is not the place to go into the details of the experiment, the
problems of design, laboratory management, the use of electronic com-
puters and of personnel drawn from various academic disciplines and
from the Air Force itself. Nor has the time come when the contribution
of laboratory simulation to the understanding and management of
logistics can be fully evaluated. The hope is, of course, that it will help
avoid some expensive and disruptive service-testing in the Air Force
and advance the starting point of those service tests that need to be
made. It has become quite clear that the laboratory is effective in stim-
ulating and rounding off logistics studies and in educating logistics per-
sonnel in the economic problems of their field.

The simulation technique seems to answer an old need of economics
and other social sciences for a laboratory in which the object under
study can be manipulated and observed more closely. But the applica-
bility of the technique in logistics rests on a special feature of the sub-
ject. The activity in which economy is being sought is a managerial
process and forms a virtually closed circuit. A logistics laboratory does
not have to allow for a welter of independent decision-units (producers,
consumers, organizations) following their own purposes with regard to
an infinite number of goods and services. Instead, it encompasses repre-
sentatives of certain functions in a hierarchical organization that exe-
cute a single plan. The goods and services involved are limited in num-
ber and kind. To be sure, the "single plan" is usually a whole pattern

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[7] Time compression, which reduced a day of actual operations to 30-40 minutes of
laboratory time, made it possible to act out several years of operations in a few months.
Bases, commands, depots, contractor's factory, etc., were abstracted and modeled so as to
fit the capacities of a few dozen people in the laboratory.

of plans pursued by different commands, with gaps and contradictions that call for judgment and decisions at various levels—the military hierarchy leaves many important decisions to lower echelons—and the execution is no mechanical matter. Still, compared to the national economy, the logistics system is a monolith. It has, however, much in common with the internal activities of many a large corporation. The Air Force's pioneering in the laboratory simulation of a large-scale management process may indeed lead to similar experiments in the management of corporations.

V. *Conclusion*

It is not surprising to find that a developing contact between two disciplines leads to two-way traffic. The infusion of economic concepts and methodology into military logistics promises to help in the handling of complex problems and the development of a more rational use of human and material resources. The receptiveness of military men to economic thinking has increased considerably over the years. This is true for all of the military services. Many of their problems turn out to be quite similar once they are lifted out of their peculiar institutional context and looked at as problems of resource allocation. To economists, on the other hand, this field offers not only a new area of application but also opportunities for the development of techniques that may prove to be valuable elsewhere.

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COMMUNICATIONS

Response of Consumer Loans to General Credit Conditions

Vigorous growth of consumer credit throughout recent periods of general credit stringency has led to the widespread belief that this type of credit is not responsive to general credit conditions. Outstanding instalment credit increased $5 billion or by nearly a third from mid-1952 to mid-1953 when credit conditions were tightening. Again, instalment credit expanded by nearly a third from mid-1955 to the fall of 1957 when credit conditions were also tightening.

Consumer credit's reputation for insensitivity to general credit conditions has been widely accepted by both economists and bankers.[1] Advocates of selective credit controls eagerly accept the insensitivity of consumer credit as an argument to support their positions. Many opponents of selective control also have accepted the insensitivity of consumer credit and have argued against selective controls on other grounds.

This reputation for insensitivity, however, may be based erroneously upon the unusual strength of the demand for consumer credit rather than upon any unique lack of responsiveness to general credit conditions. Bullets fired into a wooden block at different velocities penetrate to different depths. Similarly, different types of credit under different demand pressures can be expected to penetrate the restrictions of general credit conditions by different degrees. The mere expansion of consumer credit during periods of credit restraint is not proof of its insensitivity to general credit conditions.

The impact of credit policies falls unevenly on individual commercial banks. When credit policies are designed to hold total reserves at a constant level or to limit the increase in reserves to a gradual rate of growth, some banks lose reserves while others gain reserves. The banks that lose reserves must limit their loan activities or liquidate part of their portfolios while the others are relatively free to expand their loans and investments. Individual banks in a given locality face similar local markets and similar general demand conditions but may have different deposit experience.[2] A comparison of the loan and investment policies of banks that have lost reserves with those of banks of a similar type and size that have gained reserves should give some insight into the adjustments that individual banks make under tightening credit conditions.

The study described below (1) examines lending activities of commercial banks under different degrees of credit stringency, as indicated by deposit experience, but under similar conditions of demand, and (2) compares the re-

[1] Warren L. Smith in a review of the comprehensive study of consumer credit undertaken by the Federal Reserve System concludes that ". . . most of the participants seem to feel that consumer credit . . . is rather insensitive to general instruments of monetary policy." "Consumer Instalment Credit," *Am. Econ. Rev.*, Dec. 1957, *47*, 983.

[2] Comparison of the geographic location of the banks studied indicated that the differences in deposit experience were not related to differences in location.

sponsiveness of consumer lending to that of other types of loans and invest-ments under these conditions.

Two samples of banks were selected for study. One sample included 28 of the 32 largest consumer credit lenders.[3] All of these were among the 100 largest banks in the country when classified by deposit size. In many cases however, their consumer loans were a relatively small part of their total loans and investments. The second sample consisted of 35 medium-sized banks (i.e., with deposits of $10 to $50 million) that specialized in consumer loans. All of these banks had more than one-third of their total loans in loans to consumers.

Each of these samples was divided into two parts according to the deposit experience from mid-1955 to the end of 1956. Banks that lost deposits or showed a smaller than average gain (as indicated by statistics for banks in leading cities) were included in one group, and the remaining banks that showed larger than average deposit gains were combined in the other.

Throughout the period from mid-1955 to the end of 1956 strong credit demands from all sectors pressed against limited supplies in credit markets. Demands for consumer credit were particularly strong in the last half of 1955 when the already large credit demand for purchases of new cars was reinforced by a competitive easing of instalment-credit terms. Down payments were re-duced and standard maturities on new cars were frequently extended from 24 to 36 months. From mid-1955 to the end of the year outstanding auto-mobile credit increased nearly $2 billion or 15 per cent and total instalment credit increased $3 billion. In 1956, consumers' demands for funds continued, but at a reduced rate, while business demands were especially heavy.

The Federal Reserve System maintained a general condition of restraint on credit throughout the last half of 1955 and all of 1956.[4] As a result total reserves of member banks changed relatively little during this period and banks were able to expand their loans only by more complete utilization of reserves and by selling securities. Except for a brief period early in 1956 member-bank borrowings were larger than their excess reserves. Interest rates rose steadily through the last half of 1955 and all of 1956.

The experience of some banks was much better than that of others during this period. Shifts in deposits provided many banks with reserves which per-mitted them to expand their loans in response to the heavy demands. Such gains, however, were at the expense of other banks as there was no over-all increase in reserves. Banks that lost deposits were forced to restrict their lending activities or to reduce their investments to obtain funds for the ex-pansion of their loans.

I. *Experience of Large Banks*

Thirteen of the 28 large consumer lenders (the first sample) either lost deposits from mid-1955 to the end of 1956 or showed smaller than average gains. The combined deposits of these 13 banks increased only slightly during

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[3] Data were not available for four of the 32 largest banks.

[4] *42nd Annual Report of the Board of Governors of the Federal Reserve System*, pp. 5-6 and *43rd Annual Report*, pp. 13-14.

CHART 1

CHANGES IN LOANS AND SECURITIES - LARGE BANKS

Indexes, June 1955=100
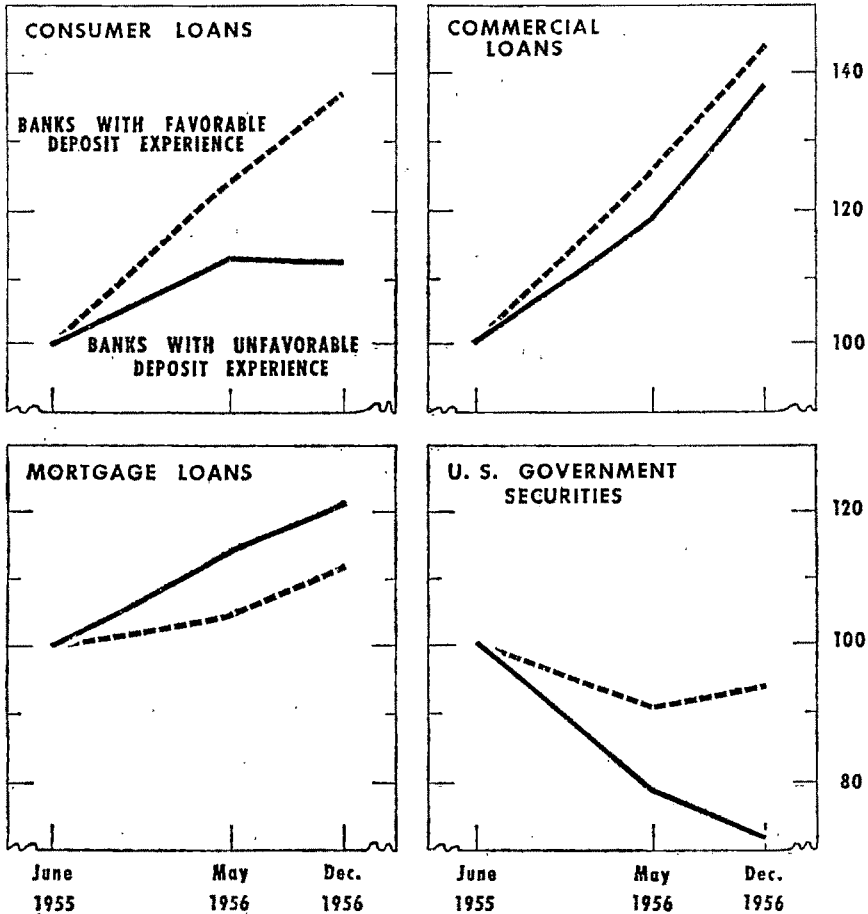
CONSUMER LOANS

BANKS WITH FAVORABLE
   DEPOSIT EXPERIENCE

BANKS WITH UNFAVORABLE
        DEPOSIT EXPERIENCE

COMMERCIAL
   LOANS

140

120

100

MORTGAGE LOANS

U. S. GOVERNMENT
     SECURITIES

120

100

80

| June | May | Dec. | June | May | Dec. |
| 1955 | 1956 | 1956 | 1955 | 1956 | 1956 |

Note: May was used as a dividing point for the entire period because reserve positions reached their tightest point (as indicated by the excess of borrowings over excess reserves) in that month.

the period while the deposits of the other large banks increased about 15 per cent.

The effects of the loss of deposits were apparent in nearly all of the major portfolio items of the 13 banks affected, as shown on Chart 1. Although they expanded their consumer loans from mid-1955 to the end of 1956, the increase was only one-third as large as that at banks with more favorable deposit experience. Both groups of banks increased their commercial loans but the expansion was greater at the banks with deposit gains. Both groups of banks reduced their holdings of U. S. Government securities but the decline in

the case of banks that lost deposits was more than four times that at the banks with more favorable deposit experience.

The only exception—among the types of assets studied—to this pattern of restrictive activity on the part of banks that lost deposits occurred in mort-- gage loans. They reported a larger increase in outstanding mortgage loans than banks with more favorable deposit experience. This may reflect the fact that several of the banks that reported the largest increases in mortgage loans were heavily involved in the warehousing of mortgages. They may have been obligated to expand their mortgage portfolios because of advance commitments.

TABLE 1.—CHANGES IN DEPOSITS AND PRINCIPAL ASSET ITEMS, SAMPLE OF LARGE
BANKS, JUNE 1955–DECEMBER 1956

(Indexes, June 1955 = 100)

Item	Index (Dec. 1956) for Banks with		Ratio: Index for Banks with Favorable to Index for Those with Unfavorable Experience
	Unfavorable Deposit Experience[a]	Favorable Deposit Experience[a]	
Total deposits	103.3	114.2	110.6
Commercial and industrial loans	137.7	144.0	104.6
Mortgage loans	121.2	111.7	92.2
Consumer loans[b]	112.0	137.1	122.4
U. S. Government securities	71.8	93.7	130.5
Other securities	79.4	100.9	127.1
Number of banks	13	15	—

[a] Banks that lost deposits or showed a smaller gain than the average for banks in leading cities were included in the group with "unfavorable deposit experience." Those that showed a larger gain were included in the group with "favorable deposit experience."

[b] Data for "other" loans as classified in the weekly reporting member bank statement. Comprised predominantly of consumer loans.

Nearly all of the banks with unfavorable deposit experience restricted their consumer loan activity. Only one of them reported a larger increase in consumer loans than the average for banks with better deposit experience. On the other hand, nearly all of the banks that gained deposits expanded their consumer loans more than the average expansion indicated by the banks that lost deposits.

A comparison of the changes in the principal items of loans and investment at banks with favorable deposit experience with those at banks with unfavorable experience gives a rough indication of the extent to which various types of loans and investments were restricted (see ratios in last column of Table 1). Consumer loans were more severely restricted than any other type of loan as indicated by the relatively high ratio of the index of banks with favorable ex-

CHART 2

CHANGES IN LOANS AND SECURITIES - MEDIUM-SIZED BANKS

Indexes, 1955=100
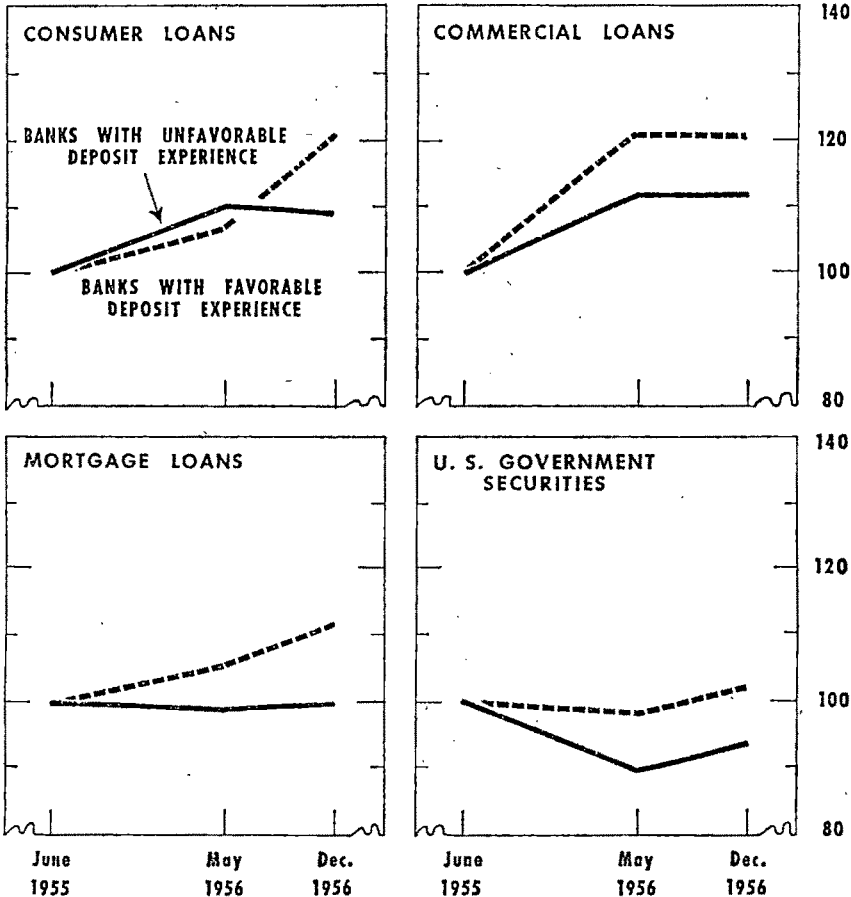
Note: May was used as a dividing point for the entire period because reserve positions reached their tightest point (as indicated by the excess of borrowings over excess reserves) in that month.

perience to that of banks with unfavorable experience. Only investments in securities were more adversely affected.

II. Experience of Medium-sized Banks

Sixteen of the sample of 35 medium-sized consumer lenders (the second sample) lost deposits or showed smaller than average gains. The combined total for the deposits of these 16 banks showed practically no change from mid-1955 to the end of 1956. This compared with an increase of about 15 per cent for the other 19 banks.

The restrictive effects of the loss of deposits at the 16 banks was reflected in every major type of loan and investment, as shown in Chart 2. Their consumer loans increased only about half as much as similar loans at the more fortunate banks. Expansion of commercial loans was smaller at these banks than at the other banks and they reduced their holdings of securities while the other banks expanded theirs slightly.

Nearly all of the banks that lost deposits restricted their consumer-lending activities. Only 2 of the 16 banks with unfavorable deposit experience reported

TABLE 2.—CHANGES IN DEPOSITS AND PRINCIPAL ASSET ITEMS, SAMPLE OF MEDIUM-
SIZED BANKS, JUNE 1955–DECEMBER 1956

(Indexes, June 1955=100)

Item	Index (Dec. 1956) for Banks with		Ratio: Index for Banks with Favorable to Index for Those with Unfavorable Experience
	Unfavorable Deposit Experience[a]	Favorable Deposit Experience[a]	
Total deposits	100.2	113.4	113.2
Commercial and industrial loans	111.6	120.4	107.9
Mortgage loans	99.8	111.4	111.6
Consumer loans[b]	108.8	120.8	111.0
U. S. Government securities	93.4	102.0	109.2
Other securities	95.6	100.0	104.6
Number of banks	16	19	—

[a] Banks that lost deposits or showed a smaller gain than the average for banks in leading cities were included in the group with "unfavorable deposit experience." Those that showed a larger gain were included in the group with "favorable deposit experience."

[b] Data for "other" loans as classified in the weekly reporting member bank statement. Comprised predominantly of consumer loans.

a larger expansion in consumer loans than the average for the 19 banks that gained deposits. On the other hand, nearly all of the banks with favorable deposit experience expanded their consumer loans more than the average for banks that lost deposits.

As was the case with the larger banks, the medium-sized banks with unfavorable deposit experience restricted their consumer loans more severely than most other types of loans and investments (see ratios in last column of Table 2). There were some indications, however, that adjustments were not made as quickly in consumer loans as in other items. Banks with unfavorable deposit experience actually expanded their loans more rapidly than the other banks during the early part of the period studied. This suggests that there may be a time lag before adjustments are made in consumer-credit lending policies at banks that specialize in these loans.

III. *Concluding Observations*

This study suggests that consumer credit is not uniquely insensitive to general credit conditions. It shows that banks under pressure to restrict credit limited their loans to consumers as well as their other loans and investments. It also shows that banks were responsive to market pressures and that strong market demands tended to be served. It seems likely that the reputation of consumer credit for insensitivity to general credit conditions is erroneously based on unusual demand forces associated with its growth. If, as many think, consumer credit has reached economic maturity, future demand forces for this type of credit may be less unusual and it may be more responsive to general credit conditions.

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The Federal Personal Income Tax and the Incidence of Deductible Costs

Discussion of the impact of state and local taxes frequently fails to take account of the degree to which these taxes are passed on to the federal government in the form of increased deductions from taxable income under the federal income tax.[1] Even where such effects are noted, only the marginal rates of federal personal income tax are taken into account.

In practice, however, the federal personal income tax has an additional provision of major importance in any analysis of incidence. This provision is the one allowing a standard deduction of 10 per cent of adjusted gross income up to a maximum of $1000 in place of the itemized deductions. For anyone using the standard deduction, changes in such things as state and local taxes, interest payments, or medical bills, cannot be passed on to the federal government unless they are so large as to induce him to itemize deductions.

The upper line on Chart 1 shows that, except for a sag around $10,000, there is a strong positive relationship between gross taxable income and the proportion of taxpayers in an income group who use itemized deductions. The lower line on the chart, based on the marginal tax rate for a family with two exemptions (married couple) shows an estimate of the proportion of any increase in deductible expenses of each group that would be passed on to the federal government if every family in that group had two people in it. For instance, the marginal tax rate around $12,500 is 23 per cent, but only 47 per cent of returns itemize deductions, so that slightly over 10 per cent (23 times 47) of an increase in sales taxes, for instance, paid by this group would be recouped in lower federal income taxes. At $125,000 adjusted gross income,

[1] For an excellent study that does take such effects into account, see *Report of the Governor's Minnesota Tax Study Committee*, Minneapolis 1956.

some 96.9 per cent itemize deductions and the marginal tax rate is 70, so that 68 per cent of an increase in state and local taxes paid by this group would be recouped in lower federal income taxes.

A more precise analysis would take account of the average number of exemptions per tax return in each income group, since it increases slightly as one goes up the income scale. However, an exemption amounts only to $600 and this refinement would not appreciably change the marginal tax rates

CHART 1. SHIFTABILITY OF DEDUCTIBLE EXPENSES AT DIFFERENT INCOME LEVELS
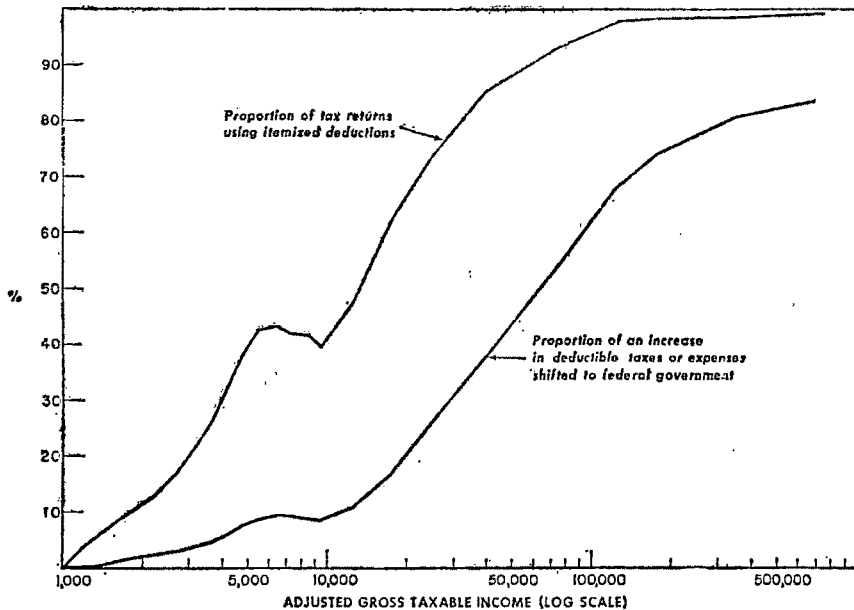
SOURCE:
U. S. Treasury Department, Internal Revenue Service, Individual Tax Returns For 1954, Washington, D. C.:
U. S. Gov't Printing Office, 1957 (Pub. No. 79). Table 1. p. 33 and Table 5, p. 48.
(Marginal tax rates from tax tables)

estimated here. One might also consider the extent to which changes in deductible expenses would change the proportion of people who itemize deductions; but if we are dealing with small marginal changes in a single item, this may well be an insignificant fraction.

. Any study of the incidence of changes in state or local taxes, of interest rates, or of medical costs paid by individuals must therefore take account of three different factors: the initial incidence of the original cost for different income groups, the degree to which each group can avail itself of itemized deductions, and the marginal rate of federal income taxation for that income group.[2] It is clear that any increase in costs that are deductible for federal

[2] When one comes to specific taxes such as the property tax, of course, the initial incidence problem is itself complicated. One must ask whether an increase in taxes on rented property is passed on to the renter, and for owner-occupied properties must take account

income tax purposes, is likely to be regressive unless the increase itself is extremely progressive in order to offset the effects shown in Chart 1. This applies to increases in state and local taxes, in mortgage and instalment interest rates, in noninsurable medical costs or in medical insurance rates. The reverse is also true: any decrease in any of these things is quite progressive in impact.

Any progressive tax with provision for deductions of other (usually state and local) taxes, is much less progressive than it looks unless these other deductible taxes or expenses are also highly progressive. Particularly "at the margin," in discussing changes in state and local taxes, it is important to keep this in mind.

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of the fact that as one goes up the income scale one finds an increasing proportion of people who own their homes (an increase from 30 to 90 per cent) and a declining ratio of house value to income. Also, the owners with mortgage interest and taxes to deduct are more likely to itemize deductions.

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Some Little-Understood Aspects of Korea's Monetary and Fiscal Systems: Comment

Colin D. Campbell and Gordon Tullock in a recent issue of this *Review* discuss the difficulty of giving economic advice to foreign governments, illustrating the problem from American experience in the Republic of Korea from 1945 to 1954 [4]. However the institutional and political structure of the Korean economy has changed markedly since 1954. This paper will consider briefly those changes which, as of August 1957, would modify some of the findings of Campbell and Tullock.

The institutional framework of aid goods' distribution has been drastically revised [10, pp. 3-17].[1] The Korean government is no longer responsible for the sale of aid goods, and normally it does not set their market prices. The Korean government, through its various offices and through the Bank of Korea, administers the allocation and sale of aid dollars under the supervision of the International Cooperation Administration.

Aid expenditures may be broadly classified as "nonspecified 'end-user" and "specified end-user." In the nonspecified end-user portion of the program, specific quantities of dollars are allocated to the procurement of specific raw materials and consumer goods. The dollars in each category are then sold to the importers who offer the highest local currency downpayment against the purchase cost of the dollars; in case of equal bids buyers are selected by lot. The commodities imported may be sold in the open market at the prevailing market price. Nonspecified end-user dollar sales accounted for about

[1] A summary of the regulations governing the allocation of nonspecified end-user funds through the Bank of Korea was drafted by De Alessi and checked by Adams and Elmendorf of the Office of the Controller, UNC/OEC for Korea. The only change relevant to this paper that they noted as of August 29, 1957, was an increase in compulsory National Bonds sales from Hẃ/20 to Hẃ/30 per dollar purchased. The dollar allocation mechanism has been discussed in more detail by Louis De Alessi [5].

$81 million in fiscal year 1956 and $122.5 in fiscal year 1957, or 25 per cent and 37 per cent of the respective total defense-support programs [8, pp. 49-54, 77-81].

Dollars in the specified end-user portion of the program are used to support specific rehabilitation or development projects, and are sold to recipients administratively selected by joint United States-Republic of Korea agreement. Such recipients may be either private or government units, and they must use the dollars to import the capital goods and raw materials specified in the allocation authorization. These goods normally must be utilized by the importing unit, and cannot be sold in the market.

Dollars in both categories are sold at a basic rate of 500 hwan to the dollar. The hwan thus collected are used by the Korean government to finance some of its expenditures. Since the private sector buys dollars at a basic rate of Hw500:1 and sells the commodities imported or manufactured with imported materials at whatever price it can get, windfall profits are possible. Selling dollars in the open market to the highest bidder would eliminate such profits as well as increase government revenue, and thus exert some anti-inflationary pressure (on the basis of past experience it may be assumed that the additional revenue would finance expenditures that otherwise would be met through increased bank credit [9, p. 10]). Revision of the allocation mechanism is under study, with the aim of eliminating such profit opportunities.

Government enterprises are no longer expected to subsidize consumers by selling their output at prices lower than cost. The Monetary Board recently recommended that government enterprises be permitted to set prices that cover costs of production, that such enterprises be placed on a *laissez-faire* footing and that the participation of private capital be aggressively encouraged [1]. These recommendations are representative of current economic thought in Korea. On January 1, 1957, rail passenger rates were increased by 85 per cent, telephone and telegraph charges by 180 per cent, coal prices by 54 per cent, tobacco and salt prices by 50 per cent; accounting profits were anticipated in these areas [6]. Although rail freight rates were increased by 160 per cent, postal charges by 100 per cent, electric power generation rates by 150 per cent, and electric power distribution rates by 50 per cent, such services were expected to show a deficit; further rates increases were under consideration [6].

Tax increases now seem to be politically feasible, with an average increase in tax rates of 13 per cent taking effect January 1, 1957 [6]. This tax rise is probably inadequate as an anti-inflationary measure, since in fiscal year 1956 estimated revenues from all taxes and stamp duties financed only 21 per cent of the expenditures budgeted; however it should help to alleviate the pressure.

In 1956 consumption expenditures absorbed about 80 per cent and government expenditures 7 per cent of a gross national product estimated at $1,833 million [7, pp. 34-35]. The same source estimated per capita consumption at $67.

Private enterprise now plays a more important role in the economy than it did prior to 1955. By June 1957 about 80 per cent of all Japanese prop-

erties appropriated by the government had been sold to the public. Direct government control is largely limited to coal (about 60 per cent of the domestic output), tungsten, rail and air transportation, shipbuilding; electric power, tobacco, and salt. Government influence, exerted through a network of direct and indirect subsidies, appears to favor the growth of private business.

The Korean government still floats the bulk of its bond issues by means of compulsory sales. Such behavior is explicable, since the market rate of interest on a moderate-risk loan of Hẃ1 million is 8 per cent per month while government bonds yield a return of 5 per cent per year. In August 1957 all importers had to buy National Bonds to equal 25 per cent of the custom duty paid (an average of Hẃ30 per dollar spent). Importers of aid goods (staples excepted) had to buy additional bonds at the rate of Hẃ30 per dollar purchased [10, p. 11]. At that time the resale value of newly issued bonds was about 20 per cent of their face value, thus increasing the aid dollar purchase rate from 500:1 to about 550:1. Current black market rates were 990:1 for United States currency and 1,150:1 for export dollars.

The money supply, computed on the basis of the July 1955 definition now applied by the Bank of Korea, increased by 61 per cent in 1955 and by 29.3 per cent in 1956; the index of Seoul wholesale prices increased by 42.8 per cent in 1955 and by 42.6 per cent in 1956.[2] Koreans continued to use the hwan as a medium of exchange, as a unit of account, and, to some extent, as a store of value.

It is difficult to believe that the hwan should have been discarded as a unit of account [4, p. 349]. The Seoul wholesale price index or the United States dollar, suggested by Campbell and Tullock, would not have been practical alternatives. The Seoul wholesale price index appears subject to erratic fluctuations [3, pp. 17-18],[3] and the "real" value of the hwan in dollars is difficult if not impossible to obtain at a given historical time, let alone consistently over time. Even if an adequate unit of account were selected and an adequate method of computing the rate of conversion were evolved, that rate would have to be adjusted at least weekly. The difficulty of having all government and business units adhere to this procedure would seem to exceed the advantages, if any, that might be derived from its adoption.

Campbell and Tullock assert that the Korean economy would have benefited from the import of an alternative store of value, such as foreign currency or precious metals [4, p. 349]. It is not clear that the social cost of hoarding dollars or gold is necessarily less than that of hoarding goods. The alternative propensities to hoard would seem to be one of several relevant factors calling for careful analysis.

The Korean standard of living has improved since 1953, and present conditions indicate that the current rate of economic development will be maintained in the future. However Korea still has serious economic problems to

[2] These percentages represent increases from the end of the previous year [2], and extend Table I in Campbell and Tullock [4, p. 340].

[3] Comments concerning weaknesses in the statistical reports of the Bank of Korea stem from an awareness of the difficulties involved in collecting the data, and do not reflect discredit upon officials and staff members of the BOK.

solve before it can achieve that rate of self-sustaining growth necessary to foster a substantial and continuing increase in its real per capita output of good and services.

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. ROBERT EVANS*</div>

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Some Little-Understood Aspects of Korea's Monetary and Fiscal Systems: Comment

Colin D. Campbell and Gordon Tullock's article, "Some Little-Understood Aspects of Korea's Monetary and Fiscal Systems," in the June 1957 issue of the *Review* is a welcome contribution because it draws attention to the peculiar problems of the war-torn economy of Korea. A few words of caution are nevertheless in order.

This relates to their discussion of "nonbudgetary revenues" of individual government agencies of Korea, a discussion which involves some, no doubt unintentional, exaggeration. According to the authors, the Korean government has maintained the prices of aid goods at a level materially lower than prevailing market prices for the dual purpose of enhancing government popularity with end-users, and of using that part of the proceeds of the aid commodities that represents the spread between the market and official prices

to meet expenditures of individual government agencies. The illustrations which the authors give however—specifically those of gasoline and fertilizer— are not cases that can substantiate the authors' assertions that the Korean government pursued the policies described by them.

For instance, all of the gasoline and other petroleum products required for the civil economy are procured by the United States Army and turned over for distribution to the Korea Oil Storage Company, jointly operated by Caltex, Socony Vacuum, and the Shell Company, which retails the products through its authorized dealers. Decision on the kinds and quantities to be procured, on their distribution, and on their pricing are made by the U. S. aid authorities and the Korean government, acting jointly though the Combined Economic Board.

Every phase of the fertilizer program, comprising procurement, inland transportation, and distribution, is also conducted under the joint supervision of the Korean government and the U. S. aid agency in Korea. And since there is a strict requirement of joint agreement between the Korean and United States government authorities on every important aspect of the fertilizer program, it simply is not true that the Korean government can or does act unilaterally in any material way to "produce revenue for individual bureaus."[1] Indeed the pricing of fertilizer is not only subject to this joint control, but to the approval of the Korean National Assembly as well.

There probably were some irregularities among some officials in times of product scarcity resulting from poorly coordinated programs. But they were undoubtedly much less prevalent than Campbell and Tullock suggested; and the situation in any event has greatly improved as government operations have increased in efficiency.

I am nevertheless anxious to add that, despite my rejection of certain portions of the article, there is no intention to deprecate the authors' approach to the problems of the Korean economy and its administration.

SEI-YOUNG PARK*

[1] I may be excused a brief personal reference. I myself never encountered, during my four years' tour of duty with the Korean government, 1950-1954, the alleged unlawful practices of securing funds for expenditures from sources outside the budget authorized by the national legislature.

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Reply

Although some changes in economic conditions in South Korea undoubtedly have occurred since 1954, we are impressed by the similarity of the comments made by De Alessi and Evans to those made by a number of economists prior to 1954. The relationship of the aid program to the budget, the development of free enterprise in South Korea, and the "windfall profits" related to the aid program were all described in much the same terms before 1954. Our belief is that Korean institutions are more complicated than such descriptions indicate and that they cannot be understood properly in terms of their counterparts in the United States.

The suggestion that Koreans discard their depreciating hwan as a unit of account is concerned with the important accounting problems created by a continuously depreciating currency. In South Korea accounting has been almost completely useless as a basis for making management decisions because the values of purchases and sales have varied with the date of the transactions. Our suggestion had in mind an educational effort to acquaint Koreans with the advantages of using alternative accounting units. We would be opposed to requiring all government agencies and business firms to adhere to any particular unit of account. Business firms should be free to adopt or reject alternative units of account in order that they change over to new accounting procedures only if such procedures appear advantageous. A problem of enforcement, which De Alessi and Evans believe is a weakness of our suggestion, would not arise. Conversion factors relating current market prices to a unit of account have been used successfully in countries with depreciating currencies [1] [2, pp. 92-93] [3]. There is no reason to believe that such arrangements are impractical.

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An Estimate of the Tax Element in Soviet Bonds: Comment

Professor Franklyn Holzman's discussion of the tax element in Soviet bonds [4] contains two important errors, one in mathematics and one concerning bond redemption schedules, which are responsible for an unsatisfactory appraisal of the 1957 Soviet bond freeze. The purposes of the present note are (1) to point out these errors and the weakness of the tax element approach in an analysis of Soviet bond policy and (2) to offer an alternative explanation of the Soviet bond freeze measures.[1]

I. *The Tax Element in Soviet Bonds*

Holzman's conclusion that the tax element in Soviet bonds, as he defines it,[2] declined sharply or was eliminated in the postwar period results from an

[1] This note is based in part on sources which would not have been available to Holzman during the preparation of his article early in 1957.

[2] The tax element in the compulsory sale of a bond is defined as follows [4, p. 393]:

$$T = V_j - \frac{V_m}{P_n/P_o}, \text{ when}$$

error in his calculations. He states [4, p. 395] that the tax element in an issue with a 2 per cent official rate and a 15-year effective term, under the assumption of consumer expectations of a 50 per cent decline in retail prices over the term of the loan, would be 5 per cent when the "market" rate is 10 per cent, and 39 per cent when the market rate is 15 per cent. Under these assumptions, the tax element works out to 36 and 67 per cent, not 5 and 39 per cent, respectively.

Moreover, if it is assumed that in the past few years Soviet consumers expected a continuation of the retail price stability which has, on the whole, prevailed since 1954 [11, p. 232], the tax element according to Holzman's formula would be still higher. If a 2 per cent official rate, a 15-year effective term, and no expectation of price changes are assumed, the tax element would be 68 per cent with a 10 per cent market rate, and 83 per cent with a 15 per cent market rate. Under either set of assumptions regarding price-change expectations, the decline in the tax element from the levels estimated for the 1930's by Holzman [4, p. 394] is not significant enough to warrant his conclusion that such a decline was responsible for the termination of "mass-subscription" bond issues.

In view of the extremely crude assumptions which must be made concerning consumer price expectations and the market rate of interest, as Holzman defines it, the meaningfulness of the figures introduced into his formula and the usefulness of the tax element approach in interpreting Soviet fiscal policy are questionable. For example, Holzman's estimate of a 15 or 20 per cent market rate in the 1930's is based on the doubtful assumption [4, p. 393] that in 1927 the Soviet population did not expect either further inflation or a possible future bond conversion. On the contrary, the experience of the Soviet population with severe and prolonged inflation and currency conversions during and after the first world war [2, *passim*] would have led it to anticipate or at least suspect further inflation and conversions. The prevalence of such expectations would suggest even higher market rates for the 1930's in Holzman's formula, although it does not appear very fruitful to speculate upon what these hypothetical market rates might be.

In his account of the features of Soviet bond issues [4, p. 391], Holzman overlooks one characteristic of these issues, and this oversight contributes to his incorrect interpretation of the 1957 bond freeze.[3] Holzman incorrectly states that holders of nonprize-winning bonds did not recover their principal until the 20-year period of the issue had elapsed and that retirements of such bonds were not scheduled to begin until 1968. In fact, in addition to the semiannual "prize" drawings to select prize-winning bonds, separate nonprize or "retirement" drawings were held annually to redeem at face value a portion

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T is the absolute amount of tax; V_f is the present value of the bond at its nominal or stated rate of interest; V_m is the present value of the bond at the market rate of interest (defined as the rate at which the Soviet government could just sell to the population on a voluntary basis the specified flotation of bonds) on the assumption that the purchaser expects no change in consumers' goods prices over the life of the bond; and P_n/P_o is the price index of consumers' goods which the bondholder, at date of purchase, expects to prevail in the year when the bond matures.

[3] This oversight occurs also in his earlier study of Soviet taxation [5, pp. 202-3].

of the nonprize-winning bonds.[4] Prize drawings on a given issue began in the following year, whereas retirement drawings did not begin until the fifth year of the life of an issue. Thus, on the 1947 issue, prize drawings began in 1948 and retirement drawings in 1952; on the 1948 issue, respectively in 1949 and 1953; etc. Furthermore, the size of the retirement drawings was not the same throughout the 5th-20th years of the issue's life; instead, it increased in the 11th-15th and 16th-20th years over the level of retirements scheduled for the preceding 5-year period [6] [7] [3, pp. 60-61]. The addition each year of both new prize drawings and new retirement drawings to the drawings already being held therefore caused a steady and substantial annual increase in debt service during the early 1950's [12, p. 25]. An even greater annual increase in service was to be expected during the late 1950's and 1960's, when retirement drawings on the early postwar issues were scheduled to rise as these issues reached first their 11th and then their 16th years.

II. The 1957 Bond Freeze

Holzman evaluates the 1957 bond freeze[5] as follows [4, p. 396]:

> The discontinuance of sales of "mass-subscription" bonds is easy to understand in light of the decline or elimination of the tax element in the bonds. The (what appears to be a) default is a drastic step and is hard to understand at the present time in the absence of overt crisis and in view of the fact that the service burden is slight and will continue to be slight until 1968 when the first of the bonds outstanding, aside from lottery winners' bonds, are due for retirement.

The discussion in the preceding section has shown the deficiencies of this appraisal. First, the tax element, as Holzman defines it, did not decline significantly but remained quite high. Second, retirements of nonprize-winning bonds began in 1952 and contributed to the sharp increase in debt service in the early 1950's and the further increase to be anticipated thereafter.

A more likely explanation of the bond freeze is that given in Khrushchev's original proposal [8] and in subsequent official statements [9] [12, p. 24], namely, that annual debt service (in the form of prizes, redemptions, and administrative costs) was absorbing a rapidly increasing share of annual loan revenue, thereby reducing both the gain to the treasury in the form of net revenue and the net anti-inflationary impact of the bond program. As the effectiveness of the bond program diminished, it became less attractive to the Soviet government, which decided to discard it in favor of other sources of revenue. Thus, in the 1958 budget the loss of net bond revenue was more than offset by the expected increase in turnover (excise) tax revenues [10].

[4] Thus, even for the bondholder who did not win a prize there was an element of chance—the uncertainty as to how soon his bond would be redeemed at face value.

[5] The freeze included (1) postponement of service on outstanding mass-subscription issues until 1977, when these issues are supposed to be consolidated and retired over a 20-year period; (2) flotation in 1957 of an issue less than half the size of the 1956 issue and redemption of the 1957 issue in 1958-62; (3) elimination of new flotations after 1957; and (4) continuation of sales of the voluntarily purchased "cash" or "freely circulating" bonds [9].

Another objective of the bond freeze, not acknowledged by the Soviet regime, may have been the desire to increase the monetary incentives, and thereby the productivity, of the labor force through a simultaneous increase in disposable earned income and elimination of unearned income. Because bond subscriptions were compulsory, progressive,[6] and deducted from workers' pay, they had the same disincentive effect as the personal income tax. Their elimination should in turn have the same incentive effect as a cut in personal income taxes, which also has become effective in 1958 [10]. At the same time, the importance of earned income to the Soviet household has increased as a result of the loss of unearned income in the form of bond prizes and redemptions, on which every Soviet household must have counted to some extent because of the virtual universality of bond subscriptions. Although the impact of an increase in disposable income is of course dependent upon the availability of consumer goods and their price level, it is likely that the combined elimination of new subscriptions and termination of service payments on old issues had a favorable (from the viewpoint of the regime) effect on worker incentives.

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[6] Subscription requirements were defined in terms of the number of weeks' wages to be subscribed, and the number required increased for successively higher wage brackets.

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Reply

Mr. Bornstein is correct in showing that I made a computational error in estimating the tax element in Soviet bonds for the postwar years, an error which led me to underestimate the tax element for these years. However, on

my assumption that Soviet prices will continue to fall, the tax element for the postwar period turns out to be considerably less than that for the prewar period, and I still believe that this was probably an important factor in the decision to "freeze" and "discontinue" the bond sales. A 100-ruble bond bought in 1930 had, by 1940, less than 20 rubles of purchasing power in 1940 prices; a 100-ruble bond bought in 1948, had, by 1958, about 200 rubles of purchasing power in 1958 prices.

Now Bornstein argues that prices have been relatively stable since 1954, and therefore from the point of view of the bondholder the tax element is still fairly high (though I would argue not as high as in the 'thirties). The state's decision to discontinue the sale of bonds, however, is not based on bondholders' expectations but rather on its own plans regarding the consumer-goods price level.[1] I would anticipate that in the future the Soviets will continue to use price cuts as a means of passing on increases in the standard of living while keeping wages and other incomes roughly stable. This has been their avowed policy since 1928 and has been implemented since 1948 when control over incomes made price cuts possible. I regard the cessation of price cuts since 1954 as temporary in nature due to: the stepped-up industrialization when Khrushchev first took over; the numerous special measures which have been taken in the past few years to adjust relative incomes (increase in payments to collective farmers, increase in pensions, increase in minimum wages, etc.); the stepped-up housing program, a form of consumer service not reflected in the price indices; and so forth.[2]

Finally, I agree with Bornstein that the "official" argument concerning the freeze and discontinuation has merit: that with repayments plus debt service gradually overtaking receipts, the bond program had become less attractive to the planners. But I think this argument is much more convincing when viewed in the perspective of the change from an inflationary to a deflationary economy with its implications for the "tax element." I do not believe the Soviets would have repudiated (in effect) 260 billion rubles in bonds, equal to about one-half year's personal income, just because their net receipts from sales of bonds were declining. This is not the kind of action that a government, even as centralized and powerful as the USSR, contemplates lightly. The big problem is that repayments today are worth so much more in *real* terms than the original receipts, whereas in the 'thirties they were worth so much less.

If inflation were still rampant: (1) It would still pay the Soviets to sell bonds today paying back a fraction of their real value sometime in the future. Bonds, unlike taxes, are an intertemporal fiscal weapon and it makes sense to assess their utility over time as well as in relation to problems of the moment.

[1] The major purpose of my original note was to estimate the tax element from the point of view of the bondholder. As the note was about to go to press, the Soviets initiated their freeze. My brief addendum on the freeze which is a major target of Bornstein's, was unfortunately misleading on this point.

[2] This point was made in a letter to *Problems of Communism* accepted for publication in January 1958. In Moscow in May 1958, two leading Soviet financial specialists, Bachurin and D'iachenko, expressed the same opinion to me in private conversation.

(2) The 260 billion rubles worth of bonds instead of equaling one-half year's annual income would have been worth much less. Similarly the annual repayments would have constituted much less of a *real* burden than they actually are. (3) Receipts would remain ahead of repayments plus debt service for a longer time, perhaps forever, since sales of bonds would increase each year along with wages, as was the case in the 'thirties. It is difficult to increase bond sales with incomes roughly stabilized.

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BOOK REVIEWS

General Economics; Methodology

Three Essays on the State of Economic Science. By TJALLING C. KOOPMANS.
New York: McGraw-Hill, 1957. Pp. xi, 231. $6.50.

In this important new book Professor Koopmans strives to achieve three major objectives: First, to make available to the nonmathematical economist the achievements in a certain branch of mathematical economics, namely the branch dealing with the properties of competitive equilibrium; both descriptively and normatively, and the closely related problem of efficient allocation of resources through a price system. Second, to present his methodological credo on the construction of knowledge in economics. Third, to display the effectiveness of the "new" mathematics, linear algebra and point set topology, in the formulation and solution of the economist's problems.

The first essay, entitled "Allocation of Resources and the Price System," is by far the most important and the most difficult of the three. In it Koopmans attempts to bring together within a single conceptual structure the variety of contributions to such topics as the decentralization of economic decisions, the existence of competitive equilibrium, the connection between competitive equilibrium and Pareto optimality, activity analysis, linear programming, and input-output studies. Despite the fact that this essay is intended as a summary report or survey of a wide variety of studies, it is so successful in developing the connections among seemingly different kinds of undertakings, that it should be viewed as an eminently creative synthesis. It must be conceded, however, that depite the great efforts of the author to simplify the argument and present it in terms as nontechnical as is consistent with the nature of the subject matter, the going is hard. The mythical "average economist" may expect to get the flavor of the argument, but should not really expect to understand it fully, unless he is prepared to acquaint himself with the mathematical tools needed.

This essay offers an excellent discussion of the conditions under which decentralization through a price system is feasible. However, its main limitation, and by implication the limitation of the literature from which it draws, is the absence of any discussion of the purely organizational as against the technological aspects of the problem of decentralization. The theoretical framework of activity analysis leaves the question of the optimum number of productive units in an economy without a satisfactory answer. Koopmans recognizes this and proposes that a solution be sought through the introduction of strictly convex production sets (decreasing economies to scale) which are associated with indivisible resources. The whole flavor of this proposal is technological and thus the opportunity is missed of expanding the research horizon to include strictly organizational considerations in the theory of the size of the firm.

The first essay also contains an argument in favor of employing the powerful techniques of point set topology and linear algebra in lieu of the "myopic" technique of the calculus with its necessary emphasis on local maxima and minima and local solutions. The argument is well taken, provided that it is kept in mind that, for a wide range of problems in comparative statics and dynamics, it is convenient to fall back on the techniques of "old-fashioned" mathematics.

In the second essay, "On the Construction of Economic Knowledge," Koopmans forcefully argues that economics as a scientific enterprise (rather than as an art) requires a clear separation between the logical and the factual sources of our knowledge. This is best achieved by a fuller use of the postulational method which is described briefly but lucidly by the author. The explicit methodological argument in this essay is generally consistent with the prevailing views, but its tone along with the author's *use* of the postulational method may be somewhat misleading. Koopmans argues correctly that the process of reasoning from premises to conclusions (according to the rules of logic or mathematics) within a given postulational system is fully independent of any factual knowledge. He proceeds to argue, however, that as soon as we provide the terms of a postulational system with a descriptive interpretation (thus using the verbiage of the economist rather than the logician or mathematician), the system assumes relevance and economic meaning. Indeed Koopmans practices what he preaches because in the first essay he distinguishes between descriptively uninterpreted and descriptively interpreted systems, by drawing a sharp distinction between "theorems" (mathematical statements) and "propositions" (economic statements).

This view is not entirely correct. The assignment of a descriptive interpretation to the terms of a postulational system does not necessarily turn it into an applied (as distinguished from pure mathematical) system. A system may remain pure or analytic despite a descriptive interpretation of its terms. Consider, for instance, the sentence: "If the cat is black, then the cat is black." An examination of the "propositions" presented by Koopmans in the first essay establishes that they are all analytic or logically necessary. He accomplishes this by deriving theorems (which he calls "propositions") in which the postulates are asserted to imply the conclusions. If the class of statements B are conclusions from a class of statements A, then the statement "A implies B" is analytic independently of the factual content of A and B taken separately.

It should be stressed that nothing prevents us from deriving some factual statements from the postulational system presented by Koopmans in his first essay. It is merely asserted here that he did not choose to do so. Furthermore, the fact that he provides the terms with a descriptive interpretation does not turn his "propositions" into factual statements and prevents him, therefore, from using them in the manner in which he claims they can be used, namely as empirically refutable or confirmable hypotheses. (Interestingly enough the "propositions" of the first essay contain the essence of what goes under the name of "welfare economics." In the opinion of this reviewer this is no accident. The branch of economics known as "welfare economics" seems to be a

collection of the interesting analytic statements which may be constructed from the body of economic theory.)

The third essay on "The Interaction of Tools and Problems in Economics" is the least interesting of the three. It is a survey of the variety of new tools and techniques that have developed in recent years, in the light of the needs of the discipline. The sample survey, statistical inference, computing techniques, and the "new" mathematics, all receive some attention and brief treatment.

Looking at the book as a whole one cannot help but admire the elegance and high technical competence with which it has been written. Koopmans' primary objective, however, was to write a book that would reduce the isolation of the mathematical economist from his nonmathematical colleague. On this score the book is only moderately successful—not because its execution is defective, but rather because the basic argument requires a degree of mathematical sophistication which is typically possessed only by those who have dealt extensively with mathematical methods.

<div align="right">ANDREAS G. PAPANDREOU</div>

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Principles of Economic Policy. By KENNETH E. BOULDING. Englewood Cliffs, N.J.: Prentice-Hall, 1958. Pp. vii, 440. $7.95.

This latest volume by Professor Boulding was begun as a revision of his *The Economics of Peace* which was published in 1945. As he points out in the preface, however, it has developed into an almost completely new book and, while his preoccupation with war and peace remains in evidence, the book represents an attempt to analyze economic policy in general.

As the title suggests, this is not a book of techniques but rather an exposition of the principles involved in economic policy-making. In fact, Boulding goes so far as to claim it is an introduction to economic principles. In a way this is true, but certainly not in any ordinary sense. It is, on the one hand, actually less than this, for some fundamental economic concepts are given no explicit attention. On the other, it is considerably more because the treatment of policy has forced Boulding beyond the boundaries of pure economics and into the other social sciences and ethics. In this respect the book is difficult to classify. However, while there are points at which the full significance of Boulding's words can be appreciated only by persons of some degree of economic sophistication, its level is generally such that it can be profitably read by intelligent laymen and beginning students.

The book is divided, unequally, into two parts. There is first an analysis of economic policy as such. This section, somewhat the smaller of the two, emphasizes the objectives of economic actions and institutions, indicating the role of the economist and his tools of analysis in the drawing of policy. Following the opening statement of the nature of policy and the uses of the tools of economic science for policy purposes—which are very cleanly enunciated—there is an extended development of each of Boulding's several objectives of policy. Here the general conditions for each to be achieved are set forth or, as is more frequently the case, the conditions which impede the achievement

of each objective are set forth. The concluding chapter of this first part of the book is a useful discussion of ends and means which, in addition to a treatment of the rudimentary ideas of conflict and complementarity of objectives, points up the tendencies to confuse means with ends and to come to regard social and economic institutions as ends in themselves. The role of involvement and loyalty in the dynamics of social organization is also considered, and Boulding shows how different institutions beget different degrees of loyalty from the public and how such loyalty while providing social stability may also be costly. This chapter is among the best of the book.

Much may also be said for his forthrightness in coming to grips with the relationship between ethics and economic policy in the final chapter of the book. It is indeed refreshing to read an economist who, while he has earlier stated that science is concerned with means not ends and has suggested that the critique of ends is more in the province of philosophy than science, frankly acknowledges that the operation of the economy may produce "ideals and value systems which are incompatible" and that these conflicts may challenge the survival of the economy. Boulding sees that the economist in making policy must do more than simply take values and ideals as a part of the empirical baggage with which he is confronted. Where social irresponsibility prevails, for example, policy may have little significance. The limits on policy are set by the organizational structure of society, and this structure, in turn, rests upon the value systems of the society's members.

Despite the obviously gratifying features of those portions of the book dealing with ends and with ethics, the reader may finish them feeling unsatisfied. The willingness to accept for consideration the problems which are associated with ethics, realistic and important though they are, infinitely complicates the task of offering a well-rounded body of principles for economic policy. Boulding recognizes that no single objective of policy is adequate; that policy must achieve a balance among a multiple of objectives.

Since policies imply particular objectives, policy-making, if it is not an art but a science as Boulding claims, is normative rather than positive in nature. It will not do to count one system's fruits and costs, therefore, in terms of the objectives of another. Yet Boulding implicitly lapses into this error, using his list of objectives as a standard for economic policy wherever applied. Thus the exact significance of the book's conclusions about the costs and fruits of communism as judged by noncommunist objectives is uncertain. A similar problem arises if we try to apply essentially normative principles of policy to the large body of "uncommitted" underdeveloped countries of the world. These countries are different from us and probably wish to remain so. No doubt all systems have points of similarity where objectives coincide, but in the development of policy it would appear most appropriate to spend much effort in discerning the full range of each society's goals before the policy "principles" are established.

Economic analysis can block out ranges of impossible action and establish degrees of probability or improbability as guides for building policy, as Boulding points out. These are universal propositions, at least in part, but they are grossly inadequate for policy-making unless supported by a set of

objectives. The scope of Boulding's principles of policy is thus limited in large measure to societies similar to our own. But the book does not make this clear.

Turning to what is quantitatively the larger part of the book, Boulding's earlier discussion of objectives proves helpful within the boundaries of our own social organization. Here he presents a canvass of the problem areas for policy. Attention is given successively to fiscal, monetary, income maintenance, foreign trade, antitrust, agriculture, and labor policies and finally there is a consideration of the economics of war and peace. While this survey has something to offer the layman or beginning student, it will prove less interesting to the professional economist than the earlier section of the book.

Several things work against this wide sweep of policy areas. Each problem area is presented in the context of its history and, to be appropriate for the layman, some of the fundamentals of economics are enunciated. Almost inevitably, because of space limitations, the treatment is less than exhaustive and at some points the analysis will not prove compelling to professional readers even when they can agree generally with the policy conclusions. From this part of the book one gains a feeling of incompleteness. The suspicion arises that what is a chapter should in each case be a book, for the problems addressed are not a few of the technical questions of these fields but those of grandest scope.

Finally, one additional characteristic of this work which is disturbing is the use of language. Certain terms have come to have fairly precise and standard meanings in the field. There are at times advantages to be gained from redefinition when this is exercised with care; but when it is indiscriminate the language can become a jungle. When we find competition defined in this book as the ability of the superior to displace the inferior, the whole scope of the term shifts. The forceful displacement of the peasants by collective farms in Russia is cited as competition along with the suppression of "inferior" minority groups by a totalitarian government. The term "transactions" becomes synonymous with any kind of change. Even the liberal interpretation of this term made by John R. Commons never approached such scope. Distortion of generally accepted meanings for important terms such as these can only be justified by extraordinary analytic advantage.

Yet tempering these critical remarks is the fact that Boulding's approach to policy is made with the careful use of the economist's tools. For the most part he does not point to particular policies except to draw out their implications. He does not suggest that policy can advocate specific "optimum" conditions. Instead he uses his tools to block out certain areas as unfruitful for the achievement of particular policy goals. There is no doctrinaire preaching of specific policies, therefore, but a systematic circumscribing of the ranges within which workable policy must fall. In this sense an approach to policy-making is set forth that should prove educational to all. This is the broad message of the book and it is a worthy message.

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Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Linear Programming and Economic Analysis. By ROBERT DORFMAN, PAUL A. SAMUELSON, and ROBERT M. SOLOW. New York: McGraw-Hill, 1958. Pp. ix, 527. $10.

(As an experiment or gamble, your editor assigned this essentially mathematical volume to a merely semimathematical reviewer. A semimathematical reviewer might appreciate some finite proportion of the contents before being overwhelmed or exasperated by mathematics beyond his ken. At the same time he might speak his piece without vested interest in any mathematical or econometric school or cult. Whether or not such a recipe entered the editor's mind, it is both a logical and an interesting recipe on first impression. But the proof of the pudding is in the eating, and the proof of the review is in the contents.)

Linear Programming and Economics Analysis is in a very real sense Volume II of Samuelson's *Foundations of Economic Analysis,* largely written twenty years ago. As in the *Foundations,* a common set of mathematical tools is shown to underlie what seem at first widely disparate branches of economic thought—in this case linear programming, input-output analysis, capital theory, general equilibrium theory, welfare economics, and the theory of games. This time, however, Samuelson profits from two full partners, each distinguished in his own right. Their shares were (we surmised at first) concentrated most particularly in the fields of their previous contributions: Dorfman's in linear programming for the individual firm (Chapters 6-8) and Solow's in the treatment of Leontief models and capital theory (Chapters 9-12), but we have been assured these surmises were wrong. The book is a compound, not a mixture.

The essential difference between the linear-programming or linear-economics approach and the continuous-function or differential-calculus approach to a wide range of production, allocation, and welfare problems can be put in literary terms: The former can locate in practice the closest feasible approaches (in a discontinuous and otherwise imperfect world) to optimum positions which the latter attains only in theory. Traces of concern for this sort of concreteness and practicality were found in the *Foundations* (for example, in the handling of discontinuities at pp. 46-52); here they dominate the entire volume. The something new which has been added, however, is the technique for actual location of optimal positions in specific cases. And the very fact of something new belies Boulding's famous review article on the *Foundations (Jour. Pol. Econ.,* June 1948, *61,* 187-99) forecasting that only sterile formalism would be added to *Foundations* which formed in themselves the end of one particular blind alley in the history of economic thought.

But architectonics concern most potential readers less than mundane matters: (1) "What mean such mysteries to me, Who am untrained in indices and surds?" (2) Is this perhaps a text for my course or a focus for my seminar? (3) Will it forecast me the markets, or bend tribunals and legislatures to my bidding?

Question 1a. Neophytes (including sophomores, laymen, and the super-annuated) can profit from Chapters 1, 2, and most of 6 (linear programming proper), not to mention the first halves of Chapters 9 (input-output), 14 (welfare economics), and 15 (game theory). For much more they had better seek foundation grants to support them while working through the algebra of sets and matrices. (The Dorfman-Samuelson-Solow appendix on matrices should be touched only *after* the reader has mastered, say, the similar appendix in Klein's *Econometrics*.)

Question 1b. Other volumes are better fitted to conveying linear programming *a la* cookbook. Charnes, Cooper, and Henderson's *Introduction to Linear Programming*, for example, includes many more intermediate steps than this volume; it also pays more attention to computational mechanics. Furthermore (now transcending Fanny Farmer) it spends more time explaining the meanings of the complex parts of the simplex tableau. On the other hand, Dorfman, Samuelson, and Solow have chosen their illustrative examples with much greater felicity.

Question 2. We find the customary disclaimer: "This book is intended not as a text but as a general exposition," although "it has been successfully used for graduate classes in economics." Given the price tag and the demise of the "G. I. Bill," teachers below the seminar level would do well to rely on library copies and hope for a cheaper abridgement covering primarily the material listed above under (1a).

Question 3. No substitutes here, alas, for the sound judgment, the inside information, the silver tongue, or the campaign contribution. The seeker for stupidity insurance must likewise go elsewhere.

Not professing encyclopedism, this volume reflects the special interests and opinions of its three authors. Certain of these may irritate other authorities. Modigliani and his Carnegie co-workers may not approve the Dorfman-Samuelson-Solow neglect of their schemes of quadratic programming in Chapter 8. Leontief and his Harvard group may find the criticisms of input-output projections somewhat severe. Skepticism as to the applicability of game theory to economic analysis may not be seconded by Morgenstern and his associates at Princeton. (This reviewer would tend to sympathize more deeply with the Carnegie complaints than with the others.)

It is sometimes thought misleading to derive conclusions sounding like policy presciptions or theoretical innovations on the basis of restrictive assumptions "in fine print" or otherwise concealed from the casual reader. This book offers three examples in its later chapters, all elegant derivations from the mathematical viewpoint. In Chapter 12 the competitive market is shown to provide optimal, or at least efficient, patterns of capital accumulation and economic growth through time—provided all persons foresee the indefinite future with absolute certainty, live forever, enjoy transmigration of souls, and/or treat their descendants' interests as their own. In Chapter 13 is demonstrated the existence of an unique and nonnegative solution for the Walras-Cassel general equilibrium model (fixed production coefficients, fixed stocks of resources, no intermediate products, an' a' that), provided we permit structural unemployment of some of our resources. Finally (Chapter 14) a

fundamental theorem of welfare economics is derived, equating free competition with Pareto welfare optima—a new equilibrium and a new Pareto optimum for every change in the initial distribution of assets. The welfare objections to excise taxation are also restated here—without warning that similar objections apply to a great many other types of taxes, once leisure is recognized as a commodity.

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Economic History; Economic Development; National Economies

Trade and Market in the Early Empires: Economies in History and Theory.
Edited by KARL POLANYI, CONRAD M. ARENSBERG and HARRY W.
PEARSON. Glencoe, Ill.: The Free Press, 1957. Pp. xviii, 382. $6.00.

This book is a collection of essays growing out of research at Columbia University on the origins of economic institutions and the economic aspects of institutional growth. Most of the contributors have been members of the faculty or graduate students at Columbia in one or another of the social sciences. Some of the essays discuss aspects of early or primitive economies (Babylonia, early Assyria, Mesopotamia, Aztec and Mayan civilizations, the Guinea Coast, the Berber highlands, the Indian village). Others are of a more general quality ("The Place of Economics in Societies," "The Economy as Instituted Process," "Economic Theory Misplaced: Livelihood in Primitive Society," "The Market in Theory and History").

The burden of the book is summarized in an introductory note:

> Most of us . . . think that the hallmark of the economy is the market. . . .
> What is to be done, though, when it appears that some economies have operated on altogether different principles. . . . The conceptual problem arises in marketless economies where there is no "economizing," i.e., no institutional framework to compel the individual to "rational" and "efficient" economic activity, or "optimum" allocation of his resources. . . .
> In that case the economy would not be subject to economic analysis since this presumes economizing behavior with supporting institutional paraphernalia, e.g., price-making markets, all-purpose money and market trade. . . . The aim [of the book] is not to reject economic analysis, but to set its historical and institutional limitations, namely, to the economies where price-making markets have sway.

If one examines economies empirically, it is said, three patterns of economic behavior are encountered. These are "reciprocity, redistribution, and exchange."

> Reciprocity denotes movements between correlative points of symmetrical groupings; redistribution designates appropriational movements toward a center and out of it again; exchange refers to vice-versa movements taking place as between "hands" under a market system.

Formal economics, or economic theory, or economic analysis, say the authors, has relevance only to the final case in this trilogy. But only a brief period

in the whole history of human society, and only a few societies in the universe of all societies have been characterized by "market systems." Therefore, formal economics is not helpful in explaining or understanding aggregate behavior for most of history and for most societies. "The economist cannot be of help to the student of primitive economies; indeed, he may hinder him." The anthropologists who have sought to study primitive economies "within the framework of orthodox economic theory" have fallen into methodological traps.

What are the qualities which, it is said, are possessed by the economies for the study of which conventional analysis is not helpful? They are: inflexible or sluggish prices or exchange ratios; inelastic (sometimes absolutely inelastic) supply; inelastic (sometimes absolutely inelastic) demand.

The specific instances enumerated by the authors—e.g., set rates, customary or statutory equivalencies, gift trade, administered trade, status-trading, trading partnerships, the influence of kinship, magic and etiquette on economic behavior, noncompeting groups—seem to fall into one or more of these boxes.

It can be seen, for example, that the supply of imported goods at retail in Dahomey is less elastic than it would be if foreigners were not excluded from the trade; that the supply of watchman services would be more elastic in the Indian village if it were not necessary to be of the watchman caste to qualify for producing the service; or that the demand for the goods of a Tobriand Islander is less elastic than it would be if he were not bound to transact with specified "partners" and were free to exchange with all comers.

Now the conventional doctrine and techniques of formal economics have much to say about economies or markets in which inflexibilities and inelasticities occur. It is not true that economic analysis cannot perform useful predictive tasks in such economies. Even a quick glance at a text on economic theory (say, Kenneth Boulding's *Economic Analysis*) can show the attention which has been given to rigid prices and to the meaning, for a large number of analytical questions, of different degrees of elasticity of supply and demand schedules. That is to say, economics, in its "language" sense, can explain phenomena in economies in which prices are not free to move or in which demand and resources are not responsive to price changes. It can explain phenomena even in economies in which people do not maximize economic quantities.

But is it true, in fact, as the contributors to this volume profess, that people in the economies they have examined are not maximizers of economic quantities? People may seek to maximize in two distinct frameworks, one of which is free of constraints and the other subject to constraints. The first can be illustrated by the case of a hypothetical primitive who has pigs which he may exchange for yams at any pig-yam ratio and who may trade with anyone. The second is the case of the primitive who may trade at any pig-yam ratio but for whom a single trading-partner is specified. In both cases, the pig-owning primitive may maximize in deciding whether to trade at all, and, if so, how much pig he is willing to forego. The position taken in this book is that maximizing behavior or the achievement of optimal solutions requires the prior existence of a "System of Self-Regulating Markets" free of rules which

constrain choice and (implicitly) in which supply and demand schedules are price-elastic. This is surely not true.

It would have been interesting to examine whether, with given constraints, primitives do in fact maximize. If the traditional share of the rice heap "paid" to the barber in the Indian village is much below the value of his services to the community, is there no way in which he can escape to some other trade or reduce the quantity or quality of the services he renders? The door to this area of inquiry was foreclosed by prejudgment.

It would also be interesting to see whether, in primitive societies, the constraints themselves do not change when they interfere with the achievement of optima. If the technology of fishing progresses more rapidly than that of yam culture, does the fish-yam exchange ratio change, however slowly, in favor of yams? Here and there, hints appear that at least some constraining rules do so change. For example, in Assyrian trade, "the necessaries of life were supposed to be subject to permanent equivalencies ('prices'); actually they were subject to long-range changes." This question, however, was also not really subjected to close examination.

The meaning given by the authors to the distinction between formal and substantive economics may be the source of the whole difficulty. Formal economics derives from logic and refers to the rules for choosing among alternatives. Substantive economics derives from fact and is the "process of interaction between man and his environment, which results in [want satisfaction]." The two, they say, have nothing in common; they "could not be further apart." And they propose that only the substantive meaning of economics can yield the "concepts that are required by the social sciences for an investigation of all the empirical economies of the past and present."

In neither respect are they correct. First, formal economics and the empirical economy have everything in common. Theory is not an isolated exercise in pure logic; it is an instrument for making predictive statements about experience and these predictions are tested by reference to the real world. Secondly, while economies can be described empirically by exclusive reference to them, fruitful predictive statements cannot be made by this procedure and it is precisely the task of the social sciences to make predictions about social phenomena. John Neville Keynes once said: "The prevalence of a low type of inductive reasoning in the treatment of economic questions is one of the most fertile sources of economic fallacy."

The only way to know whether the usual economic theory will give good predictive results for primitive economies is to test the predictions derived from the theory by observing whether empirical observation is consistent with the predictions. This the authors did not do. What they did instead was to examine the conventional assumptions of the theory and seek to determine whether these found empirical counterparts in the primitive economies. This is a fruitless search. The conventions of economic theory (as of any manipulable theory in any scientific discipline) are so ideal and abstract that they are found in no real world. The significant question is not whether real-world duplicates can be found for the assumptions, but whether real-world observed experience duplicates theoretically derived predictions.

. The book does not quite carry the day and claims ("first breakthrough," "fundamentally different starting point," "a significant widening of our out-look," "the threshold of much more comprehensive research in the social sciences may well have been reached") are more impressive than achievement.

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Rich Lands and Poor: The Road to World Prosperity. By GUNNAR MYRDAL.
    New York: Harper & Brothers, 1957. Pp. xx, 168. $3.00; text ed. $2.25.
    (Published in England as Economic Theory and Under-developed
  . Regions.)

The very large and steadily increasing income differences between developed and underdeveloped countries constitute the focus of Gunnar .Myrdal's new book, a revised version of the Anniversary Commemoration Lectures of the National Bank of Egypt, delivered in Cairo in 1955 (published by the bank under the title Development and Underdevelopment: A Note on the Mech-anism of National and International Economic Inequality, Cairo 1956). As the eminent Swedish scholar points out, "though these inequalities and their tendency to grow are flagrant realities, and though they form a basic cause of the international tension in our present world, they are usually not treated as a central problem in the literature on underdevelopment and development" (p. xviii). Part I of the book attempts to explain why and how these inequali-ties came to exist, why they persist, and why they tend to increase, and touches briefly on the question: How could these trends be reversed? Part II inquires into the reasons for the relative neglect of these questions in orthodox economic theory.

. . An examination of the sources of income inequality between regions within a country provides the model for interpretation of the international differences. While some attention is devoted to political factors (and there is much of interest in this discussion) the center of the analysis—and the part most challenging to standard economic theory—concerns the role played by free market forces. Economic growth is conceived as a cumulative process—in part because of a circular interaction of rising investment, incomes, demand, and investment; in part, and more importantly, because of "ever-increasing inter-nal and external economies—interpreted in the widest sense of the word to include, for instance, a working population trained in various crafts, easy communications, the feeling of growth and elbow room, and the spirit of new enterprise . . ." (p. 27). Thus, an initial change for the better is conceived to produce a cumulative movement upward; conversely an initial change for the worse sets in motion a cumulative process downward. If a cumulative upward movement is initiated in one region, it is argued, this gives rise via movements of goods and resources to certain adverse changes ("backwash effects") in other regions, which initiate a downward movement. For example, an influx of products from the growing region tends to destroy handicrafts in the poorer region, or out-migration from the poorer region has adverse effects on the age composition of the population and thus on the productivity of that region. The resulting downward movement tends to cumulate because the provision of such

things as public utilities, medical services and education in the poorer region shows a relative deterioration. To be sure, there are certain positive repercussions or "spread effects" on the poorer region, such as the new market for raw materials for the developing industries in the growing region. And there are factors which may slow down the cumulative process in the growing region and have favorable repercussions on the poorer—"external dis-economies" arising from excessive concentration of industry and population, rising wage costs which may drive industry away, a growing stock of outmoded equipment. and so on. But such factors are considered to be "complications and qualifications" (p. 35), and it is held that in the normal case the backwash effects will outweigh the spread effects, and that typically "the play of the market forces works toward inequality" (p. 26). The possibility of an exception is recognized in the case of rich countries, where the spread effects exert greater influence because economic growth is accompanied by "improved transportation and communications, higher levels of education, and a more dynamic communion of ideas and values" (p. 34). But when the analysis shifts to the all important problem of international as opposed to regional income differences, it is asserted that "the spread effects are much weaker and the cumulative process will more easily go in the direction of inequality if the forces in the market are given their free play" (p. 56). So far as policy is concerned, the general conclusion is that "economic development has to be brought about by policy interferences by the world community or by the individual underdeveloped country . . ." (p. 53).

Needless to say, this brief sketch is an inadequate outline of the author's analysis—in particular, the stimulating and provocative consideration of the role of noneconomic factors in the cumulative process has not been treated. Moreover, one cannot be sure that a fair representation has been given even with regard to the portion relating directly to economic theory, since the analysis in the original is loosely formulated. But the presentation does perhaps serve to highlight the principal challenge which Myrdal directs to the usual conclusions of economic theory, namely that the income-equalizing processes ordinarily emphasized—trade, migration, and capital flows—in fact tend to have just the opposite effect. And on this point he is quite explicit: ". . . The movements of labor, capital, goods and services do not by themselves counteract the natural tendency to regional inequality. By themselves, migration, capital movements and trade are rather the media through which the cumulative process evolves—upward in the lucky regions and downward in the unlucky ones" (p. 27). To be sure, this assertion is moderately qualified later on, when allowance is made for the "spread effect" of trade (though not, it should be noted, for that of migration or capital flows), but as the foregoing sketch suggests, it is maintained that typically the net effect of these flows of goods and resources is to widen income differences.

It must be added, however, that Myrdal's position on this point seems somewhat equivocal. For, as noted, he does recognize the equalizing or "spread" effects of certain of these flows, and even goes so far as to say that "it would be possible to conceive a situation where in a very rich country the spread effects would on the average be stronger than the backwash effects, with the

result that inequalities would actually be diminishing *as an effect of the play of the market forces*" (p. 39, italics added). Now in view of what has gone before, this is a rather puzzling statement. For if sustained growth is initiated in one area in a low-income country, and then, following Myrdal, flows of goods, labor, and capital, set in motion a process of ever-widening income differences among regions, one wonders how this process is ever to be reversed, especially when one also takes into account Myrdal's emphasis on certain cumulative processes which reinforce the initial tendency towards divergence of income levels.

But, it might be argued, this is mere theoretical quibbling. Does not the overwhelming evidence of the growth of international income differences in the past itself testify to the validity of Myrdal's model and the erroneousness of the conclusions of standard economic theory—specifically international trade theory? The answer to this would seem to be that the conclusions of standard theory have very little relevance to the problem at hand. For these conclusions are drawn from a static model which bears little resemblance to the international scene of the past 150 years. The outstanding economic fact of this century and a half is the unparalleled cumulative development in the body of technological knowledge applicable to economic processes. And it seems self-evident that the pattern of ever-widening international income differences is to a very great extent a reflection of the marked differences among nations in the rate at which this new and ever-expanding technology has been absorbed. One might maintain, of course, that in theory the diffusion of new techniques, under competitive pressures, should proceed rapidly. But such an argument fails to allow for the wide variations among nations in factors such as historical heritage, politico-social structure, natural endowment, and demographic pattern, and the fact, clearly established by the experience of presently developed nations, that absorption of this technology demands extensive internal adaptations not only in economic organization but in the whole structure of individual and social life.

Now when one takes account of these manifestly relevant circumstances, it is hardly surprising that international income differences have progressively widened despite the enormous expansion in international flows of goods and resources—itself without historical parallel. For it seems a plausible hypothesis that while these flows may have worked in the direction of equalizing income levels—as static theory would suggest—that their quantitative effect was far outweighed by the unprecedented pace of technological advance and differential absorption of this technology by the nations of the world. Moreover, the hypothesis would be consistent with resort to these flows for explanation of the observed tendency towards convergence of regional income levels *within* some developed nations, for in this case the relative magnitude of these flows would quite clearly be larger and the differential geographic impact of technological change much less.

To be sure, the foregoing is no explanation of the widening of international income differences—one might almost say it is a truism. Nor is it necessarily to disagree with Myrdal's conclusion that the economic development of underdeveloped countries can not be left to the free play of market forces. But it

does suggest a shift in analytical focus—from the international economic flows emphasized in static analysis and emphasized also by Myrdal, though with "inverted" effects—to the basic internal processes which encourage or inhibit exploitation of the new opportunities made possible by modern economic technology. This is not to deny the possible importance of international factors— whether economic or noneconomic—in the process of national economic growth, but one wonders if these forces are not sometimes made to bear undue weight relative to internal processes.

It goes without saying that a work from the pen of Gunnar Myrdal is well worth reading. There is much that is stimulating and valuable—the emphasis on circular causation and the factors involved, both economic and noneconomic, in the process of economic growth; the discussion of the role of the state and the colonial system; the penetrating remarks on the current political relations between the developed and underdeveloped countries; and the criticism of the preconceptions of current economic theory. And above all there is the ever-present concern with the relevance of economic theory to the diffusion of material well-being and the growth of political democracy.

RICHARD A. EASTERLIN

University of Pennsylvania

The Economics of Full Employment in Agricultural Countries with Special Reference to India and Ceylon. By C. SURIYAKUMARAN. Kandy, Ceylon: K. V. G. DeSilva, 1957. Pp. 307. Rs 12/50.

The author of this book is 35 years old. He received his B. Econ. degree at the University of London in 1945 and subsequently spent two years in the United Kingdom and on the continent of Europe "in practical work and postgraduate study." The publisher refers to the present book as that of "the first Ceylon economist to publish a full work."

The outstanding characteristics of the book are the great breadth of treatment and the reading upon which it is based. This reviewer would have liked to have found more of specificity as to the lines that economic development should take in such countries as India and Ceylon. The author deliberately rules this out, however, by distinguishing between policy and programs and saying that his book considers only policy.

The main conclusions reached are that first emphasis should be placed on wage-goods industries producing goods that replace imports, and second emphasis on industries like housebuilding and the like and then on export industries "wherever feasible under world competitive conditions." Industrialization is the remedy for unemployment and underemployment, both of which are endemic in underdeveloped agricultural countries. But he defines industrialization broadly enough to include "large-scale programmes for agriculture, raw materials, and certain processed 'consumption' goods" (p. 290).

The next link in the chain of reasoning is that such industrialization can be achieved only by investments and that investment savings are in scant supply in such countries. Ways must be found, therefore, for channeling more of what savings there are into industrialization investment, and in this the state has a large role to play. Suriyakumaran does not go so far as to advocate the extreme

enforced savings measures employed by the Russians, but in general takes the position that countries like India and Ceylon need to carry planning and state direction and conduct of economic activities to a degree maybe half way between that of the Russians and that of the extreme Western world. (He very carefully discriminates between Russian and Communist.)

The other source of investment funds must be the United States and other countries with large supplies of these. The author builds up a strong case for the gains that will come to these countries from trade with India and Ceylon and their like when these countries become balanced economies. He pictures a world of the future in which even the rich countries will live more abundantly as a result of obtaining from other countries the goods which the latter are best qualified to produce. In the period while the agricultural countries are getting their wage-goods industries developed, however, their use of import quotas and even of protective tariffs, preferably the former, is not only warranted, says Suriyakumaran, but highly to be recommended.

Considerable attention is given to the question of large-scale and heavy industries versus small-scale and cottage industries. The conclusion is that both are needed, the latter because of the need for employment close to the homes of families with surplus workers all or much of the year, also close to sources of raw materials. Lack of coal and iron in Ceylon limits heavy industry.

The author takes a strong position against using public funds in leaf-raking and other make-work employments. Instead, they should be employed along with capital investment in developing new industry. Full employment is carefully defined as something more than a job for everyone. Merely putting a person to work at a task that yields little product is not enough. The level of living attained is also an essential part of full employment. But very little is said to indicate what this level is to be (Suriyakumaran actually uses the term standard of living, but what he means is level of living.)

One of the longest chapters outlines the national planning needed before any undertaking in industrialization is begun, its *modus operandi,* and all the factors that must be taken into account, such as consumer's choice, wages, prices, costs, and international exchange balance.

Really only one chapter (Ch. 9 in the center of the book) is devoted to describing the economic organization of the two countries chosen as cases. The two countries differ greatly in one important respect. Half of the income of Ceylon is from exports of tea, rubber and cocoanut, largely grown on plantations. But still three-fourths of the rural dwellings in Ceylon and two-thirds of the urban ones have only one or two rooms. Most of the rural houses in both countries are mud huts with thatched roofs.

The author in conclusion says that the theory he employs in his analysis is after all only the general theory of employment and unemployment, and that there will be residual unemployment in agricultural countries as long as there is any capital starvation; and after this has been cured there will be frictional unemployment and the traditional unemployment that comes with depressions.

JOHN D. BLACK

Harvard University

Indonesia's Economic Stabilization and Development. By BENJAMIN HIGGINS.
New York: Institute of Pacific Relations, 1957. Pp. xxii, 179. $4.00.

In this interim report by the Massachusetts Institute of Technology Center
for International Studies on its ambitious survey of Indonesia, Professor
Higgins briefly describes and appraises both the short-run stabilization meas-
ures and the long-run development policies adopted by the young republic.

With an 83 million population that is growing at a 1.7 per cent annual rate
and is plagued by disguised unemployment, Indonesia faces an urgent yet
difficult development problem. Fortunately, however, a lack of natural re-
sources does not constitute a serious development obstacle. The main require-
ment centers around the complex problem of increasing the rate of capital
accumulation. Higgins estimates that an annual net investment rate of about
Rp. 12 billion is necessary to initiate and sustain a 2 per cent annual rise in
per capita income. Consequently, as one who stresses the importance of a "big"
push to break out of the vicious circle, he contends that the approximately
Rp. 6 billion of annual public and private net investment outlined in the first
phase of Indonesia's five-year plan is inadequate. He also argues that insuffi-
cient attention is directed towards the fostering of entrepreneurship and to-
wards the development of large-scale industrialization projects.

According to Higgins, the present modest plans for capital formation are
not limited so much by an inability to secure domestic and foreign investment
funds but rather by a political environment that prevents any government
from making clear-cut decisions concerning the format of the Indonesian
economy. Before significant development efforts can be undertaken, the Indo-
nesians must decide such issues as the future importance of the traditional
"family-like" society, of foreign businesses, and of public enterprise.

Higgins not only fills an important gap in our knowledge of Indonesia, but
for those interested in general development issues he provides an excellent,
concise case-study of the type of problem faced by the poor nations. Perhaps
he overemphasizes the need for a substantially greater initial effort to start a
cumulative development process, but, utilizing the growing body of economic,
political, and sociological knowledge collected by his group, he does present
convincing reasons for his conclusions. Certainly, to judge by this short report,
the final study of the Indonesia Project which will integrate the results of all
its economic and sociological surveys promises to be an outstanding contribu-
tion to development literature.

ROBERT E. BALDWIN

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Land Reform and Development in the Middle East. By DOREEN WARRINER.
New York and London: Royal Institute of International Affairs and
Oxford University Press, 1957. Pp. vii, 197. $2.90.

The Middle East is a land of dilemmas, paradoxes, and enigmas. It is also
an area in the process of rapid and sometimes bewildering change. Into this
tangled situation of problem within problem Miss Warriner casts a bright
light from the perspective of years of study of the conditions of agricultural
progress. This book, a sequel to her modern classic, *Land and Poverty in the*

Middle East, published in 1948, is not a general essay on land reform and economic development. Rather, it is a surefooted selection of critical variables in change with reference to unique situations.

The author's thesis is that the picture of the Arab world as static and medieval is no longer true. Three new dynamic forces are observed: (1) the money from oil in Iraq; (2) the private enterprise of "merchant-tractorists" (merchants who have financed the mechanization of extensive agriculture) in Syria; and (3) the revolutionary development of a national function for the professional middle class, and of a new status for the fellahin in Egypt. She sees hope for the struggle against poverty only in the confluence of the three forces. It is a measure of the irony of nature and of history that each country lacks what the other has.

The platform from which the author writes is that "The need for reform is two-fold, a social need for a higher income for the cultivators, and an economic need for better farming through more investment and better methods" (p. 7). The basic malady in the three Arab countries here considered is seen to be the prevalence of institutional monopoly in land-ownership, linked with a monopolistic supply of capital to agriculture (p. 6).

Since this is her outlook, it is to be expected that she should view with favor the 1952 land redistribution in Egypt and the advent of a new merchant-tractorist group in Syria, but should see little hope for an engineering of huge public works projects in Iraq unaccompanied by basic change in the social structure.

The author concludes that in the Egyptian land reform of 1952, "The Government has done as much as was practicable, and far more than might have been expected" (p. 48). She observes that it was carried through with no change in the type of farming or the scale of operation and with a high degree of administrative competence. According to the author, "There can be no doubt that the farmers have gained a considerable increase in income. It is true they have not become independent owners, and that the cooperatives are managed by state officials, in what is, in effect a system of collective farming" (p. 47). Again, "In practice, though not in theory, the redistribution of property means the land is nationalized" (p. 48).

But the situation in Egypt remains desperate, with mounting population pressure, which can be met only by massive capital outlays on irrigation and new industry. The author is not overly sanguine about the present (she points to the doubtful success of lowering rents and raising wages by decree) but she often makes apologies for the future. Thus: "Egypt . . . gets the men and women and land ready for the water, while raising funds and the whirlwind by playing Great Power politics. If the pitch seems rather too high, and the expenditure rather too lavish, that is probably the only way in which anything can be accomplished in this very old country" (p. 54).

In Iraq and Syria (in contrast to Egypt) the author points out that land areas are expanding, yields are low and variable, and agriculture is extensive. The social structure is looser and less stratified. She finds that few underdeveloped countries in the past ten years have made such rapid progress as Syria (p. 71). This is in spite of the fact that Syrian governments have done almost nothing to promote agricultural development, and is due to the fact that "The

merchant class of Syria, and chiefly of Aleppo, famous as traders in other countries, has turned its business acumen back into its own country and has used its capital to mechanize agriculture" (p. 75). This enterprise was quite unlooked-for by the author ten years earlier.

In this reviewer's opinion, the most interesting section of the book is on Iraq. The author finds that "Iraq's experience is fascinating, because it demonstrates what money can and cannot do" (p. 113). "What money apparently cannot do is increase production. . . . Nor can money bring about a rise in the standard of living" (p. 125). The fault is: ". . . the social structure of the country is not adapted to expansion" (p. 121); there is no new economic class to rival the power of the landowners (p. 172). Oil does not create a class of domestic enterprisers. "Iraqi planners have yet to learn to do little things. The result is that too much is invested in the big projects, and not enough in working capital, or in human resources. The country is an economist's cloud-cuckoo land, in a Hayekian gap without a crisis, and building Keynesian pyramids without inflations or multipliers" (p. 126).

The social vacuum in Iraq's tribal society, in its villages and its cities, and the gaps btween new and old and young and aged are convincingly described. The author is frank to say that she cannot foresee how the vaccum in social relationships will be filled (p. 183).

The reader is cautioned against association of economic development and solution of political problems of the West. "Land reform is a way of raising the standard of life and of raising production, and rather a slow way. It is not likely to pluck irons out of the fire for the West, or to settle issues in the cold war" (p. 186).

The book is written in a mood of intense concentration on the main problems at hand. The mood is broken with a few rather academic catalogings of land-tenure practices which are not fully integrated with the rest of the book. The subdued fury with which the frustrations are described is lightened by a journalistic air with which the reader is introduced to a wide variety of persons and places. One leaves the book with the hope that Miss Warriner will not wait ten years for her next book on the Arab East.

ROBERT J. LAMPMAN

University of Wisconsin

The Growth Rate of the Japanese Economy since 1878. By KAZUSHI OHKAWA and ASSOCIATES. Tokyo: Kinokuniya Bookstore Co. Ltd., 1957. Pp. xvii, 250.

This book is the first of a projected series of English translations of studies conducted by the Hitotsubashi Institute of Economic Research during the past decade or so. The Institute, staffed by a very able group of economists formerly under the direction of Shigeto Tsuru and presently of Keiji Ohara, has been undertaking a comprehensive quantitative survey of the Japanese economy and much of the preliminary results has already appeared in Japanese. In this volume, the Institute's national income and capital studies are presented as an interim report.

The pioneer work on the long-term estimates of Japanese national income was carried out by Yuzo Yamada of Hitotsubashi in the study *Nihon Kokumin*

Shotoku Suikei Shiryo (Tokyo 1951). (The studies of S. Hijikata in the 1930's also deserve to be mentioned.) Ohkawa's work is an extensive reworking of Yamada's series.

The book contains estimates of national income by industrial origin from 1878 to the present and a detailed discussion of the methods, sources and reliability of the figures. Since income in current prices is deflated to a constant-price basis and reduced to per capita units, there are also chapters on price indices and population statistics. A third portion of the book (the work of Ito and Shinohara) is devoted to estimates of capital formation and capital coefficients. To Western students of Japan, the wealth of supplementary statistics—the best currently available—and the discussion of source materials from which the estimates were constructed may prove to be just as valuable as the income estimates themselves.

The authors are well aware of the difficulties and pitfalls in the measurement of the income, savings and capital of Japan going as far back as the beginnings of modern Japan. Their report is replete with caveats and candid references to the weak points of their work. The major finding is that national income in 1928-32 prices grew at an average (compounded) rate of about 4 per cent per year during 1878-1942; and with population growing at a rate of 1.2 per cent, the rate of increase of per capita income was about 3 per cent. On the basis of my own studies, and for a number of reasons too involved to be stated here, my guess is that 4 per cent overstates the increase and the true rate may be closer to 3 per cent. The value of Ohkawa's study, despite a host of difficulties, is that it is possible to state with some degree of confidence that the national income of Japan grew at a rate ranging from 3 to 4 per cent. Another important finding is that the secondary sector (mainly manufacturing) grew about three times as fast as the primary sector (agriculture). (This compares with two times faster in the United States for a comparable period and suggests a number of interesting problems concerning Japanese growth.) The estimates are too crude, as the authors point out, for the changes in the rates of growth from decade to decade to be taken seriously. For example, the rates of growth shown for the early decades appear too high to me, particularly since industrialization did not make a significant start until the turn of the century.

The estimates of saving and total capital (even their levels let alone their changes) are also regarded by the authors as based on shaky grounds. But here again if Ito's estimates based on comprehensive wealth data are compared and checked with Shinohara's figures based on detailed but less comprehensive source data, it appears that the average capital-output ratio was somewhere around 3 to 4 and the average saving-income ratio was around 10 to 15 per cent. (In terms of the Harrodian formula, and assuming the equality of the average and marginal capital coefficient, if the growth rate is 3 to 4 per cent and the capital-output ratio is 3 to 4, then their product, the saving ratio, is from 9 to 16 per cent.)

The dollar estimates of the United Nations Statistical Office for 1952-1954 show Japan's per capita dollar income to be about one-tenth that of the United States, one-fifth New Zealand's and Sweden's, one-half of Argentina's and about the same as Mexico's. Even if we double Japan's per capita dollar

income to allow for understatement in the deflation procedures, Japan's per capita income is only one-fifth of the United States', less than one-half of New Zealand's, and about equal to Malaya's. A puzzling question is posed by this comparison: what happened to all the increases in national product and productivity represented by the rapid rate of growth over nearly a century? (Even if we allow for an upward bias and reduce the over-all rate from 4 to 3 per cent, Japan's per capita rate still represents an impressive achievement and compares favorably with those of the United States, Canada and Sweden, countries with the highest rate over a long span of time.) Part of the answer may be that Meiji Japan in the 1870's started with a level of per capita income considerably below that of the United States in the 1870's, and another element may have been the unusually rapid structural shift from agriculture (low-income sector) to nonagriculture (high-income sector). Or it may be that per capita dollar comparisons for civilizations as far apart as Asian and Western civilizations are statistically meaningless over a wide range of income. Granting all this, the fact remains that levels of living (meaning more than incomes and including length and intensity of work) in Japan today are far below those of Western countries. A significant part of the increases in Japanese national product and productive capacity must have been dissipated in wars and preparations for wars. If this is so, is such a high rate of growth desirable, especially since it entails almost inevitably an authoritarian political structure? Because the development of Japan is so relevant to the economics of growth of other Asian countries, one hopes that in their analytical studies which are now under way, the Hitotsubashi economists will be able to throw some light on this question.

HARRY T. OSHIMA

University of Washington

Statistical Methods; Econometrics; Social Accounting

The National Economic Accounts of the United States. Hearings Before the Subcommittee on Economic Statistics of the Joint Economic Committee, 85th Cong., 1st sess., October 29 and 30, 1957. Washington: Supt. Docs., 1957. Pp. 302. 75c.

One of the most important advances in economics in the last twenty-five years has been the development of the national economic accounts, which are now known and used in varying degree by almost everyone in the profession. The complexity of the accounts has made necessary an extension of the division of labor within the profession with the development of specialists in the national accounts. This division of labor creates a need for communication from the specialists to the profession at large concerning the nature and intelligent use of the accounts. It also creates a need for periodic appraisal of the accounts from the point of view of the economic analysts who use them.

With the latter objective in mind the Bureau of the Budget requested the National Bureau of Economic Research to set up a National Accounts Review Committee. A committee of nine members was appointed in November 1956 with Raymond W. Goldsmith as chairman. This committee completed its report in June 1957. On October 29 and 30, 1957, hearings on the report

were held by the Subcommittee on Economic Statistics of the Joint Economic Committee of the Congress. The volume under review contains the transcript of those hearings and, as an appendix, the report of the committee appointed by the National Bureau.

The objective of the committee was to provide in its report "a road map for national accounting during the next 5 to 10 years." The main recommendation of the committee is for the development of an integrated body of national accounts. The committee distinguishes five segments of the national accounts, including, in addition to the national income and product accounts, the international balance of payments statement, the flow-of-funds statements, input-output tables, and the national balance sheet. The committee recommends that attention be given to the conceptual integration of these five systems of accounts, with eventual publication of a single combined set of accounts. Responsibility for this publication should be given to a single agency, the committee believes. The committee worked out and included in its report a set of accounts and tables illustrating how a unified set of accounts might be organized.[1]

The committee's case for integration is persuasive; but in view of the uneven development of the different sections of the accounts, an integration of the accounts of the type the committee proposes must be regarded as an ultimate goal rather than a practical possibility for the near future. Integration also raises questions in the minds of the officials of different agencies concerning their role in the integrated effort. Integration, nevertheless, should be the goal.

The report contains a discussion of the present status and objectives of each segment of the national accounts. Several chapters deal with various problems and proposals having to do primarily with the national income and product accounts. An interesting chapter appraises the statistical adequacy of these accounts. The other segments are treated more briefly.

The further recommendations of the committee are numerous and do not lend themselves easily to summary. As far as the national income and product accounts are concerned the committee took the position that the work of the National Income Division is of high quality and that little improvement can be expected without an expansion of the resources devoted to its work. The committee deplores the cut of about one-quarter in the staff of the National Income Division from 1951 to 1957, and urges that this trend be reversed. The committee also makes a variety of proposals for the improvement of the underlying statistical series on which the national accounts rest. It notes what its chairman refers to as our "scandalous ignorance" about unincorporated business, and the lack of information about capital expenditures. The committee also stresses the importance of a more complete set of constant-dollar estimates by the National Income Division, including quarterly data. Of a group of 61 users of the data polled by the committee, more mentioned need for quarterly data on GNP and its principal components in constant-dollars than any other extension of the national accounts. The committee recommends that quarterly data in constant prices should be published in the near future,

[1] This system, the committee notes, was strongly influenced by that set forth in *National Income Accounts and Income Analysis*, Richard and Nancy D. Ruggles, New York 1956.

with a more elaborate program of constant-dollar estimates in the longer run, including, for example, the development of additional price indexes appropriate for deriving constant-dollar estimates for such commodities as producers' and consumers' durables. It may serve to illustrate the range and diversity of the recommendations related to the income and product accounts to mention as examples the following: a proposal that the personal segment of the accounts be deconsolidated to show separate figures for households and institutions (such as nonprofit organizations); a proposal for an improved classification of expenditures which, among other things, would show separately gross outlays for the acquisition of assets; a proposal that further development of regional and local estimates of national accounts should be undertaken at the local level rather than by federal statistical agencies; and a series of proposals concerning estimates of size distributions of incomes. The committee also makes a series of recommendations concerning flow-of-funds statements as these are being developed by the Federal Reserve Board, particularly stressing the importance of the effort now being made to put these statements on a quarterly basis. It recommends that work on input-output tables by the government should be resumed and that a table should be constructed based on the 1958 economic censuses. It also proposes further work on estimates of the national balance sheet but feels that this work should take the form of a study by a private research institution in view of the roughness of the estimates and the unresolved conceptual problems in the field.

The list of recommendations, in the reviewer's opinion, mixes together proposals which are of vital importance and proposals whose importance is secondary. An alternative program for the committee would have been to focus attention on those areas in the national accounts where improvement is most urgently needed. The report pays little attention to the crucial practical question of priorities. In which areas would additional expenditures make the greatest contribution to knowledge of the economy? The proposal to obtain reliable data on saving separately for unincorporated businesses, households, and nonprofit organizations deserves a prominence which it did not receive in the report. The remark about our "scandalous ignorance" about unincorporated business is no exaggeration.

Even in a report which makes so many recommendations some topics are slighted. The reviewer, for example, is interested in the relation between microeconomic data and the national accounts and, in setting up a program for the next decade, would have placed more emphasis on the need for integration of cross-section and aggregate data. Direct insight often can be obtained into the forces creating movements observed in the aggregate statistics by identifying the relevant decision-makers and interviewing them. Information about who is performing a given type of economic activity is a step toward constructing a satisfactory explanation of why they are taking this action. Distributions of income consistent with the aggregate data are now being published, but for such items as assets, debts, saving, and investment, no such distributions are available.

But specific criticism should not distract attention from the merits of the report. The committee has laid out a broad program for the development of the national economic accounts. One cannot read the report without respect for

the technical competence of its authors and appreciation for the effort devoted
to its preparation. If the proposals of the committee are carried out, the ac-
counts will be greatly improved as tools of economic analysis and our knowl-
edge of the working of the economy correspondingly extended.

<div align="right">JOHN B. LANSING</div>

University of Michigan

The Lognormal Distribution with Special Reference to Its Uses in Economics.
    By J. AITCHISON and J. A. C. BROWN, University of Cambridge, Depart-
    ment of Applied Economics, Monograph 5. Cambridge: Cambridge Uni-
    versity Press, 1957. Pp. xviii, 176. $6.50.

The central limit theorems of mathematical statistics are almost magical in
the way that they bring order out of chaos, and statisticians have been quick
to use them to rationalize basic harmonies observable in an apparently unruly
world. By means of the theorems, it has been shown that almost regardless of
what a population is like, for large samples withdrawn from it, nearly all of
our common statistics are approximately normally distributed. In addition,
they have been used to show that, if the right conditions hold, populations
themselves will be normally distributed. But each of these propositions has a
parallel. The theorems lead to the conclusion that populations determined by
other conditions, often more plausible, will be distributed log-normally (i.e.,
the natural logarithms of the values of the populations will be distributed
normally). And though most common statistics are asymptotically normally
distributed (perhaps in part this is why they are common!), the theorems
show that certain interesting statistics are asymptotically log-normal. Unfortu-
nately, these latter propositions have received too little attention. The mono-
graph under review, the fifth in an excellent series, will provide well-deserved
publicity for a neglected distribution. A scholarly account is given of the
research done on the log-normal distribution since it was originally espoused
by Galton, McAlister, and Kapteyn, and its usefulness in present-day research
is documented. More than that, the monograph makes independent contribu-
tions to our knowledge.

There is almost no estimating problem in connection with the log-normal
distribution which is not explored in this book. The literature has been combed
for methods of estimating parameters of almost any conceivable kind of log-
normal distribution, and for almost any form of sample data. Consideration is
given to the simple two-parameter and the more unusual three- and four-
parameter distributions, and the probit analysis distribution; and the grouped-
data, ungrouped-data, censored, and truncated cases. The standard estimating
methods are appraised for each estimating problem in terms of the often con-
flicting goals of statistical efficiency and ease of computation. The large-sample
properties of these standard methods are compared when possible.

The unusual feature of this part of the book is the attempt by means of
Monte Carlo sampling to examine the small-sample properties of the different
methods, and to compare the graphical method of estimation with the more
conventional ones. In view of the extreme care the authors have displayed
in the rest of their book, it is surprising that they were emboldened to commit

themselves here on the relative merits of the different methods for small samples. One would expect a priori that the method of maximum likelihood would be better than the other methods. The small number of Monte Carlo samples and their mixed character precluded the possibility of satisfactorily testing this hypothesis, however, and as a matter of fact, no tests were made of the significance of the observed differences. A few informal tests made by the reviewer showed that, in at least some cases, the differences were not at all significant. About the only important conclusion that can be substantiated from the Monte Carlo sampling data is that the graphical method, which is so easy to apply computationally, seems to compare favorably with the other methods.

As electronic computers become available more generally, economic models used in empirical investigations will be freed of the restrictions imposed by computing difficulties. Increasing the flexibility of our models to allow for nonlinearities and special stochastic properties is certain to make them more realistic. Such increased flexibility is needed for example to take account of threshold effects. The probit model, long standard in biometry, is designed for just this purpose. The various forms of the probit model are now being used only occasionally in econometric research, but they undoubtedly will be used much more frequently in the future. Though the log-normal distribution does not necessarily figure in probit analysis, it usually plays a prominent role, so probit analysis and applications are described in detail in this book. Researchers in this area will be particularly grateful to Aitchison and Brown for their work on the convergence problem. A difficulty arising in probit analysis is finding initial values in the iterative estimating procedure which will be certain to lead to convergence to maximum likelihood estimators. A happy result of the authors' look at this problem is the tentative conclusion that convergence can be expected even for initial values unreasonably far from the maximum likelihood estimators.

Examples are given of uses to which the log-normal distribution has been put in a number of different disciplines, but detailed attention to substantive applications is reserved for two areas of economics. The subtitle of the book is justified by the chapters on size distributions of income and on analysis of consumers' behavior:

The log-normal distribution meets fairly well the quite sensible criteria the authors suggest for a specific functional form for the income distribution. The empirical fits presented are reasonably representative of what one finds in working with survey data. The log-normal distribution seems adequate for most analytic purposes, but if a high level of precision is required (e.g., for estimating income tax revenues) it should be used with caution.

The relationship between household expenditures and income is discussed in terms of a multiple threshold model. It is assumed that a household facing given prices has a set of income thresholds for each good which determines how many units of the good it will buy. By aggregating the market behavior of groups of households, log-normal "pseudo-Engel curves" are derived which describe average consumption at various income levels. In this formulation there is no ambiguity about where the apostrophe should be placed in the

expression "consumers' behavior." While there is an underlying theory in the background to describe the behavior of individual households, the empirical results obtained by utilizing the model are to be interpreted strictly in terms of group behavior.

A question which the authors did not take up—but, after all, they had to draw the line somewhere—is: What are the consequences of applying to income distributions statistical inference procedures which require the assumption of normality? Skewness in a population makes such procedures statistically inefficient and leads to biases in the selection of critical values. Though it can be shown that for large enough samples these effects are unimportant, statistical theory provides no guidance as to just how large the samples must be. The question is of some interest because the saving in computation is substantial if normalizing the distribution by transforming to natural logarithms can be avoided. A few simple calculations based upon formulae given by the authors show that, for many income distributions with which one is likely to come into contact (where the coefficient of variation is less than, say, one-third), the loss in statistical efficiency is insignificant. A sampling experiment performed by the reviewer (300 samples for each of a number of sample sizes and for V equal to .3 and 1.3) shows that for samples larger than 40, the biases will be negligible. For more skewed income distributions, the sample size must be somewhat larger for this to be true.

The number of typographical errors in the book is surprisingly small, but attention should be called to one which could be costly to a person setting up an electronic computer program for probit analysis on the basis of the constants provided on page 71. The constant d, erroneously reported there, should be .2316419. A researcher at the Cowles Foundation discovered this mistake the hard way!

<div style="text-align: right">ROBERT SUMMERS</div>

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Economic Systems; Planning and Reform; Cooperation

Mengenplanung und Preisplanung in der Sowjetunion. By HANS HIRSCH. Veroeffentlichungen der List Gesellschaft E. V., Series B: Studien zur Oekonomik der Gegenwart, Vol. V. Tübingen: J. C. B. Mohr, Paul Siebeck, 1957. Pp. xii, 195. DM 24, 50.

This study attempts to summarize the results of Russian and western studies of Soviet economic planning from a theoretical point of view. Hirsch first deals with planning of quantities, that is, allocation in physical units, and with "financial planning" or allocation in value units. He then discusses their interaction with particular reference to the delegation of authority to make allocation decisions, and finally presents certain principles of price planning in the USSR.

The planning of quantities is, according to Hirsch, a balancing out of "quantities" demanded by the "leading links" of Soviet industries as determined by "party directives," on the one hand, against the production capabilities (including stocks and foreign trade deliveries) determined by rates of

aggregate output, on the other hand. Thousands of incommensurable commodity balances could presumably be reduced to comparable units by the functioning of money as a standard of value (*Evidenzfunktion des Geldes* in Hirsch's terminology). Hirsch argues, however, that "the quantitative ranking order of ends and goods cannot as such be translated into a financial system of values," either by an individual consumer or by a central planner (p. 74). This thesis is supported by three propositions. In the first place, there are no "quantitatively fixed relations of magnitudes in the quantitative ranking order which the consumer sets up with respect to his ends and goods as indicated in the price system." In other words, while the consumer measures the "economic importance" of goods and services ordinally (indifference, more or less important), the price system measures the "economic importance" cardinally. Secondly, while "the prices reflect at best the importance of the marginal unit (the 'marginal utility') of a good," the consumer's "ranking order is essentially related to the intramarginal region" (p. 71). Thirdly, whenever the demands are inelastic, "the prices do not really indicate the order of ranking of goods even at the margin" (p. 72). The prices reflect then the variations in the marginal costs of production rather than in the marginal utilities of consumers.

Hirsch failed to emphasize that for most purposes it is sufficient if relative prices correspond to marginal rates of substitution. His analysis lacks discussion of consumer's and producer's rents which supposedly equate "the economic importance" of the marginal unit to all the other intramarginal units. On the other hand, the concept of consumers' surplus itself has been criticized, particularly with respect to interpersonal comparisons of marginal utility. This fundamental difficulty appears to have been recognized by Hirsch when he states that "a consistent coordination of all choices is only then possible when one head can still review the entire region of coordination, comparing its elements in the imagination" (p. 75), but that "the determination of ends by the state presents a coordination problem of such magnitude that it infinitely exceeds the imaginative and coordinative power of any one head" (p. 76). Hirsch cuts the Gordian knot by a practical proposition, namely that the various ends (and presumably also the means) should be aggregated by the elimination of refinements until "the remaining assortment of ends can be reviewed by one head carrying out the highest coordination" (p. 76). The decisions arrived at by this "one head" in charge of the state affairs should then be dictatorially imposed on subordinate agencies, which are in turn permitted only to introduce certain refinements within the respective capacities of their "heads."

This theorizing based on political and administrative experience fails in meeting the economic issues. Hirsch argues that "the financial magnitudes obviously arise as a result of decisions made with respect to quantities in the choice of ends" (p. 80) and, therefore, the central authority is justified in disregarding prices as "regulators of quantities" (p. 81). A central planner allocates physical quantities very much in the same way as a private consumer or a business firm, that is, according to Hirsch, without reference to prices. Hirsch ignores the important role of prices used as weights in the balancing out of quantities by a consumer, a business firm, and a central planner. The

problem of mutual and simultaneous determination of prices and quantities is not explicitly discussed. Considerable doubt arises when Hirsch finally concludes that an allocation in physical units is theoretically consistent with a "financial system of values," but that such a mixed system has not been "consciously" realized in the USSR (p. 91).

All these and various other abstract discussions, whatever their theoretical importance, appear quite divorced from the "principles of price planning" presented in the last chapter, where the relevant writings of Baykov, Dobb, Jasny, Schwartz, and a few other authorities are haphazardly summarized. On the basis of these summaries, Hirsch purports to explain the alleged "irrationality" of Soviet prices by reference to the dynamic nature of the Soviet economy in three respects: (1) the structure of costs and productivity relationships, (2) the structure of demand, and (3) "the independent dynamics of the financial system" (inflation). But even within the static framework of economic theory, the Soviet price system contains, according to Hirsch, certain rational features such as "a direct regulation of prices in accordance with scarcity relationships" (p. 185). Prices are increased in order to encourage production and to discourage consumption, and vice versa with price decreases. Hirsch also argues that there are "considerable tendencies" towards a "rational price system," at least in the pricing of producers' goods, though Hirsch carefully modifies his findings by numerous qualifications.

This study deals with many important questions. Undoubtedly, it will be welcomed by specialists in the field of Soviet economics as a useful summary of research materials, despite its cumbrous style and many vague statements. The reader will have considerable difficulty in finding an answer to the crucial question which Hirsch considered the core of his study, namely the "why" (rather than the "how") of planning. Hirsch has failed to define the term "planning" as well as many other important concepts used throughout his study, leaving it up to the reader to infer various meanings from his terminology. This omission naturally detracts from the value of his contribution to our understanding of Soviet economic planning.

GEORGE J. NOVAK

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Ekonomika promyshlennosti SSSR. (Economics of industry of the USSR.) Academy of Sciences of the USSR, Institute of Economics. Moscow: Gosudarstvennoye Izadetl'stvo Politicheskoy Literatury, 1956. Pp. 463. Rbl. 10.20. [Referred to below as Book I.]

Ekonomika sotsialisticheskikh promyshlennykh predpriyatiy. (Economics of socialist industrial enterprises.) Moscow: Gosudarstvennoye Izdatel'stvo Politicheskoy Literatury. 1956. Pp. 471. Rbl. 10.50. [Referred to below as Book II.]

The two books under review mark the most recent progress attained by Soviet economists in the explanation of the economics of the industry as a whole and of the individual firm in particular in their economic system.

Book I, however, is more descriptive than analytical. It describes the planning of Soviet industry in great detail, it lists a great many of the interesting economic ratios and balances which comprise the plan; and yet at

the end it only comes to the general conclusion that they are important, leaving the reader without knowledge as to why or in what way they are important. It states that before new plans are elaborated the fulfillment of old plans is analyzed (p. 83); but it says nothing as to how this is done. It speaks convincingly about the necessity for a "rational location of industries" (p. 165), but it does not explain how it is achieved in practice. No doubt Soviet decision-makers on the industry level do use some methods of economic analysis in practice. Numerous quite elaborate articles in technical and engineering journals bear witness to this. Yet it seems that academic economists have not succeeded in translating this experience into a meaningful economic theory that could serve as a guide to action and to explanation.

There are some exceptions in Book I, however. The book goes, though to a limited extent, into an analysis of capital depreciation (p. 233) and the acceleration of the turnover of variable capital (p. 266); it provides interesting analytical indices of the comparative costs of different fuels (p. 306), etc. It also provides for the first time some revealing data on such things as, e.g., the number of labor shifts in various industries, on the computation of the piece-rate wages, and the structure of some prime costs.

Book II is much better. In the preface to this book it is stated that it was written by the Faculty of Soviet Economics at the Supreme Party School of the Central Committee of the C.P.S.U. It is slightly surprising that, unlike the above text of the Institute of Economics of the Academy of Sciences, this text prepared by a purely political school shows a remarkable awareness of genuine economics.

Book II consists of 13 chapters, partially overlapping those of Book I. They embrace the following topics: the development of Soviet industry; the management of individual enterprises; planning on the enterprise level; fixed capital and its productive capacity; variable capital and supplies; the organization of input of labor; labor productivity; wages; costs, prices, and profitability; economic calculations; enterprise finance; bookkeeping and accounting; the analysis of economic activities of the firm.

The microeconomics of the Soviet industrial firm has never before been described in such clear and precise terms as in Book II. Written probably for the instruction of party laymen, the book provides many practical suggestions and explanations of how and why the firm has to economize. Because of its simplicity and clarity the book may be accessible to interested Western students with but a limited knowledge of Russian.

Certainly this book does not explain everything, but it does convey the impression that the microeconomics of a firm under the Soviet system of total state monopoly is perhaps somewhat less complex than that of its counterpart under competitive capitalism. Soviet microeconomics is limited by a given plan: output quotas, prices, and costs are usually fixed from above. The firm can economize only by reduction of inputs and/or an increase in productivity. Book II describes a series of analyses of surpluses and economies resulting from different opportunities for manipulation of inputs and outputs. Of particular interest is, for example, the analysis of the situation in which the government permits an increase of prime costs in the producing firm, if, as a result of the improvement in its product, the consuming firm would

realize an economic surplus larger than the accretion of costs to the producer
(p. 107). Of similar interest is the thesis that the projected capacity of an
enterprise under construction should not be limited by the consideration of
the presently existing bottlenecks, but instead should aim at the maximum
feasible under the terms of long-run costs (p. 126). The reader of Book II
will find many thought-provoking ideas, naturally sometimes contradicting
established principles of economic analysis in the West, but nonetheless worth
pondering and wondering about.

<div align="right">VSEVOLOD HOLUBNYCHY</div>

Columbia University

Economic Planning by Programme and Control in Great Britain. By GILBERT
    WALKER. New York: Macmillan Co., 1957. Pp. v, 175. $3.75.

The stimulus for this book was an invitation from the Harvard Summer
School to offer a series of lectures on economic planning in Great Britain.
Gilbert Walker, professor of commerce in the University of Birmingham, set
himself to enquire what the plan was and how it was operated until the change
of government in 1951. He succeeded.

Beginning in true British fashion, the author traces briefly the major events
exciting a mounting desire for planning. These include economic development
accompanied by periods of serious unemployment, the end of dominion by
Say's Law, and ideas for "social improvement." With this background, the
reader is carried into theoretical and statistical foundations for program and
control through a study of income flows, quantification, and social accounting.

"The British plan," Walker believes, "grew out of a determination, wide-
spread during the war, that the second peace should not be marked by the
slump, depression, and unemployment which had so marred the first." Experi-
ence during the second world war is examined by describing conditions related
to dollar scarcity, temporary relief through Lend-lease, insufficient labor sup-
ply, and endeavors to control menacing inflation. The manner in which the
economic system operated during these and other wartime gyrations is de-
scribed by reporting the activities of central agencies attempting to solve the
many difficult problems.

Fear of a return to prewar stagnation was lively in Britain as victory ap-
peared on the horizon. History did not repeat itself. Rising prices, shortages,
and an apparently chronic deficit in the balance of payments—not unemploy-
ment—were the dragons faced in 1945. The British were ready with a plan;
they adjusted the plan to meet unexpected situations. Walker describes these
adjustments by presenting the British design for "a system of democratic
economic planning."

A chapter sets forth the manner in which a "democratic" program, a man-
power budget, and an investment program were drafted and arranged. The
mechanism for controling imports is examined in a chapter of its own.
Throughout his text the author follows a pattern of stating in succession the
character of a threatening problem, describing the operations of organizations
for preventing disorder, and offering pertinent comments of his own.

Much was accomplished during the war because, Walker believes, military purposes were paramount and recognized as such. The postwar system was soon in trouble because it had no engine to move the people toward a chosen objective. The elaborate postwar machinery for program and control has gone. Only "the most important controls" remain, a visible and constant reminder of Britain's orientation toward overseas trade.

One interesting and engaging note closes the book: "Planning for stability of income and continuity of employment in peace has perhaps this, and this only, in common with planning for a maximum of military output in war— that in each case the problem transcends national frontiers and combined policy undertaken by all the free nations is an essential requirement for success."

This short study is an excellent supplement to W. Arthur Lewis' *Principles* or J. E. Meade's *Planning*. The author has examined and summarized numerous white papers, surveys, and detailed studies of British wartime and postwar operations. A pictorial history of the emerging plan is a most attractive feature for readers with abiding sentiment. It begins with Lloyd George and Winston Churchill carrying the People's Budget in 1909; it portrays the hunger marchers in 1933; it presents Lord Keynes, Godfather of the Plan, as depicted by David Low; it shows Lord Beveridge, Father of the Plan, speaking at Caxton Hall in 1943. Needless to say the pictures delightfully outline some of the story that follows.

EDWARD G. NELSON

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Business Fluctuations

Inflation: A Study in Economics, Ethics, and Politics. By G. L. BACH. Providence: Brown University Press, 1958. Pp. vi, 103. $2.50.

Prosperity Without Inflation. By ARTHUR F. BURNS. New York: Fordham University Press, 1957. Pp. ix, 88. $2.00.

The Control of Inflation: An Inaugural Lecture. By J. E. MEADE. Cambridge: Cambridge University Press, 1958. Pp. 52. $1.00.

The formulation of a policy to maintain prosperity would seem, at first glance, to be a relatively simple matter since it appears to be to almost everyone's advantage that depression be avoided. In principle, there are very few toes to be stepped on by antidepression measures, and since there are no obvious conflicts of interest involved, the problems to be handled would appear to be simply technical ones; at least this was the common impression until a few months ago.

The control of inflation has, however, been generally regarded as far more difficult. Not only is the objective itself open to some question—for it is generally agreed that mild inflation does at least some good—but there is the additional problem that measures to check inflation are likely to run counter to at least the short-run interests of some of the most powerful sectors of the economy, and hence are likely to be strongly resisted. Most economists in

consequence seem to feel that prices are very likely to increase, assuming we
can avoid prolonged depression.

Reading these volumes—which record lectures delivered between February
1957 and March 1958—certainly does not dispel the doubts that economists
have entertained about the prospects of containing inflation. While the three
writers, or speakers, urge a policy of price stabilization, and set out at least the
broad lines of policy to assure it, they are very cautious about promising suc-
cess. In my judgment, they make a convincing case for the view that we are
faced with secular inflation, and we should be well-advised to consider means
for rendering the experience tolerable.

They are united in their opposition to inflation—even to a gradual increase
in the price level. Their strongest objection is to the inequities that inflation
creates, especially for the aged, but also for other creditors and those whose
incomes are relatively fixed by long-term contracts or other institutional
arrangements. Burns and Meade are also concerned about the likelihood of
difficulties in the inflation-ridden country's balance of international payments
—a concern which clearly is unnecessary if inflation is widespread instead of
being confined to a single country. Bach stresses the degree to which inflation
distorts measures of profit, and so stands in the way of proper management
decisions. His analysis of the consequences of a gradual inflation is the most
detailed, but he too in the end concludes that the greatest objection to infla-
tion is that it makes for inequities.

Their analyses of the causes of inflation are also essentially similar. Exces-
sive demand and rising costs (or "sellers' push") are the main causal factors.
The former can be avoided by a suitable combination of monetary and fiscal
policies: Meade in this connection urges that attention be paid to the possi-
bility of using some kind of levy upon personal incomes which can be
promptly and frequently altered as demand becomes excessive or deficient.
The pressures of rising costs can be at least minimized by keeping demand
from becoming too high; by encouraging advances in productivity; by pro-
moting restraint in profit mark-ups and in wage negotiations: Meade is hope-
ful that the publication at intervals of an official estimate of the average wage
increase that would be compatible with price stability and full employment
would prove helpful. Meade also suggests the desirability of increasing factor
mobility in order to raise the employment level that is consistent with price
stability; while Burns urges that the Employment Act be amended to include
"reasonable stability of the consumer price level" amongst its objectives, not
so much in order to remind public officials of their obvious duty, but rather
to check the public's expectations that prices will continue to rise.

These numerous prescriptions are not put forward in a doctrinaire spirit
or indeed with unduly optimistic statements of confidence about their efficacy.
Their sponsors are too keenly aware of the difficulties of timing, of recent
institutional developments that may minimize their effects (see especially
Burns on monetary controls in this connection), and of the problems of secur-
ing assent. But it is difficult to imagine a new wonder-drug that could promise
more.

But, do they promise enough? I think not: certainly not, if we are going

to avoid depression. It is hard to see why wages and profit margins should not be pushed up faster than productivity if times are good. It is no answer to threaten unemployment if prices rise. The profit margins that go up too fast, or the out-of-line wage increases that are secured, are not necessarily going to bring a cut in sales or unemployment to the businessmen or wage · earners who enjoy them. The penalties may be levied elsewhere. In short, the price of price stability may turn out to be a level of activity much lower than society will or should tolerate. At least, none of the authors dispel this fear. Perhaps we will have to learn to live with inflation after all.

One final word: Burns discusses not only policy for inflation but also measures to prevent recession. In a way his words, which are éloquent and authoritative, make melancholy reading, for we have clearly learned so much and, at least recently, been willing to apply so little. He stresses the point that if we hope for success in controlling inflation in the future, we must take prompt and vigorous steps to deal with recession whenever it appears. Failing this, we are scarcely likely to secure the support of labor or business for the austere anti-inflation policies that will be needed.

These little books can be strongly recommended. They are persuasive and clear; and certainly no economists can speak with greater authority than their authors.

<div align="right">LORIE TARSHIS</div>

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Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Financial Intermediaries in the American Economy since 1900. By RAYMOND W. GOLDSMITH. National Bureau of Economic Research Study in Capital Formation and Financing, No. 3. Princeton: Princeton University Press, 1958. Pp. xxxv, 415. $8.50.

This pioneering study of the changing role of financial intermediaries in our economy provides a wealth of data on the growth in total assets of these intermediaries by type of institution from 1900 to 1952 (with some information back to 1850); on changes in the structure of their assets and liabilities; and on their shares in the financing, and in the channelization of saving, of different economic groups. Much of the basic data for these purposes were obtained from Goldsmith's earlier work published in *A Study of Saving in the United States.* While assets and liabilities data for virtually all individual types of financial intermediaries were available previously, they had not been put together in a consistent set of accounts and systematically related to the saving and financing of the ultimate suppliers and users of funds in the economy. In addition to the provision of an articulated set of accounts focusing on the interrelationships of financial intermediaries and the rest of the economy, which is probably the major contribution of this study, Goldsmith attempts to contribute to "a much needed general theory of financial institutions." Finally, he presents a wide variety of specific data on the size and geographic distribution of such institutions.

To summarize briefly the major findings of this study, the relative impor-
tance of financial institutions as a group is shown to have increased greatly
over the past century. Probably since 1850 and at least since 1900 their assets
grew at a rate of 6.7 per cent annually, which is faster than other relevant
economic magnitudes such as the national income, national assets, wealth or
saving. There is some evidence that this rate of increase has been diminishing
in recent years. Currently, or more accurately in 1952, the assets of financial
intermediaries represented 20 per cent of the total of national assets, 40 per
cent of intangible assets, and—though this is a comparison Goldsmith doesn't
think highly of—45 per cent of national wealth. Over the period covered by
the study, the share of the banking system in the assets of all financial inter-
mediaries has fallen, that of insurance organizations has risen—in recent years
almost entirely due to the rise of government and private pension funds.
Most of the funds raised by financial intermediaries have at all times been
furnished by nonfarm households; the funds supplied by financial intermedi-
aries are more evenly distributed among the major economic groups—with
little change in the proportion going to nonfarm households over the last 50
years, a substantial increase in the share going to the federal government, and
pronounced declines in the shares of business and agriculture. During the first
half of the century, financial intermediaries supplied slightly more than half
of all external funds absorbed by other economic groups, with an even higher
proportion in the last 20 years: they have supplied two-fifths of all external
funds and one-fourth of total net financing of nonfinancial corporations.

The study also indicates that in most groups of financial intermediaries a
small number of large institutions account for a substantial fraction of the
aggregate assets of all institutions of the same type and that the degree of
concentration has generally increased over the last 50 years. The geographical
density of financial intermediaries (where density is defined in various ways)
is highest in the Middle Atlantic states and lowest in the South, with little
change in over-all density over the last 50 years but some lessening in regional
differentials.

Though Goldsmith's attempt to set up the general outlines of a theory of
financial institutions is full of interesting observations on trends in various
"immediate" and "ultimate" determinants of the role of financial intermedi-
aries in the national balance sheet, the theoretical structure seems to me on
the whole to be inadequate. The analytical apparatus used, which is handled
with consummate skill, is primarily an attempt to describe how financial inter-
mediary magnitudes have varied in relation to other national magnitudes
rather than to analyze the basic supply and demand equations involved (relat-
ing to the asset and liability preferences of the various sectors). Goldsmith
starts by breaking down the ratio of financial intermediary assets to total
national assets—which of course is equal to the corresponding ratio for liabili-
ties and equities—into the sum of four "basic" immediate determinants of the
role of financial intermediaries: the ratios to total national assets of (1) non-
metallic money, (2) insurance reserves, (3) other liabilities of financial inter-
mediaries (excluding money and insurance reserves), and (4) financial inter-
mediary equity (or net worth). The last two ratios he treats differently from the
first two on the ground that there is "no competition for funds between financial

intermediaries and other debtors for these [first] two types of liabilities. . . . In the case of . . . other liabilities and equity, financial intermediaries . . . have to compete directly for them with other debtors and issuers" (p. 19). As a result, the ratios of other liabilities and equity of intermediaries to national assets, (3) and (4), are each expressed as the product of the share of financial intermediaries in such liabilities or equity and the ratio of the total of such liabilities or equity to total national assets. It is not clear why it is assumed that other liabilities of financial intermediaries, largely time deposits, can be considered competitive with other liabilities of nonfinancial groups, largely bonds and accounts payable. It may be, as Goldsmith seems to implicitly assume, that demand deposits (or insurance contracts) are less competitive with time deposits than the latter are with bonds, though this might be argued. However, it is difficult to understand how demand deposits can be considered less competitive with time deposits than the latter are with accounts payable.

After breaking down the ratio of financial intermediary assets to total national assets into the four main components indicated above, Goldsmith discusses the immediate determinants of each component ratio. For this purpose, he factors (i.e., expresses as a product) the ratios of each of the four major types of financial intermediary liabilities (and equity) to total national assets into a number of subsidiary ratios, resulting in a number of interesting findings but also raising a number of questions. In view of space considerations, I shall comment here briefly only on his treatment of the first ratio—the share of money in the national assets.

In introducing the analysis of the money-national assets ratio, Goldsmith states that "the volume of check (demand) deposits of commercial bankers . . . is determined primarily by the public's demand for cash" (p. 21). It is not clear why he seems to assume that changes on the supply side are largely irrelevant to an explanation of historical fluctuations either in the volume of money or in its relation to national assets. Monetary authorities can and do markedly influence the volume of money and, while it is true, as Goldsmith points out, that prices, interest rates and the national income may change in the re-establishment of equilibrium between the supply of and demand for cash, there does not seem to be any good reason for assuming that at the new equilibrium position the ratio of money to national assets remains the same as formerly.

Goldsmith factors the ratio of money to national assets into four subsidiary ratios: (1) money to national income; (2) national income to equity of ultimates (households and governments); (3) equity of ultimates to total equity of all units; and (4) total equity to total national assets. The first factor (Cambridge k) has of course received most attention from monetary theorists and turns out to have the greatest influence in the observed rise in the ratio of money to national assets. It is not clear, however, why the other three factors should be expected to be particularly useful. The argument that the second ratio can be transformed into the "economically significant" capital-output ratio seems rather tenuous since the ratio involved is essentially not technological but rather a value ratio which depends among other things on the public's demand for assets of fluctuating value. For example, if income remains reasonably stable but corporate stocks go up markedly in price, as in

the late 1920's, the ratio of national income to equity of ultimates will go down even though the capital-output ratio may be unchanged in any economic sense.

Goldsmith notes that while the immediate determinants of the share of financial intermediaries in national assets can be (and have been) expressed "in a small number of fairly well defined and measurable relations" (p. 36), it is possible only to identify some of the ultimate economic determinants. These he designates as asset price movements and a set of national balance-sheet ratios—viz. the share of nonfinancial business in the national assets, the "financial interrelations" ratio of intangibles to tangibles, the capital-output ratio, the "deadweight" debt ratio (measured as the ratio of the balance sheet deficit of the federal government to total national assets), the short-long debt ratio, and "layering" ratios for financial intermediaries and for non-financial business. It might be questioned in what sense some of these determinants can usefully be regarded as "ultimate," totally apart from the point which Goldsmith makes that they are not necessarily independent. Thus the short-term debt ratio is simply a statistic which, depending on how short-term debt is defined, may be positively correlated with the share of financial intermediaries in national assets, but this ratio hardly seems to be a useful theoretical construct for the purpose used since it is not directly related to the behavior of any homogeneous set of transactions or transactors. Similarly, to explain the share of financial intermediaries in the national assets by the behavior of the financial interrelations ratio does not answer, or address itself to, the "ultimate" question of the determinants of the composition of assets and liabilities (which governs the position of financial intermediaries in the economy). Goldsmith finds that three of his ultimate variables—the financial interrelations ratio, the deadweight debt ratio and the movement of asset prices—are statistically most important in explaining past trends in the share of financial intermediaries in national assets.

The study presents an impressive array of other statistical data and analytical ratios relating to financial intermediaries which cannot be covered in this review. Some questionable conclusions drawn from the material presented, however, should be pointed out.

After noting that "The supply of funds . . . is an essential if not the primary economic function of financial intermediaries . . . [which] can be understood only through comparison to the volume of internal financing and of external financing from other sources" (p. 180), Goldsmith makes a number of references to the increased importance of internal financing in the structure of financing throughout the period. In discussing the financing of all nonfinancial corporations, he states "external financing has been somewhat less important since World War II in relation to internal financing than before 1929 if capital consumption allowances are regarded as part of internal financing. The decline is much more pronounced if internal financing is limited to retained earnings" (p. 221). Later, in his discussion of financing the American economy as a whole, he indicates that "The reason for the diverging trends in the share of financial intermediaries in total and in external financing is, of course, the increase in the proportion of internal financing, gross or net, in total financing.

This change in the relationship between internal and external financing is a basic development in the American economy . . ." (p. 302).

Goldsmith is, of course, repeating what many financial analysts believe to be true, but the figures he presents do not confirm these statements. The two sectors in the economy which normally utilize the great preponderance of internal and external sources of funds are nonfarm households and nonfinancial corporations. For the former there is no difference between 1946-49 (the latest period for which such data are shown) and the earlier 1900's in the ratio of (1) total internal sources (including capital-consumption allowances) to total sources of funds, and (2) net saving to total sources of funds. For nonfinancial corporations there is a rise in both ratios from the turn of the century to 1946-49, but for the later years 1950-52, which Goldsmith obtains from the Department of Commerce, there is no evidence of a secular increase in these ratios over the preceding half century. Moreover, subsequent Commerce data indicate that the first ratio has risen only slightly from 1950-52 to 1956-57 (and is apparently somewhat below its level at the turn of the century) while the second ratio has declined markedly (and is also below its level at the turn of the century).[1] The Goldsmith and Commerce data show about the same relation of internal to total corporate financing in the overlap years 1946-49.

It is possible that my interpretation of the aggregate data on corporate financing simply reflects the inadequacies of the estimates. There does not, however, appear to be much basis in these data for reference to structural changes or basic developments in the American economy in the proportion of internal to external financing. Probably the most sensible way of testing for such changes is to study intensively a sample of corporations over time. It is *possible*, even if a re-examination of the aggregate data should confirm the absence of a secular increase in the relative importance of internal financing, that this result may reflect a decline in the relative importance of small corporations largely dependent on internal financing associated with a rise in internal financing for large, publicly held corporations.

There are other parts of the study in which Goldsmith, although presumably covering the period 1900-1952, seems to address himself to the period ending with 1949, the termination date of his earlier *Study of Saving*. For example, he refers to the "contribution of financial intermediaries through the acquisition of common stock" as being "small in relation to total common stock financing" (p. 256). Actually, this is true for the period 1901-49 as a whole when the ratio of financial-intermediary net purchases to total net sales of all stock (common and preferred) was 18 per cent, but for the more recent period 1950-52 according to figures he presents the ratio was 68 per cent. Moreover, at the time his study was written, there was every reason to believe that institutions would continue to absorb a major proportion of new common stock offerings, and data for the past few years indicate that well over half of such offerings have been absorbed by financial intermediaries (primarily by private pension funds and open-end investment companies).

[1] *Surv. Curr. Bus.*, Sept. 1957, p. 7 and *Economic Report of the President*, Jan. 1958, p. 183.

Though it is not intended to criticize an author for not doing what he has not set out to do, it may be useful to point out to the prospective reader of this volume that it does not cover such matters as portfolio policies, control policies in relation to portfolio companies, the nature of and instrumentalities of control over financial intermediaries themselves, etc. Goldsmith does make the interesting observation that while "inequality in size distribution (i.e., the degree of concentration) has in all likelihood increased in most branches of financial intermediaries over the last half century . . . it was more pronounced in the 1920's and during the Great Depression than before or after." However, it is difficult to evaluate this statement (at least without reference to an appendix which is not part of the published study) since from many points of view the most interesting over-all statistics indicating the potential concentration of power of financial intermediaries relate to the proportion of claims and equity securities of different sectors in the economy which a relatively small number of institutions own (or otherwise control). These are different from statistics indicating the internal degree of concentration of ownership of assets within the financial-intermediary sector.

In conclusion, I might note that this is not an easy book to read (nor, I suspect, is this review). Thus a careful reader will find that for definitions of many essential concepts, particularly those relating to the national balance sheet, it will be necessary to refer to Goldsmith's *Study of Saving*. (In view of the heavy dependence on his earlier work of some of the less firmly grounded estimates presented—e.g., those applicable to unincorporated business—I have excluded a consideration of estimating errors from this review.) A number of findings cited in the present study are supported in appendixes which are not included in the book. There are some proofing errors which are disconcerting (e.g., percentage increases on p. 133 which are understated by a 100 factor and "internal" for "external" financing on p. 247), but more troublesome are a number of statements which either seem inconsistent with the data or inconsistent with other statements. To illustrate the latter, it is stated that the share of financial intermediaries in the external financing of nonfinancial corporate business "is higher for short- than for long-term financing," but the table seems to imply the contrary is true (p. 184). One also finds on one page (p. 218) that "probably the share of financial intermediaries in total external financing is somewhat higher (outside of railroads and public utilities) for small than for large corporations," but on another (p. 258) the apparently correct statement that "It is probably safe to say, however, that the share of financial intermediaries in total external financing is substantially higher for large than for small nonfinancial corporations other than railroads and public utilities."

In spite of these minor flaws and the other limitations previously mentioned, however, the study is an imaginative, painstaking and useful work which is a must for any scholar interested in financial intermediaries. There are few persons in the profession who could attempt, much less achieve, this masterful treatment of a complex and important body of data.

<div align="right">IRWIN FRIEND</div>

University of Pennsylvania

Relations Between the Central Banks and Commercial Banks. Frankfort
Main: Fritz Knapp, 1957. Pp. 209. DM 24,—.

This book, which is composed of a series of lectures delivered by leading
international economists and bankers at the Tenth International Banking
Summer School in Garmisch-Partenkirchen, Germany, opens with a discussion
of monetary policy. It emphasizes those changes which have occurred since
1948, and examines the influence of the (1) discount policy, (2) reserve re-
quirements, (3) credits granted by commercial banks, (4) rediscount quotas,
(5) open-market policy, and (6) policy to be followed by public authorities
when depositing funds. The situation in Italy, Switzerland, Austria, France
and the Benelux countries is outlined, and the effectiveness of these various
monetary policies is evaluated. The authors in general believe that the policies,
for the most part, have brought about the desired results.

The reader is given a good picture of the degree of development of the
money market—or lack of it—and the effects such development has on the
economy. In Germany it is so uncertain a source of funds that no responsible
bank manager will allow his institution to become entirely dependent on it.
In London where it has two centuries of history behind it, the money
market is a compact, informal organization where the size of Great Britain
itself makes such a system easy to operate. This is in contrast with the overseas
system where it has to be more formal.

In discussing monetary management in the United States, E. Sherman
Adams of the American Banking Association, points out that the main objec-
tive here is the same as in most countries: to keep the value of money stable;
one notable difference is its preoccupation with domestic conditions. In for-
mulating their policies the Federal Reserve authorities can virtually ignore the
balance or imbalance of international payments. While other papers had to
concern themselves with exchange controls and quotas, the United States
representative devoted most of his paper to an analysis of the open-market
operations of the Federal Reserve.

Students in this country will be interested in the account of the practice
of financing through securities as found in Germany where one and the same
institution handles all three kinds of business, namely, deposit banking, the
work of the broker and that of the underwriter. Throughout the book abun-
dant examples are found of the tight control exercised over commercial banks
by central banks or governments.

In a world in which monetary policy will continue to play a major role in
the economies, where it will continue to have a major impact upon commer-
cial bank investments, lesser but still significant effects upon bank lending.
and of course, important effects upon rates of return and bank earnings, this
book will serve as a good analysis of the way in which monetary policy
operates.

Each paper is complete in itself, and the lectures as a unit hold together
well. The work is timely, and since the complexities of monetary management
will be with us for some time to come—problems which will tax both the
economist and the banker—these lectures will be of interest to both.

THOMAS J. BRYDE

Iona College

French Banking Structure and Credit Policy. By J. S. G. WILSON. Cambridge, Mass.: Harvard University Press, 1957. Pp. xiii, 453. $8.50.

This well-written book will be equally appreciated by scholars and business-men who wish to obtain a comprehensive understanding of the French finan-cial system. The first part deals with institutions, the second part with the Bank of France and credit controls. In the institutional part, Mr. Wilson, a lecturer at the London School of Economics, fills a gap in the English-language banking literature by giving a concise exposition of the structure of the French banking and financial system, followed by a chapter on each of its compo-nents: The deposit banks (of which the largest four were nationalized in 1945); the investment banks (which frequently retain up to one-third of their underwritings); the discount houses (through which the Bank of France carries out its open-market operations); and a variety of public and semi-public specialized lending agencies and money market institutions with their tier of rediscount facilities. Each of these chapters contains a short historical sketch and a description of every major and medium-sized bank and other financial institution. There is a comprehensive analysis of bank balance-sheet accounts and a great deal of other informative detail, including the names of investment banks that have the best connections with the small regional coun-try banks. A discussion of means available to facilitate the financing of exports concludes this altogether well-balanced institutional survey. Unfortunately this part is somewhat short on analysis of the relative functional importance of the various financial sectors in the total economy.

Part II sketches the evolution of the Bank of France and then gives a lucid explanation of the complex of intertwining relationships between the Bank, the National Credit Council and three Ministries of the Executive, all of which have a voice in the formulation of monetary and credit policies. The last quarter of the book contains a survey of postwar monetary and credit policies together with a discussion of the quantitative and qualitative credit controls that have proven so ineffective in preventing a nearly uninterrupted inflationary spiral. The author shows much insight in arriving at his judicious and well-balanced evaluations of the various credit control experiments, always preceded by clear and painstaking analysis.

Wilson attributes much of the 1945-48 inflationary spiral to the policy of reconstruction, as a result of which investment outlays were out of proportion to savings and foreign aid. During this period, credit controls were essentially of the qualitative type. Large bank loans were subject to prior approval by the Bank of France and the commercial banks were expected to exercise dis-criminating judgment in making smaller loans. The program was ineffective, partly because of the loopholes in the prior-approval program, partly because all self-liquidating loans were entirely exempt from controls, on the fallacious assumption that they were inherently noninflationary. While Wilson points out these and other shortcomings in the techniques of qualitative control, it is doubtful whether any credit controls, no matter how stringent, would have prevented inflation in the absence of a one-time drastic reduction of the war-time-created excess liquidity. The experience of Belgium, Denmark, Germany and Holland seems to demonstrate that the elimination, through currency reform, of the excess money supply is a prerequisite for the re-establishment

of a climate of financial confidence, without which the curse of velocity infla-
tion will superimpose itself on the existing inflated money supply. France's
unwillingness to accept the inequalities of such a tax on liquid wealth brought
open inflation accompanied by far greater inequities which have undoubtedly
contributed to eroding the very roots of the Republic.

After 1948, quantitative credit controls, in the form of rediscount ceilings
and secondary security reserve requirements, were the cornerstone of credit
policy. Wilson says it "was the degree of elasticity required, particularly dur-
ing a period of rapidly rising prices, which constituted the chief weakness of
the ceiling technique" (p. 400). The causal relationship appears to have been
the reverse. The monetary authorities failed to withstand the unrelenting
pressures for upward revisions of the ceilings and, through the ensuing credit
expansion, prices rose.

This is not to suggest that the author is unaware of the fundamental causes
which have plagued the French economy; "For too long France has clung
with an ideological tenacity to a system of small and insufficiently capitalized
units protected by tariffs, quotas, price supports and State purchases of sur-
pluses" (p. 375). Wilson concludes, "the problem is largely political," and can
be solved only by "strong government with widespread support and some
prospect of continuing in office long enough for its plans to mature. . . ."
(p. 375).

STEPHEN F. SHERWIN

Washington, D.C.

Kreditschöpfung und Kreditvermittlung. By HANS HELLWIG. Stuttgart: Curt
    E. Schwab, 1958. Pp. 495. DM 24.

Very rarely is monetary theory enriched by a contribution that is both
attractively presented and challenging in theoretical content. Hans Hellwig's
treatise scores high on both counts.

Its subtitle, "Inquiries into Modern Inflationism" indicates the author's
basic objective. He finds an important root of modern doctrines of inflationism
in the popular idea of fiduciary credit creation by commercial banks. Whoever
accepts this doctrine without qualification, says Hellwig, is prone to accept
unlimited government control over credit and banking. Nationalization of
credit, however, would merely serve the cause of inflation in the same way
as the nationalization of note issue has served it since the evolution of central
banking.

The author analyzes numerous modern variations of the theory of credit
creation. He finds them wanting with respect to their treatment of the banks'
ability to create credit, the limits on credit expansion, and finally the economic
interpretation of the process. He especially finds fault with the modern doctrine
that creation of bank credit is possible up to some multiple of the bank's
reserve position.

According to the author, the credit coefficients which the American literature
on banking is so fond of computing are theoretically and practically worthless.
Such a calculation not only tacitly assumes uniform behavior of all bankers
but also that a bank will extend credit to the extent that its reserves have
increased. In reality a banker's reaction depends on many unpredictable

factors. Some of these are the position of the bank within the system, which again depends on its size, liquidity, field of specialization, etc., the economic situation as seen not only by the banker but also by his borrowers, and finally, the credit policy of the central bank which in turn is influenced by its position in the international system.

Hellwig's investigations lend support to the constant denial of bankers that they can extend credit above the limit that is determined by deposits and their own investments. Indeed this denial should disconcert the theorist for if it cannot be proved that the individual bank creates fiduciary credit it is logical to infer that the banking system, too, is incapable of creating credit.

This reviewer is impressed by the cogency of Hellwig's reasoning, such as the following: It is true, demand deposits, which constitute cash holdings at the ready disposal of the depositor in spite of their juristic character as credit to the bank, formally lend themselves to "duplication" through bank credit. The bank debtor receives purchasing power while the depositor does not renounce the power of disposal over his deposits. But how much additional credit is actually created cannot be determined. For we do not know the part of demand deposits that actually constitutes true savings which would make deposits not only juristic but also economic credits to the banks. Furthermore, in order to evaluate the extent of bank credit expansion we would have to know the credit relations between bank creditors and debtors. If, for instance, the depositors are indebted to their suppliers, and the suppliers borrow the same amounts from the banks, we formally may speak of credit creation, but economically the banks merely mediate between creditors and debtors. Finally, we must bear in mind that extensions of credit that follow repayments of old credits do not constitute credit creation, for such a transaction obviously does not enlarge the volume of fiduciary credit.

Whenever the credit volume of commercial banks is enlarged due to an increase in deposits, Hellwig maintains, under certain circumstances credit expansion may take place. But it by no means follows that an increase in deposits is the result of credit creation. For credit creation does not lie in the nature of commercial banking but may result from the decision of depositors to increase their cash holdings with commercial banks. In other words, commercial banks cannot expand their fiduciary deposits even if they are not subject to minimum reserve requirements. Only the central bank can create almost any amount of money and credit it chooses to create and can cause the well-known ill effects of inflation.

This treatise is, to say the least, penetrating and thought-provoking. It demands careful consideration on the part of all serious students of money and banking, especially since its conclusions run counter to the prevailing theory of bank credit. In my belief, it shatters the doctrine of multiple credit expansion which has won almost unanimous acceptance in Anglo-American literature. And it lays a finger on the ultimate source of all inflationary adventures: the central bank. It is to be hoped that Hans Hellwig's work will soon be available also to the English reader.

<div align="right">HANS F. SENNHOLZ</div>

Grove City College

Die veränderte Währungspolitik und ihre Folgen. By OTTO VEIT. Institut
für das Kreditwesen, Johann Wolfgang Goethe-Universität. Frankfurt:
F. Knapp, 1957. Pp. 182. DM 13,20.

This study touches on a great variety of subjects of much interest. In an
introductory chapter, some major changes in modern economies of the Western
world are considered, such as the growing preponderance of money creation
by the banks, the increasing public share in the national product, and the
diminishing impact of the interest factor on the calculations of the borrower,
mainly in view of mounting taxes and of tax incentives for investment.

Then, in a comparison of the Bundesrepublik, the United States of America,
and Great Britain, it is explained how central banking has turned to influenc-
ing the supply of money more than the demand side by regulating the liquid-
ity of banks through open-market operations and changes in the reserve ratios,
instead of mainly relying in the traditional way on the use of the discount
rate. Here the surprising statement is made that, because of the lack of an
adequate quantity of qualified securities and a developed market for their
resorption before May 1955, no open-market policy in the full meaning of the
term was possible in Western Germany prior to that date. Further, attention
is drawn to developments which in these three countries have latterly led in
some degree to the restoration of the discount rate as a major means of
monetary policy. This tendency is contrasted with the situation in Switzerland
and Sweden, and reasons are advanced for the different attitude of these two
countries.

Although references to the matter are to be found, no systematic attempt
has been made to describe for the individual countries the limits of the central
banks' influence on nonbanking private financial intermediaries, which have
everywhere developed so vigorously. Here are some of the most "sensitive
points in the financial structure" of many countries, and it is of "the essence
of central banking to devise new means of imposing its influence in the direc-
tion higher social policy dictates."[1]

While those parts of this small volume which ably deal with the United
States and Great Britain may prove useful to the German reader, they contain
nothing new to the Anglo-Saxon public; on the other hand, less well-known
information as to the working of the central bank in the Bundesrepublik
during the past decade is included. This material together with the description
of the peculiarities of the Western German banking system and its situation
between 1948 and 1957, though somewhat overburdened with detail, will be
found most instructive. In this context, it may be interesting to note that the
U. S. system of Treasury tax and loan accounts with special depositories is
described in some detail (pp. 144ff.) as a good example which ought to be
followed in Western Germany to distribute liquidity, as "every money flow in
the public sector, exceeding a certain magnitude, is likely to pose problems
in the national economy" (p. 143).

Finally, it is pointed out that interest, though not for the rest of the
economy, is the decisive cost factor for the banks. On the whole, the concern

[1] As J. H. Power, *Am. Econ. Rev.*, Mar. 1958, *48*, 184, so aptly sums up the underlying
thought of R. S. Sayers, *Central Banking after Bagehot*, New York 1957, *passim.*

of the banks with monetary policy is stressed throughout so nearly exclusively that, for example, no details are given as to the use made of selective credit controls in the different countries.

Housing finance in the United States and West Germany is discussed at some length; the same might well have been done with respect to Great Britain.

E. ALEXANDER-KATZ

Israel Institute of Technology, Haifa

Public Finance; Fiscal Policy

Federal Expenditure Policy for Economic Growth and Stability: Papers Submitted by Panelists Appearing before the Subcommittee on Fiscal Policy, Joint Economic Committee, 85th Cong., 1st Sess., November 5, 1957; *Hearings before the Subcommittee on Fiscal Policy,* Joint Economic Committee, 85th Cong., 1st Sess., November 18-27, 1957; *Report of the Subcommittee on Fiscal Policy to the Joint Economic Committee,* 85th Cong., 2nd Sess., January 23, 1958. Washington: Supt. Docs., 1957; 1958; 1958. Pp. xix, 1203; xi, 663; iii, 15. $3.25; $2.00; —.

The Joint Economic Committee has put us in its debt again with its hearings on federal expenditure policy. These hearings, like the companion tax hearings of 1955, provided the occasion for economists to take stock and present whatever knowledge and insight they possessed that would bear on a number of policy problems, in this instance in the field of public expenditures.

The hearings produced three documents, a 1203 page volume of papers submitted by the 97 panelists, a set of hearings, and a brief Subcommittee Report which summarizes many of the conclusions. The volume of papers is an important book in public finance, containing the makings of an excellent textbook on public expenditures, as well as sufficient articles of high quality to fill an academic periodical for a year.

The papers range from reports on extensive new research, to surveys of applicable technical literature, to direct evaluations of policy. The enormous size of the volume makes it rather unapproachable, and so I shall single out a few of the pieces from those that should be of lasting professional interest.

There are two good historical surveys of federal finances. One is a study by P. B. Trescott, covering 1790-1956, which explains the lack of growth of the national government in the 19th century in terms of such factors as the federal system, presidential pressure against new activities, reliance on tariffs for revenue, abhorrence of debt, and finally a set of reform programs which had little cost, such as monetary reforms, antitrust policies and tariff reduction. The other paper, by A. M. Soloway, is a perceptive and detailed account of the last 50 years, tracing the growth of demand for public services and explaining their concentration in the hands of the federal government.

The relation of federal expenditures and revenue to economic stability is explored by Bert Hickman. He shows that federal expenditures are the most

unstable component of the domestic demand for GNP. In an analysis of automatic stabilizers, he finds that the more responsive tax and transfer system of recent years represents a net addition to the stabilizers, with corporate saving remaining as important a stabilizer as it was in the 1920's. To treat cyclical variations in corporate saving as a stabilizer is to consider only the effect on consumption, of course, and ignore any effects on investment.

A paper on defense spending by G. H. Hildebrand and N. V. Breckner underscores the destabilizing effect of the federal government's operations. They find that defense expenditures are determined by the state of international tension and of weapons technology (surely political leadership has some effect too!), and so move independently of the business cycle. Defense expenditures were blameless in the recession of 1949, added to the boom of 1950-53, and were the primary causal factor in the contraction of 1953-54. During much of the recent investment boom, defense expenditures remained constant, though the decline in new contracts during the presputnik economy wave surely helped to touch off the recession. Modern fiscal theory views government as a vast regulator of an inherently unstable private economy. In fact, a fiscal policy which managed to offset the fluctuations in the autonomous components of government demand and their repercussions would be quite a step forward.

A group of papers on atomic energy provides a useful summary of projections and discusses the federal program. P. D. Teitelbaum and P. Mullenbach find fossil fuels in adequate supply in the United States for the next 25 years, at little increase in cost. They bring out that the federal research program is relatively modest: more could be done without interfering with military research, but the program is in perennial peril of becoming embroiled in the private-versus-public power controversy. R. A. Tybout, at the end of a very comprehensive account of the program, wonders whether public aid to private reactor construction is not too generous. The aid consists of a public contribution to the demonstration program, a high price to be paid for the plutonium produced in breeder reactors, granting of rapid amortization certificates, and favorable treatment of the costs in utility rate-making. He suggests that a more leisurely program would develop atomic energy at a lower research and development cost, and still in plenty of time before a national need develops. But he admits that these conclusions may need to be modified by foreign policy considerations. One wonders whether the United States is trying hard enough in this phase of the East-West technological competition.

There are many other papers of note, some dealing with such broad issues as the proper duties of government, the optimal division of functions between federal and state and local levels, general policies to stimulate growth, and the problem of budget reform. Others address themselves to specific expenditure areas, including defense, foreign aid, natural resource development, housing, health and welfare, transportation, and research. On the whole, the group felt that there were pressing needs for further expenditures, particularly for education and research. Not much alarm over harmful effects of government on business was expressed, and there was considerable skepticism about the

virtues of turning functions over to state and local governments. Several papers advocated higher user charges to improve resource allocation.

The record of the Hearings, while frequently stimulating, is not as enlightening. The economists testifying were rather cautious witnesses, and when subcommittee chairman W. D. Mills asked for specific recommendations for expenditure reductions to finance the increased defense outlays, the answers were disappointing; agriculture and the veterans programs were mentioned again and again, with an occasional listing of water resource projects. Yet these are the areas in which it is a political achievement to keep the expenditures from rising. The 97 economists, despite the wide range of their political views, did not prove of much help in isolating areas in which savings might be realized.

OTTO ECKSTEIN

Harvard University

Finanzpolitik—Grundlagen und Hauptprobleme. By HANS HALLER. Tübingen
     J. C. B. Mohr (Paul Siebeck); Zurich: Polygraphischer Verlag, 1957.
     Pp. viii, 326. DM 25,—; paper, DM 21,—.

This is a competent and timely book. It analyzes governmental financial policies (primarily, fiscal policies) in what today is considered their proper context, the economy as a whole. The presentation avoids generalities and goes thoroughly into the analysis of specific measures, without being too closely tied to the economy of one particular country—in this case, Germany. It is, therefore, regrettable that this study is not available in translation for the benefit of more economists, and also legislators.

Approximately one-third of the book is devoted to the formulation of the theoretical basis for fiscal policies. This scholarly attempt to supply a hitherto missing link in economic analysis is mildly successful as far as it goes. But the exclusive application of "model-building" to the analysis remains baffling. Even a brilliant display of some 60 models that visually demonstrate how particular procedures affect the economy cannot perform the miracle of giving life, i.e., the power of creative thinking, to this overworked tool. It merely confirms the evident—and the author's own—expressed scepticism towards its use.

The inquiry itself starts with the exploration of aims of fiscal policies: fiscal soundness, minimum infringement on personal freedom of action, raising the general living standard and, finally, equitable results in the distribution of the national product. It goes on to show the possible conflicts and their compromise solutions. There is a continuous process of adjustment to the ever-changing patterns and valuations of the economic development, i.e., to the dynamics of a free-market economy. Against such a background any rigid concept of fiscal policy and especially of the budget as its central instrument —balanced or unbalanced—would be anomalous. So would any static approach to size or form of the public debt or to any other implement that supports the budget.

This relativity approach gains in significance when the guiding role of the

budget in the economy is considered. Such dominance may well be taken for granted in view of the magnitudes involved. But even then the author sees a choice left in the way this guiding role is acknowledged: either the old notion of the balanced budget may be accepted and a surplus (approvingly) or a deficit (grudgingly) admitted as a consequence of later economic developments; or there is overt planning for a stated kind of budget. The first approach is compatible with the classical principle, the second is not. In both cases, however, the budget expresses fiscal intent; we might term this, somewhat facetiously, an existentialist concept.

In his examination of expenditure and revenue measures, the author distinguishes between policies that tend to stabilize current conditions and those that aim at changing them in the direction of stimulating the economy out of a recession or of curbing an oversized boom. As to the first alternative (stabilization), a certain degree of fluctuation would have to be permissible; but how much, the author does not say. A refusal to permit any deviation from the *status quo ante* puts a rigid budget concept in line with an equally rigid, fully regulated economy. Coming to the selection of means to keep the economy on an even keel, the author favors additional or curbed expenditure, depending on whether the economy is moving downward or upward. He also suggests careful "dosage" of unchanged expenditure totals, by speeding or retarding their use. He even considers anticipating now some spending that was scheduled for the next budget or transferring present appropriations to a later year whenever circumstances demand. Revenue alterations are held to be less practicable; they cannot be effected easily at the required frequent intervals. Monetary policy presents itself as a companion measure.

Changing, however, the direction of the economic trend would entail stronger fiscal and also nonfiscal procedures. In the fight to end a recession, for example, larger additional expenditure, with the strongest multiplier effect, is proposed. Tax reduction, too, is suggested: primarily a reduction of income taxes. Consideration is also given to the possibility of stimulating redistribution of tax burdens that would leave the tax total untouched presumably by a combination of decrease in rates for the lower brackets and an increase for higher ones, or an increase and decrease in exemptions. Monetary measures would more or less go along with the fiscal policies. But if the economy moves in the opposite direction, namely into an inflationary boom, then tax increases and expenditure cuts may not be as readily utilized. Here the author leans more heavily on monetary procedures than in the case of recession[1] although he stresses their impact in that they may make it difficult to stop a downward trend once they have started it.

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[1] We may call attention to the Swedish experiment of 1955-56. A 12 per cent tax on industrial investment and a rise of the bank rate were put into effect simultaneously to curb the boom. A year later, the results were checked by issuing a questionnaire to some 3000 companies. The answers showed among other points that (1) 60 per cent of the firms asked carried out their investments regardless of the restriction, and (2) the remaining 40 per cent dropped their plans, partly on account of the levy alone, partly because of the tax *and* the discount rise; none for the interest rate alone. All in all, the tax proved to be the more effective weapon. (International Monetary Fund, *International Financial News Survey*, June 15, 1956.)

Many other aspects of fiscal policies are analyzed, with regard to the public debt, foreign economic developments, foreign exchange, etc. But one most impressive conclusion stands out: that governmental financial policy emerges as the one way to guide an economy without imposing direct controls. Once again, it is implied that Keynes saved capitalism.

                                                    HEDWIG REINHARDT

The City College of New York

Federal Lending and Loan Insurance. By R. J. SAULNIER, HAROLD G. HAL-
    CROW, and NEIL H. JACOBY. Princeton: Princeton University Press, for
    National Bureau of Economic Research, 1958. Pp. xxx, 566. $12.00.

The purposes, the scope, and the economic consequences of federal lending and loan-insurance programs in recent decades have been such as to merit a comprehensive study of them; and the authors of this book have given the subject practically encyclopedic treatment. Furthermore, the quality of their work is essentially high throughout. This volume may rightfully take its place along with the other major works sponsored by the National Bureau of Economic Research.

The organization of the book is simple and effective. Part I is a summary treatment of all phases of the federal government's program of lending and loan insurance from 1916 to 1953. Part II has three long chapters dealing respectively with the details of the program with regard to three major segments of our economy: (1) agriculture, (2) business and financial institutions, and (3) housing. Lengthy appendixes, made up mainly of statistical data, complete the volume. To each category of loans or loan insurance, the authors apply a more or less uniform treatment consisting of analysis of "Services and Credit Terms," "Credit Experience," and "Impact" (economic effects). The treatment is factual, objective, and temperate.

The analysis of the economic consequences of government lending and loan insurance suggests immediately that students of fiscal policy have been rather seriously remiss in not taking account of such activities in their study of the leverage effects of governmental fiscal transactions. Students of fiscal policy have typically divided their analysis into three parts: (1) spending, (2) taxing, and (3) borrowing. The study under consideration indicates clearly that a fourth category, lending and loan insurance, might well be added to the list. It is true that lending and loan insurance on the part of the government may be thought of as constituting a sort of "conditional spending" and thus in a sense are comprehended in "spending"; but a program of lending and loan insurance differs in so many significant respects from other spending on the part of the government that separate treatment would appear essential to an adequate treatment of fiscal policy. To the knowledge of this reviewer (admittedly quite limited), no writer on the general subject of fiscal policy has given satisfactory attention to government lending and loan insurance as a separate factor influencing the total performance of our economy. The authors of the study in question have provided an abundance of basic data bearing upon this problem and have made a commendable beginning on the analysis of them.

If there be a shortcoming in the study worthy of mention it is possibly the failure of the authors to deal explicitly with the effect of government lending and loan insurance upon the distribution of purchasing power among the people. It is to be suspected, for example, that the incomes of building tradesmen in recent decades have been considerably boosted by F. H. A. loan insurance. Concern with this problem is implicit in much of the authors' analysis, but the question is approached indirectly rather than directly. Inasmuch as the size of one's present purchasing power constitutes an important factor in determining what one will do with any increment to that purchasing power, the effect of government lending on redistribution of purchasing power comes to be a highly significant matter. Further analysis in this direction would appear desirable.

It is to be regretted that the book was not published until 1958. Most of the statistical data end with the year 1953, and therefore some of the material appears a little antiquated at present. This delay in publication seems to have been unavoidable. From another point of view, the appearance of the book is quite timely. That is, we may well see an important revival of government lending and loan insurance in connection with efforts to end the present recession.

TROY J. CAULEY

The University of Texas

Financing Highways. A symposium volume. Princeton, N.J.: Tax Institute, 1957. Pp. 217. $5.00.

This brief volume contains fifteen short papers presented at the 1956 Symposium of the Tax Institute and addressed to the problems emanating from the passage of the Federal Highway Aid Act of 1956. The panelists address themselves to the following areas of concern: (1) the charge on the new highway system and the results that can reasonably be anticipated; (2) the reapportionment of fiscal responsibility implied in future highway financing; (3) the role of highway user-revenues in financing the new system; (4) the problems arising out of the use of specific taxes and dedicated revenues; and (5) the methods to "expedite" the highway program. The papers, of course, vary considerably in quality, but the informed lay reader will find intelligent and provocative discussion of the problems associated with bringing the prospective highway program into reality. Those few papers which are disappointing are readily identified as the efforts of state officials to review too eagerly the day-to-day problems of the particular departments with which they are associated.

As a group the panelists stress the importance of integrated planning among government units in the construction of the new highway system in order to avoid unnecessary obsolescence (p. 7) and harassment of metropolitan planning (pp. 8, 9). The cost of highways may run to $186 billion by 1975 (p. 28) as we prepare for 100 million automobiles. In general the panelists express strong pleas for the increase of state as against federal authority in highway financing—meaning surrender by the federal government of tax sources. A

·provocative paper by William D. Ross argues that institutional changes of
the last fifteen years (especially consumption-income relationships) mean "the
end of the deflation-depression aspect of stabilization programs" (p. 124).
Not all of Ross' colleagues will agree with him at this time.

<div align="right">JOHN D. HOGAN</div>

Bates College

*Public Finance and Less Developed Economy—With Special Reference to
    Latin America.* By PAUL A. M. VAN PHILIPS. The Hague: Martinus
    Nijhoff, 1957. Pp. xvi, 185. ƒ 13.50.

W. A. Lewis complains in his *Theory of Economic Growth* that "there is
regrettably very little theoretical discussion of the fiscal problems of under-
developed countries." Thus, a book on the role of public finance in under-
developed countries is prima facie a welcome addition to the growing litera-
ture on the practical policy problems of underdeveloped countries.

Mr. Van Philips presents the fiscal policy problems peculiar to underde-
veloped countries in the broader framework of an inquiry into the character-
istics, economic as well as noneconomic, of underdevelopment. He also
discusses the related issues of inflation and deficit financing. His treatment of
the characteristics of underdeveloped economies deviates little from the nowa-
days commonly accepted view that underdevelopment must be analyzed in
terms of social relationships and social forces, rather than in purely economic
terms. But he also singles out certain economic peculiarities of underdevelop-
ment: the small stock of capital, the low rate of capital formation, the pro-
pensity for inflation, the great dependence on foreign trade which results in
instability in general and in unstable government revenues in particular, and
a "low effectiveness of monetary policy."

The greater part of the book consists of a discussion of problems of mone-
tary and fiscal policy. A third part is a recapitulation of the main conclusions.
These may be briefly set out, as follows: (1) Government and government
finance must play an active role in fostering economic growth; (2) The short-
age of capital restricts the effectiveness of deficit financing; (3) Increased
government activity is a prerequisite for economic development and thus
requires larger budgets; (4) Additional tax revenues must be derived from
progressive direct taxes because indirect taxes would unfavorably affect the
consumption of "the great masses of the poor," while progressive taxes, if used
to finance public capital formation, would lower luxury consumption, but not
total savings.

This summary obviously is much too brief to do justice to the author who
in many places reveals a keen insight into the policy problems of underde-
veloped countries. Nevertheless, I find it difficult to accept the main policy
conclusion that more progressive taxes would help to solve the public finance
problems of underdeveloped countries. I agree with the proposition that there
is a place for (moderately) progressive income taxes in the tax structure of
underdeveloped countries. But I miss in Van Philips' analysis any discussion
of the disincentive effects of progressive taxes, compared with the disincentive

effects of consumption taxes which, incidentally, also may have a moderately progressive incidence (cf. the findings of the Indian Tax Inquiry Commission on this subject). I also feel that the analysis is inadequate because it fails to take account of the incidence of the benefits of government expenditure—on the basis of which I believe the author could have made a stronger case for progression.

Misuse of the English language mars the readability of the book. And I am afraid Lewis' complaint still holds good.

JOHN H. ADLER

Washington, D.C.

Taxation and Foreign Investment. By National Council of Applied Economic Research, New Delhi. Bombay: Asia Publishing House, 1957. Pp. vii, 164. Rs 9/50.

The last several years have brought extensive critical examination of and major changes in the taxation system of India. *The Report of the India Taxation Inquiry Commission,* published in 1955 and the Nicholas Kaldor memorandum on tax reform encouraged substantial alterations: a revival of the capital gains tax, new expenditure and wealth taxes, and extensive revisions in individual and company income taxes. The present volume is an assessment of the impact of these changes on the future of foreign investment in India. A considerable research effort went into this study. Questionnaires were circulated among business firms and chambers of commerce; government officials, business executives and accountants were interviewed by staff members of the Council. This research was supervised by P. S. Lokanathan, director general of the Council.

All aspects of the Indian tax system relevant to the conduct of economic activity by foreign nationals are examined here, including such features of the tax laws as their impact on the personal income of technicians, the exemption of home leave travel and the tax treatment of other special allowances for foreign personnel, the burden of company taxation, the tax on improper accumulations, the requirement that companies deposit a part of their accumulations with the government, and the tax burden on nonresident companies.

The authors of the study find themselves in something of a quandary. They would like to stress the concessions that India now makes to the foreign investor and to emphasize that the climate is most temperate for foreign investment in India. But at the same time there are a number of points at which tax burdens appear to be onerous; the authors have emphasized that relief should be forthcoming. On balance the volume conveys the impression that the burden of taxation on foreigners and foreign investment in India is in fact rather severe. This appears to be particularly true of the company tax.

No effort is made in this study to suggest that the total future of India's economic development program depends on successful efforts to attract foreign private investment. The authors pointedly stress that foreign capital has not played a determining role in the industrialization of any great nation. Rather, the approach is to urge temperately that the government look at some of the features of the tax laws which have an undesired, unintended but unfortunate

impact on foreign investment. And the tone is never shrill. In this country students of public finance are accustomed to read that the Congress must repeal taxes that are strangling the free enterprise system. In India the Council writes, "There would seem to be a line of corrective action that might commend itself to Government" (p. 89).

There is one disturbing pattern of comparison in the tax-burden analysis made by the Council: Throughout the volume it is contended that underdeveloped countries are in competition, one with another, for the investments of the capital-exporting countries. Indian tax burdens on foreign investment must not get out of line, it is suggested, with such countries as Burma, Pakistan, the Philippines. This approach to tax policy has long been extant among the American states, and the resulting tax competition for new industry brings on an advanced state of fiscal incapacity. Tax competition among the underdeveloped countries similarly will tend to perpetuate the low productivity of their revenue structures.

JESSE BURKHEAD

Syracuse University

Japan's Finance and Taxation, 1940-1956. By SABURO SHIOMI. Translated by Shotaro Hasegawa. New York: Columbia University Press, 1957. Pp. xi, 190. $6.00.

As Carl Shoup notes in his preface, students of income taxation will find much of interest in Saburo Shiomi's study. This is an account of a fiscal system swollen and distorted by war, shattered by defeat, reconstructed by outsiders, and finally revamped by the Japanese themselves in a form more to their own liking. It is partially, from an American point of view, a study in futility and frustration.

Saburo Shiomi, who is professor emeritus of economics and lecturer in public finance at Kyoto University, as well as president of the Japan Tax Association, traces the establishment and development of the income tax in Japan, the adoption of a modern tax system in 1940, the disastrous effect of the war on Japan's monetary and fiscal system, the tax problems of the postwar era, the significant changes in the tax system that occurred in 1949 and 1950 as a result of Shoup's recommendations, and the subsequent efforts (largely successful) of the Japanese to "turn back the clock."

The interest of American readers is likely to focus on Chapter 5, "The Shoup Mission Recommendations" concerned with the tax mission to Japan in 1949 and again in 1950 which Shoup headed. To be fully intelligible, however, Shiomi's chapter should be read along with "The Aftermath of the Shoup Tax Reforms," Parts I and II, by M. Bronfenbrenner and K. Kogiku, in the *National Tax Journal.*[1]

Shiomi is most polite, laudatory and restrained. He says:

> I believe that Dr. Shoup's proposed tax reform is a splendid achievement which we can be proud to hand down to posterity. . . . The Shoup tax system is not the American tax system forced upon Japan. It is an ideal

[1] Sept. 1957, *10,* 236-54; Dec. 1957, *10,* 345-60.

system based on Dr. Shoup's theory, which was made public over ten years ago. . . . A creditable tax system such as has rarely been seen in the history of taxation—an ideal tax system that has never been tried either in Europe or in America—is now being put into practice in Japan. It must be remembered that it will have to be erected in a country having a long tradition and still in a wartorn condition. However desirable a system it may be, it cannot be successfully practiced unless due consideration is given to the prevailing economic and social conditions of Japan. . . . There is no doubt in my mind as to the high ideal of the Shoup tax system; it is the most advanced system in the world. My only concern is how to foster the ideal and make it work harmoniously in this country, where a number of abnormalities still exist. . . . (pp. 90-92).

This is the Japanese way of saying "impractical." Bronfenbrenner is more succinct: "The wonder is less that the Shoup program was modified drastically than that it has survived at all" (*National Tax Journal*, Dec. 1957, *10*, 358).

Shoup's objectives were indeed laudable. The progressive and broad-based personal income tax was to be retained and improved as the mainstay of the Japanese national tax structure. Yet successive amendments and changes in the tax have caused such deterioration that the Japanese personal income tax, in Bronfenbrenner's words, "may become in time little more than a disguised payroll tax." The Shoup Mission's effort to secure a reduction in the number and rates of commodity excise taxes and to minimize indirect and consumption taxes, has now been turned aside and reversed. The effort to improve the sources of local revenues, to reduce or eliminate shared national taxes, has been thwarted. The three basic fiscal innovations suggested by the Shoup group—the net worth tax, the accessions tax, and the value-added tax—received short shrift. The net worth and accessions taxes were repealed in 1953 while the value-added tax, whose enactment date was twice postponed, was finally repealed without ever having gone into effect. Other examples of negation could be cited if space permitted.

In the light of these developments, Shiomi's following comment must bring a wry smile to Shoup's face:

The new plan requires careful handling under the circumstances. Hasty enforcement must particularly be avoided. It would be advisable to set up the proper order of application so that the people gradually become familiar with the spirit of the reform; otherwise reactionary factions may spoil it. After all, a big undertaking like this cannot be accomplished in a day (p. 92).

Shiomi comes closest to reality when he says: "It has been commented that, ideal as the Shoup tax system is, Japanese social and economic conditions do not provide a solid foundation for its successful practice. Most business leaders, as well as the academic representatives present, concurred in this view, saying that the system was far too advanced for the Japanese people, whose conservative mind is set on the old-fashioned concept that an old tax is a good tax, a new tax is a bad tax, and that all light taxes are good, all heavy taxes are bad" (p. 98).

Yet even if all his recommendations had been reversed, which fortunately

has not yet been the case, Shoup's work would have served a very useful purpose in stirring Japanese thinking on tax matters, in stimulating a vast amount of public and private debate in Japan on alternative methods of taxation and standards of equity. In similar fashion, Shiomi's book may stimulate thinking about the feasibility of advanced fiscal techniques in varying economic environments. It is a pity that this study was not available to Nicholas Kaldor before he made his recommendations on Indian fiscal policy.

JEROME B. COHEN

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International Economics

Théorie et pratique de la coopération économique internationale. By JACQUES A. L'HUILLIER. Paris: Librairie de Médicis, 1957. Pp. 603. 2,400 fr.

Professor L'Huillier, of the University of Geneva, presents in this volume an informative study of contemporary efforts to promote international economic cooperation. The programs discussed are the General Agreement on Tariffs and Trade, the Marshall Plan and the Organisation for European Economic Cooperation, European Coal and Steel Community, International Monetary Fund, European Payments Union, and the Treaty for the European Economic Community. In each case, the author provides a descriptive summary of institutional arrangements and objectives, followed by an analytical critique.

The inquiry is guided by the belief that "protectionism is less an aggressive doctrinal tendency than a defensive reflex, more or less justified according to the case, in the face of certain fundamental difficulties neglected in classical theory: economic depression first . . . the instability of markets for basic materials . . . and difficulties in maintaining balance-of-payments equilibrium . . ." (p. 555). The argument is that it is misleading to criticize protective measures as though they were arbitrary interferences with what would otherwise be ideal economic efficiency: they are typically responses to pre-existing inefficiency traceable to inflexible domestic economic structures, compounded in the interwar period by deficient demand. It follows that the only realistic way to attack protection is through positive group programs to control aggregate demand, to strike at the roots of internal structural difficulties, and to provide workable payments mechanisms to reduce recourse to controls intended to protect reserves. It follows, further, that "positive intervention" in the market by international organizations may often be preferable to free-market solutions—a position which is profoundly correct in principle and exceedingly difficult to apply in assessing, say, article 58 of the Coal-Steel Treaty.

The author is quite critical of some of the restrictive powers granted to the Coal-Steel Community, but supports the basic approach as facilitating "disciplined progress" in contrast to "the somewhat primitive dynamism of natural selection" (p. 319). He is rather hard on some of the major aspects of GATT and the IMF, and most warmly inclined to the EPU and OEEC. His preference for the latter two blends several considerations: a strongly European

viewpoint and a belief that only a small group of similarly oriented countries
can cooperate to the extent of consulting usefully on each other's internal
policies; a conviction that programs to reduce trade barriers can only succeed
if closely linked to an effective payments mechanism providing international
liquidity; and a general conclusion that flexible organizations with few defined
powers and correspondingly few specific limitations are more effective than
institutions with wide powers spelled out in detail but subject to countless
precautionary limitations.

The author does make a number of useful recommendations for improve-
ments in EPU-OEEC practice. Perhaps his most important criticism is that
they are unduly reluctant to discuss desirable currency values for individual
member countries. Given strong reservations on the desirability of general
domestic contraction to correct external deficits, combined with a basic case
for low tariffs and restricted recourse to national trade controls, L'Huillier
arrives at the eminently sensible conclusion that exchange-rate changes should
be high on the list of steps considered in discussing cases of persistent deficits.
The advice is well directed: the reviewer remembers well the shocked antago-
nism generated in OEEC discussions a few years ago (circa 1952-54) by any
hint that a specific country might be able to combine more rapid expansion
with external balance if its currency value were not regarded as sacred.

The book as a whole conveys a certain atmosphere of an orderly office,
windows closed and blinds drawn, the dust and heat of the world shut out to
facilitate thorough dissection of the growing stock of official documents.
Factual references from sources outside of the institutional reports, and direct
statistical verification of quantitative judgments, are definitely minimized.
The author provides logical clarity—a most valuable contribution—at the
expense of color. That cost is sad but bearable, particularly when the degree
of clarity is as high as here. But in two major instances an additional cost,
that of a loss of relevance, also creeps in. The brief discussion of exemptions
for underdeveloped countries under GATT is sensible but does less than
justice to the genuine complexities of the issues involved. The dollar shortage
is treated carefully but with more respect for its unyielding structural charac-
teristics than recent history would seem to warrant. The explanations of the
shortage are in the past tense (unless my French verbs betray me); the ex-
planations of the case for systematic discrimination against the dollar area
because of the shortage seem to refer to the present and future. The bridge
between the 1946-50 factual situation explained and the apparently current
reference of the case for discrimination is not readily discernible.

One minor point cannot be allowed to pass. To refer to Machlup as an
eminent disciple of Keynes (in the application of income theory to interna-
tional economics) is to confuse the letter and the spirit in a way unlikely to
please either of these excellent men.

The enormous range of issues raised in this book precludes thorough investi-
gation. The author is generally excellent at raising the main questions, sug-
gestive rather than conclusive in answering them. The greatest weakness is
the insufficient use of factual reference to illuminate the quantitative signifi-

cance of the main factors discussed; the greatest strengths are the author's high order of logical competence and unifying comprehension of the role of positive international planning in promoting an environment favorable for economic efficiency.

JOHN B. SHEAHAN

Williams College

La localización de las actividades económicas en Europa después de la integración unitaria. By JOSÉ LUIS SAMPEDRO, JEAN CHARDONNET and ANDRÉ THIÉRY. Madrid: Estudios Económicos Españoles y Europeos, S.A., 1957. Pp. xliv, 820.

This is an extremely challenging work. Confronted with the inevitable creation of a European Common Market, the authorities of the Banco de España—the Spanish central bank—decided to conduct a series of investigations into the developments that European economic unification would presumably bring. Six such studies have already been published under the general title *Estudios Sobre La Unidad Económica De Europa*. The study which I am now examining is the sixth of the series. The previous studies are mostly concerned with an analysis and measurement of the various European economies. This one deals with a problem of supreme importance: "How will European unification affect the distribution of industry and agriculture in Europe?"

This study was written prior to the actual coming into being of Euromarket. That explains why some of the developments now in effect under the European economic integration scheme are considered merely as possible in this volume.

The study by Sampedro is by far the longest of the three research projects of which the volume is composed. It occupies over 700 pages. The others are approximately fifty pages each. Originally, the directors of Estudios Económicos Españoles Europeos thought of assigning the task to Sampedro alone. They decided eventually to launch a competition in all the European countries so that the views of other scholars could be obtained on the problem of industrial distribution in Europe following Euromarket. Western Germany was the only country from which no entry was received. From among the manuscripts obtained (the prize was of 150.000 pesetas), the works by Chardonnet and Thiéry were selected.

In anticipating the industrial changes that economic unification may bring to Europe, the authors contemplate four possibilities: (1) a European common market consisting exclusively of the Western European continental nations; (2) a European common market consisting of the whole of Europe, that is, continental Europe without the Soviet Union; (3) plan one plus the United Kingdom; (4) plan two plus the United Kingdom.

In their approach to this problem, Sampedro and Chardonnet—essentially Southerners—take views which are favorable to the southern economies; whereas Thiéry, a Northerner, seems to opine that economic unification would benefit mostly the northern economies.

The argument of the Spanish contributor, Sampedro, runs basically as follows: 19th century coal and steel industries made the area between the North Sea and the Alps the foremost industrial and economic center of

Europe. This is the industrial Lotharingia situated in the basins of the three great rivers, the Rhine, the Rhone and the Po. Euromarket will not change the importance of this zone. As a matter of fact, it is quite possible that this great industrial triangle will add to its present economic power. However, the economic unbalance which will become intolerably apparent after unification—the proximity of the southern economies to North Africa and the oil wealth of the Middle East, their extremely favorable Mediterranean position and their abundance of competent workers—are all factors which are bound to stimulate investment in and growth of the southern regions (essentially southern Italy and Spain), which eventually will proceed at a higher rate than that of the more industrialized northern areas. Sampedro corroborates his reasoning with an abundance of statistical information.

The French contributor, Chardonnet, ventures the opinion that, as long as Great Britain remains outside the European common market, the changes within the market will be essentially industrial changes and not agricultural changes. Should Great Britain join the market, that would mean the collapse of British agriculture and a powerful stimulus to European continental agriculture. Northern Western Europe will continue, after unification, to exercise its position of industrial leadership in Europe, especially if Britain becomes a member of the union. However, there is no doubt that the strengthening of the northern industrial belt will by necessity result in efforts toward the industrialization of the southern regions, namely southern Italy, Spain and North Africa. The mineral resources of these territories have not yet been fully explored, their capability of being lifted to much higher levels of economic well-being is substantial and their geographic position is extraordinarily valuable.

Chardonnet advances the view that, should the Soviet satellites join the European market their economies will have to pass through a painful period of adjustment. This is due to the fact that a substantial part of their industrial growth is largely artificial as it is stimulated by the supply of Russian raw materials and dedicated to the objective of strengthening the Soviet economy.

Thiéry, the contributor from Luxemburg, is a pessimist as far as the Southern European territories are concerned. He thinks that, if nothing is done, the industrial northern areas will become even more industrialized as a result of Euromarket and the southern regions will sink even deeper into industrial lethargy. He advocates that funds be made available to the less developed southern regions, that administrative decentralization on a federal basis be undertaken and, finally, that Euromarket be made to transform itself into a constitutable Euronorthafrica.

I have said that these studies preceded the actual creation of the European Economic Community. One can see now how farsighted the negotiators were when they included in the treaty a vast project for investment in and industrialization of important areas in North Africa.

One thing more should be said about Sampedro's work. In his study, he examines the fiction of a United States of America hypothetically disunited from its very beginnings and of how the industrial growth of the United States would have proceeded had such disunity been maintained.

This series of essays on European economic integration originating in Spain, a nation which is not yet a part of Euromarket, powerfully attests to the vigorous awareness in Spanish political and economic circles of the importance of this problem for Spain, as well as to the high degree of achievement attained by Spanish economics scholars.

CARLO MARIA FLUMIANI

Boston College

Die Grundlagen der Aussenwirtschaftstheorie. By ALBRECHT FORSTMANN. Berlin: Duncker & Humblot, 1956. Pp. xvi, 418. DM 32,00.

In this remarkable book an effort is made to formulate a new theory of the complex structure of international economics. The first of the five chapters is devoted to the problem of international economic theory, followed by a description of the "classic" foreign trade theories, and a theory on the adjustment and equalization of regionally unevenly distributed production factors. The author shows in detail the development of the various structural types and the trend towards the optimum combination of productive factors in all types of economies. After demonstrating the relationship between international trade and foreign exchange, Forstmann takes great pains to explain exogenous influences conditioning the changes in international economic relations, especially in regard to the economic situations after the first and second world wars.

The author recognizes the historical primacy of the classical international economic theories. He accepts in principle the theories of comparative cost and of the balance of trade, but not without the modifications necessary to meet, as he sees it, the realities of modern economics. In this connection, moreover, he offers his theory of unilateral transfers of value, which is caused, at least basically, by exogenous influences. Such unilateral action is shown in exports of goods and services without reciprocal imports, and vice versa. This produces a disequilibrium of the balance of trade, favorable or unfavorable, which has to be offset in the balance of payments by active "alimentary" payments. Forstmann attributes this situation to technological changes which have created production capacities in advanced countries surpassing domestic demand. The producers have been forced to find new markets abroad to utilize the available capacity fully and to satisfy the need for employment.

In the development of his thesis, Forstmann strongly rejects the opinions of Marx and condemns any policy of self-sufficiency as economically illegitimate. To some extent his attitude is anti-Keynesian and its trend is to accept or come close to accepting the teaching of Wilhelm Röpke.

At the end of the book, the author once more reiterates in summary the necessity of adjusting and of equalizing the uneven regional distribution of production factors. This equalization, in his opinion, can only be achieved by unilateral transfers of value, whereby the best utilization of productive forces is obtained and the general welfare of all nations is best served. He rejects counterarguments which say that international economic relations should be

restricted to the exchange of goods as the only way of "democratic foreign trade policy" to assure equality and sovereignty among nations. The development of the so-called underdeveloped countries requires foreign capital and Forstmann calls the arguments that such capital transfers would endanger the independence of the receiving nations pure "nonsense." He points out that nobody could say that the sovereignty and standing of the United States was lost or suffered under the capital inflow and foreign investments which made it the greatest debtor nation of the world prior to the first world war.

Forstmann's arguments are not always convincing and some of his conclusions are not completely acceptable. The international division of labor is not quite as easily explained as the author tries to do. It seems rather likely that overproduction would lead first to an attempt to increase domestic demand by price reductions before resulting in unilateral transfers of goods and services for which payment could be made by capital export only. The second law of Gossen could be applied only if returns for such capital transfers should be attractive enough. In fact capital movements are often politically motivated and oriented in modern times. Also, if they are not outright grants, there will always be the problem of repayment, for which the author offers no direct solution.

To some readers, at least, the book will seem complicated by long and excessive footnotes and repetition. The volume nevertheless makes interesting reading in a field where divergence of opinion is constant. It can be argued whether the author has succeeded in his endeavor, but most of the readers will agree that his presentations are provocative and challenging.

LUDWIG H. MAI

St. Mary's University

International Trade: Goods, People, and Ideas. By WENDELL C. GORDON. New York: Alfred A. Knopf, 1958. Pp. xix, 647. $6.75.

Professor Gordon's book is a textbook and a good one. But it is not a textbook in international trade, as one might infer from its title. Rather, it is an attempt "to integrate current thinking in the field of international economic relations into a coherent whole." If one feels that the first course in international economics should be something more than a pass at history of thought from the mercantilists, a good dose of theory and related commercial policy, with a final chapter on international institutions and maybe economic development, this book will be a welcome addition to the self of choices. My inclination is along these lines; for those who prefer more traditional fare, Gordon has little to offer.

As one might expect, trade theory à la Haberler and à la Machlup gets short shrift; this is not personally objectionable as plenty remains to occupy the undergraduate, perhaps of considerably more importance and certainly of more interest. This is more than a minor shift in emphasis; the course for which Gordon writes (and for most institutions a new one) should be called *Introduction to International Economics.* This presumably could be followed

by courses in trade theory, international finance, economic. development, and/or area studies for those who want to specialize.

In terms of level, the author has chosen wisely. Extensive ability to use analytic tools is not required but the student is expected to have gleaned more than a little from his principles course.

Another striking innovation in this book is the moral tone vigorously pursued throughout. Chapter I starts with "the dogmatic assumption . . . that the human race . . . is trying to bring about, or should be trying to bring about, that combination of (a) higher material level of living, (b) greater individual freedom of action and expression, and (c) increased security, which the race considers most desirable" (p. 4). From time to time, the discussion seems far removed from this assumption, but for the most part these objectives are repeated often enough to remind the reader of the author's system of values.

The foregoing objectives are particularly significant to the policy conclusions reached and which follow reasonably well from these values. They are stated well and with considerable courage. Only then will the student see the implications of accepting Gordon's values. If nothing else, the desirable objective of student soul-searching should result from recommendations for a common international currency, unhampered migration of peoples between nations (nations, themselves, are dispensable so far as Gordon is concerned, I gather), international incorporation of firms doing business across national boundaries.

Students will also find the style and format pleasing. Dry humor (e.g., Texan Gordon, speaking of the embargo on chilled and frozen beef, says "This being a fighting issue in the cattle country, perhaps the less said about it the better" [p. 208]) leavens this rather large loaf. Specifically, too, students will find interesting, interspersed discussions of Marxism and its application (or misapplication) by the Soviet Union to various cold-war issues. Hobson, Veblen, and Ayres also are woven into various parts of the discussion.

Some of the policy recommendations are personally unacceptable. Other than these, which cannot be here detailed, there are two minor criticisms: (1) The book is too long for a quarter course and perhaps even for a semester course. It demands ample time for careful classroom discussion. The length develops not so much from too many subjects but from more exhaustive treatment than some deserve. Five pages devoted to "travel and passports" (pp. 569-73) is more a pamphleteering venture than judicious allocation of space. (2) I was frequently irritated by references to unidentified people of whom I had never even heard. A good many of those cited were qualified by personal knowledge, but to most juniors and seniors these would be a mystery —and not one they would likely try to solve for themselves. The author should have included as a part of his educational responsibilities the identifying and qualifying of his cited authorities, or not have bothered to include their names.

JOHN M. HUNTER

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International Finance. By CHARLES N. HENNING. New York: Harper & Brothers, 1958. Pp. xix, 481. Text ed., $7.50.

This text is designed, so the author notes in the preface, "to integrate in one book the discussion (1) of practices used in financing foreign trade . . . and (2) of theory and problems of international finance." It is intended to be suitable both for business courses in techniques and procedures and for economics courses concerned with the basic problems of international finance.

To attempt both of these discussions in a single volume, even one of nearly 500 double-column pages, is certainly a bold undertaking; however, the product is in some respects quite satisfactory. On the one hand, the descriptive sections deal with the materials in substantial detail and present recent developments in financing foreign trade with clarity and thoroughness. On the other hand, much of the difficult analytical materials, that constitute the core of courses in economics, are treated rather summarily. For instructors who emphasize the broader problems of international finance the allocation of space will probably not be to their liking, but for those who want a thoroughly readable presentation of practical methods of finance this is probably the best book available. Most of the texts in this field include both the descriptive and the analytical materials to one extent or another, and the amount of time devoted to various subdivisions of the subject must depend finally on the tastes and interests of the instructor and on the objectives of the course. It is well to have texts available with each emphasis, and this one should prove a useful addition, both as a text and as a reference.

The introduction contains, in addition to the usual opening remarks concerning the scope and plan of presentation, the only chapter which offers a systematic treatment of the balance of payments. Like much of the book, this chapter is largely in descriptive terms and includes discussions of the double-entry nature of the balance of payments, its relation to both business and social accounting procedures, and problems involved in compiling estimates. There is a great deal here that will contribute to the understanding of the undergraduate in either business or economics. One might wish, however, that some place the author had developed the balance of payments as a significant tool of analysis, for there is much in the later sections which could have profited by such an addition.

Part II, "Procedures and Practices," is a descriptive section in which a large number of the credit instruments used in foreign-trade financing are explained in detail. More than 75 pages are devoted to the letter of credit— the forms it may take, the responsibilities of participating parties, and problems of uniformity. Many specimen are reproduced; more, it appears to this reviewer, than are necessary in view of the detailed explanations. This section of the book could perhaps be a valuable reference for businessmen who wish to expand their knowledge of alternative credit devices or for courses which do not include much descriptive detail. But good reference books sometimes present problems when it comes to teaching from them, and this difficulty may be encountered in working through the plethora of detail included.

Part III, "Institutional Operations," explains well the roles of commercial

banks and other institutions participating in the exchange market. The analysis of the various quoted rates and the operations of the New York exchange market are among the topics which receive excellent treatment. In addition to accounts of sources of, demand for, and methods of handling acceptances and drafts, a short chapter is devoted to forces determining the rate of exchange. Unfortunately, its brevity prevents the chapter from adding significantly to the discussion and it might better have been included in the last part along with other theoretical discussion.

The final section, of just over 100 pages, covers most of the materials under the heading "the theory and problems of international finance." The usual subjects are discussed including the dollar shortage and convertibility, exchange problems following both recent wars, international investment, and economic development. For so short a discussion, a large number of subjects are considered and reference made to many important contributions. However, unless the students are well-grounded in the literature or have more than average eagerness to pursue a large volume of outside sources, this section could prove difficult and confusing. For example, the author examines the arguments of Gottfried Haberler, Thomas Balogh, and Charles Kindleberger on the dollar shortage, all within slightly more than one page. When the treatment here is compared to the detailed explanation of the mechanics of the letter of credit one can justifiably argue an unevenness of the level of presentation. Fortunately, the book is generously footnoted and excellent reading lists and problems follow each chapter, but these are probably not sufficient to make the last section useful without considerable supplementing.

M. D. WATTLES

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Business Finance; Investment and Security Markets; Insurance

Investissement et financement. Origine et emploi des fonds de grandes sociétés.
    By MARCEL MALISSEN. Cahiers de la Fondation Nationale des Sciences
    Politiques, No. 88. Paris: Armand Colin, 1957. Pp. 215. 1.600 fr.

The introduction to this book points out that the problems of output and employment are related to those of economic growth. The intent of the author is to discuss why and how firms invest and grow in a developed economy. Considering the investment and financial behavior, during the years 1949-1955, of 53 French firms holding $\frac{1}{6}$ of gross physical assets of stock-issuing corporations, he concludes that the firms financed most of their investment from internally generated funds, retained earnings and depreciation, rather than through the capital market. This practice leads to variations of gross investment which magnify the problems of fluctuation in the French economy.

During periods of full employment and rising prices, the firms absorbed short-term funds which they repaid during periods of recession. To prevent this, Malissen suggests that short-term credits be more effectively rationed during periods of full employment and that medium-term credits be made more available during periods of low activity. For such a program to be effective, cooperation of businessmen and recognition of their social responsi-

bility are seen as a precondition. This exhortation and specific requests for particular monetary policies summarize the major policy recommendations of the concluding chapter.

Essentially, Malissen's procedure is to consider the 53 firms as a whole and by groups corresponding to 6 industrial classifications (steel, aluminum and nonferrous metals, metal working and machine tools, automobiles, electrical equipment, cement, and chemical). Data were obtained from the published balance sheets of the firms and from reports to the stockholders. Sales figures or other measures of activity for individual firms are obtainable for a small but increasing number of companies. Flow variables were obtained by converting balance sheets into statements of sources and uses and taking first differences. Assets were revalued by the firms at various times to adjust for inflationary changes. Further adjustments are made by the author to correct all sources and uses of funds for price changes.

Problems associated with the accuracy of data are not overlooked, but they do not prevent him from asking and attempting to answer questions which could not be meaningfully answered with the available materials: Do consumers, workers, and stockholders gain or lose from the policy of financing investment out of retained earnings? Did firms gain or lose from the inflation as a result of inventory speculation? Is the relationship between the total stock of capital and level of capacity output linear?

Elsewhere, Malissen examines the relationship between gross investment and the level of activity and obtains high correlation coefficients, but he neglects to present any measures for the reliability of his regression coefficients. In view of the problems which other economists have had in attempting to explain investment as a function of a single variable, it as difficult to understand this omission as it is to interpret the results.

A great deal can be gained from analyses of investment decisions for particular industries. The author is to be complimented for attempting this study in the face of the many problems of obtaining information. When his analysis is confined to questions concerning the relationship between variables or methods of measuring changes in real variables, he has much to say that is of interest.

There is very little discussion of the capital markets, the nature of which is closely related to the problems discussed and the recommendations made.

ALLAN H. MELTZER

Carnegie Institute of Technology

Business Organization; Managerial Economics; Marketing; Accounting

Selling in Our Economy: An Economic and Social Analysis of Selling and Advertising. By HARRY R. TOSDAL. Homewood, Illinois: Richard D. Irwin, 1957. Pp. xi, 333. $5.00.

In this interesting volume, Professor Tosdal, now in emeritus status, presents an appraisal and rationalization of selling based upon his more than 35 years of teaching and writing in the field of sales management at the Harvard

Graduate School of Business Administration. The jacket blurb suggests that the writing is aimed more largely at sales and advertising executives than at academic scholars. Teachers and students of marketing together with many economists will, however, find much of interest in this mature expression of judgment by a leading scholar in this field.

Selling is defined as "persuasive leadership to bring about buying action" (p. 10). Repeatedly selling is characterized as an energizing influence or "energizer" for a high-level economy. The rationalization runs as follows. Although selling is exercised on behalf of sellers, its ultimate objective and impact is to enhance material welfare and make for high and rising standards of living. Consumer demand, however, is the central "energizing force" in the American economy (p. 51). Selling influences consumer demand so that it is not merely a "vague general desire" but a "specific dynamic force, a moving desire for improvement as embodied in specific material goods and resources" (p. 51). Although there is some buyer-initiative "our present economic system is predominantly based on seller initiative in producing and distributing goods" (p. 243).

It is further contended that production and distribution based on aggressive buyer initiative could not serve the public welfare as well as our present seller-energized system. "More human energy and time would be required" (p. 251) to make the minimum contactual relations, and the beneficial impacts of selling upon innovation, invention, technology, economies of scale and productivity in general would to a considerable extent be lost.

Tosdal does not overlook abuses, limitations, and possible correctives. Nor does he contend that selling unaided accomplishes the miracle of high and advancing prosperity and standards of living. Natural resources, man-power and a favorable economic and social climate also play their parts. His basic contention is simply that "selling effort is the energizer of our economic machine" (p. 328); and "a chain reaction is started that energizes the whole economic system" (p. 323). Without sellers' persuasive leadership desires would not be intense enough to maintain and advance a high-level economy.

Economists will be especially interested in the chapter on "Selling and Economic Theory" which opens with the statement: "With few exceptions professional economists do not recognize selling as an important economic function" (p. 265). He explains that alleged "neglect" or indifference in terms of (1) lack of personal contact with much of selling, (2) basing judgments upon abuses in selling, (3) antagonism to endeavors to influence buying, (4) the illusion that people act rationally, (5) the exclusion of psychological motivation from economic analysis (pp. 265, 266) and (6) "the persistence of the feeling among academic men that 'trade' is not quite respectable" (p. 269). In spite of occasional references to recent writings it seems evident that Tosdal's reaction and discussion is oriented too largely to the literature of earlier decades.

It seems evident, too, that some of the more recent writings have tended to make him, perhaps, overly sensitive to criticism. Academic scholars in the field

of marketing and selling do feel at times that they are carrying a form of white man's burden on the fringes of economic analysis! It is to be regretted that Tosdal did not take the opportunity to relate his own interpretation more rigorously to recent and current developments in the theory of monopolistic competition, product differentiation, selling costs and market power and control. Perhaps it was believed that such analysis was inappropriate to a volume aimed primarily at an audience of business executives. But there are bows in the direction of the recent literature, including specific reference to "imperfect" or "monopolistic competition" and selling and market control. Further, there are numerous generalizations concerning the role of selling in a free-enterprise economy and public regulation that could be related directly to the sizable body of literature. Tosdal chose to define selling narrowly in such a way as to force him to deal directly with the aspects of market transactions usually under most criticism; viz, the persuasive leadership impacts. But this is also, to a considerable extent, the terrain of the economic analysis of product differentiation and selling costs, and of important issues in the endeavor to maintain effective competition in our markets. It would, of course, be quite a task to come to grips with this large literature and admittedly would have introduced a different note into the writing.

Furthermore, the emphasis selected by Tosdal would have provided opportunity for reference to a widely different but important body of emerging concepts, notions and exploratory research. Individual and group processes that result in buying decisions are being illuminated by studies of search behavior by consumers, influence patterns and learning theory. Such approaches are intellectually rewarding and challenge economists to incorporate their findings into models of economic behavior. Tosdal does mention in passing the Katona and Likert studies of consumer behavior (p. 60).

Tosdal also avoids the drama and interest involved in some of the grand strategy and conflicts in marketing and selling. The reviewer agrees that marketing in the American economy is basically seller-oriented, especially by manufacturer-sellers. One must agree, too, that thus far in this country, the consumer cooperatives have been relatively minor factors. Undoubtedly, too, there would be a net loss of innovative spark and persuasive zeal if our marketing system were completely buyer-oriented. On the other hand, Tosdal, in this reviewer's judgment, tends to understress the acutal and potential influences emanating from consumer-buyers and professional buyers. Further, he overlooks the significance of the basic clash in American marketing between the aggressive programs pushing downstream from the manufacturer level and those reaching upstream from the retailer-consumer end of the marketing channel.

Regardless of differences in view or of wishes for additional analysis or variation of stress, one must be grateful to the author for a clear-cut exposition of his thesis.

E. T. GRETHER

University of California

Industrial Organization; Government and Business; Industry Studies

Monopoly Problems in Regulated Industries—Airlines. Hearings Before the Antitrust Subcommittee of the House Committee on the Judiciary, 84th Cong., 2nd Sess., Feb., Mar., May, June, 1956. Pt. 1. Washington: Supt. Docs., 1956. 4 vols.

The four volumes of the Hearings, dealing with monopoly practices in the commercial air transportation industry, contain much superfluous and repetitious information which makes them both difficult and burdensome to read. Too often minor points are overstressed in hopes of bringing to light irregularities to the neglect of more significant and fundamental issues. Diverse opinions are brought out in the testimonies presented by witnesses representing the domestic scheduled and nonscheduled carriers, international carriers and related activities, federal agencies, travel bureaus, ticket agencies and trade associations. Testimonies were heard also from academic specialists in the field of air transportation.

The primary objective of the Subcommittee was to investigate the extent to which the Civil Aeronautics Board had or had not exercised the powers vested in it by Congress to prevent the growth of monopoly practices in the commercial air transportation industry of the United States. It was essential for the Subcommittee, in the hearings, to examine many of the basic problems confronting the industry.

One of the problems centered about the power of the Civil Aeronautics Board to restrict entry into the domestic trunkline operations. This was subjected to extensive and searching examination by the Subcommittee in its effort to determine the extent to which this policy, as interpreted by the Board in its decisions, had caused the exclusion of new and eligible applicants and nonscheduled carriers seeking such operating rights. This issue involved the extent to which the actions of the Board in entry cases had prevented the development of constructive competition as intended by Congress in the Civil Aeronautics Act.

The Subcommittee endeavored to determine if the members of the Board had been influenced unduly in their decisions by pressures from large air carriers and, if so, the extent to which the effectiveness of the Board had been impaired in carrying out the intent of Congress with respect to the antitrust laws.

The question of which federal agency had primary jurisdiction over antitrust policies as related to commercial air transportation was raised in the hearings. The Subcommittee was concerned if the lack of clarification as to primary jurisdiction had made for judicial noninterference and, if so, the extent to which this had resulted in providing immunity from antitrust laws and the superseding of these laws by the regulatory rulings of the Civil Aeronautics Board.

Considerable time was given in the hearings to the manner in which the Board had exercised its rate-making powers. This was to be expected in view of the fact that the Board had not carried out a formal passenger fare investi-

gation during the seventeen years of its existence, and had canceled a proposed passenger fare investigation under rather unusual circumstances.

The four volumes of the Hearings provide a useful and ready source of information for those who are interested in the basic problems underlying the development of effective regulatory policies in the commercial air transporta-' tion industry in the United States. They serve to bring together a wide range of pertinent facts and opinions, including diverse viewpoints, statistical data, leading cases and decisions, special studies and reports.

<div align="right">J. C. D. BLAINE</div>

University of North Carolina

The Regulation of Rail-Motor Rate Competition. By ERNEST W. WILLIAMS, JR. New York: Harper & Brothers, 1957. Pp. ix, 247. $4.50.

In 1935 Congress passed the Motor Carrier Act (now Part II of the Interstate Commerce Act). This Act gave to the Interstate Commerce Commission regulatory powers over motor carriers as well as railroads.

Williams' purpose is to evaluate the Commission's performance in exercising its authority over both sides of intercarrier competition since 1935. This evaluation is made by analyzing the substantive content of reported ICC decisions relating to the rates of the two types of carriers. He raises questions concerning the adequacy of the Commission's performance, and offers suggestions for improvement therein.

Chapter 1 suggests the nature of the regulatory difficulties posed by the emergence of a motor transport industry of different economic structure, and with different public responsibilities, from those of the railroads. Also discussed briefly are the provisions of the law presently relating to intercarrier competition, and the manner in which the Commission exercises its authority over the rates of the two types of carriers.

Chapters 2 and 3 are concerned with the lawfulness of "motor-compelled" rates established by the railroads in the exercise of their right to meet competition. In addition to other factors affecting their lawfulness, such rates must be no lower than is necessary to meet competition, and must be "compensatory." "Meeting competition" generally has meant rates on a par with the competitor's rates (except where the rate is meant to offset service differences). The Commission has stated no general rule for determining when rates are "compensatory." The circumstances of the individual case apparently govern the decision.

Chapter 4 deals with Commission decisions on railroad requests for fourth-section relief (relief from the long- and short-haul provisions of the law) in setting rates designed to meet motor-carrier competition. Chapters 5 and 6 are concerned with the Commission's control of motor-carrier rates—Chapter 5 with decisions regarding the lawfulness of such rates, Chapter 6 with the Commission's (sparing) use of its power to establish motor-carrier minimum rates. The Act states that regulation of the two types of carrier is to be conducted so as to preserve the "inherent advantages" of each type. Chapter 7 is concerned with the difficulties the Commission faces in determining the "proper" relation of the rates of the two carriers in light of this mandate.

The final chapter appraises the Commission's performance and suggests improvements which it is believed would make for a constructive Commission policy. In Williams' opinion the Commission has taken a negative view of its responsibilities—it has been reluctant either to act on its own initiative or to set policy. While, from an economic standpoint, the public interest would require that any given transport function be performed by the low-cost carrier, the Commission apparently has attempted to maintain each carrier in the full range of services it occupied at the time the Act was passed. The Commission has not knowingly forced either type of carrier out of any segment of transport by the exercise of its rate power. Williams believes that the Commission should be more willing to set policy and should, in its decisions, balance the value of maintaining alternatives for shippers against the advantages to be obtained from concentrating traffic on the low-cost carrier.

The readability of the book is reduced by the extensive use of material quoted directly from ICC decisions. The result, at times, is that the significant points become lost in a mass of quoted detail with the reader left wondering, "Who's on first?" Considerable improvement would have resulted if much of this material had been relegated to footnotes, with the body of the text used for more concise development of the points the author wished to make.

Williams states that it is not his purpose to "attempt here any extended discussion of the economics of either rail or motor transport" (p. 7). The result is that, while economic considerations are touched upon, very little economic analysis is undertaken. It is stated, for example, that although the economic character of the motor industry is such as to suggest the possibility of workable competition, there has been no tendency for the industry to reach an equilibrium when unregulated (p. 225). No real analysis is undertaken, however, to explain why (or if) this is the case.

The book will be of little interest to those concerned with applying the tools of economic analysis to the problems besetting the nation's transport industries. It may be of some use as supplementary reading material in certain transportation courses where the purpose is to impart to the student an understanding of regulatory procedures.

J. F. BARRON

University of California, Los Angeles

Motor Transportation. By WILLIAM J. HUDSON and JAMES A. CONSTANTIN. New York: Ronald Press, 1958. Pp. ix, 703. $7.50.

The authors of this comprehensive volume state in the preface that they have designed this book for college students and for motor-carrier personnel and industrial traffic managers who want a clear understanding of the problems and policies involved in motor-carrier operations. To a considerable extent this goal has been accomplished. However, members of each group who use the book for their own special needs may wish the authors had been less ambitious. For example, much of the detail relating to operations is not suitable for college use except in the most specialized type of course.

The early sections relating to the development of the modern highway system are especially strong, analytical, and soundly annotated. The authors put

particular emphasis upon highway development as basic to the motor-transport industry, an obvious point, but one often overlooked by writers who treat the highway problem in a more detached fashion.

It is also refreshing to note the relatively brief treatment given to the early development of the technological aspects of motor transport. The authors apparently recognize that the student will have had ample opportunity to become familiar with these developments through general reading or through previous courses. Part I also contains an exhaustive analysis of the place of the motor carrier in the social and economic organization of the nation. As in certain other portions of the book, the material relating the motor-carrier industry to various social and economic functions seems an unnecessarily detailed reiteration of the obvious.

Part II, Motor Carrier Operations, begins with an analysis of the economic characteristics of the motor-carrier industry. The authors state that they have tried to keep this section broad in scope in view of the wide diversity of motor-carrier operating procedures. This part of the book is weakened by the necessity of describing operations in sufficient detail to be useful for general students, while at the same time including material of value to motor-carrier personnel. Neither group is likely to be completely satisfied, and it is probably impossible to reconcile the needs of both groups in a single text.

While the economic characteristics are set forth in a workmanlike manner with suitable graphs and charts, the space devoted to this important area is equaled by that devoted to the selection of equipment and by the area discussing terminals and materials handling. From the standpoint of classroom use, one might wish that more space had been devoted to an intensive analysis of the economic factors at the expense of some of the more obvious operating problems. The remainder of Part II is devoted to an analysis of motor-carrier operations, most of which apparently was written with motor-carrier personnel in mind. The scholarly appearance of this section is somewhat impaired by the illustrations, some of which seem hardly suitable to a college-level text. The chapters on liability and claims and on freight classification and rates are both well done, although much of this material will represent duplication for students of transportation or traffic management. While Part II contains interesting material not readily available in any other single source, it is likely that considerable screening would have to be done in order to use the text in college-level classes.

Part III, devoted to motor-carrier regulation, is perhaps the most valuable portion of the book and could be profitably used to supplement regulatory material in any transportation course dealing with motor carriers. The discussion relating to the regulation of contract, private, and exempt carriers is especially useful in relation to the latter two categories. It includes a gratifying amount of recent material concerning the policies and problems of private and exempt carrier operations. The authors are especially to be commended for their treatment of the relationship between federal and state motor-carrier regulation.

The book is useful and well written. The emphasis placed upon motor-carrier operations and managerial problems makes it more useful for training of motor-carrier personnel than for college students in the typical under-

graduate course. The extensive annotation and the use of recent data make
the book an invaluable tool to those who wish to investigate some facet of
motor transportation in more detail. From this standpoint it would be a useful
addition to the library of transportation students and economists interested in
the general field of transportation.

HUGH S. NORTON

University of Tennessee

Public Utilities in American Capitalism. By MARTIN G. GLAESER. New York:
        Macmillan Co., 1957. Pp. xiii, 624. $7.50.

This text reflects Professor Glaeser's many years of learning, writing, and
teaching in the field of public utilities. But he goes beyond the conventional
utilities such as electricity, gas and communications, and also treats the trans-
port agencies. Many, although not all, of the problems of regulation are the
same, and a large measure of economy is effected by dealing with public
utilities and transportation in a single volume.

The subject matter is developed under three broad headings: economic and
institutional foundations, administrative aspects of public utility regulation,
and planning and coordination. Using an institutional and historical approach
in the tradition of John R. Commons, the author traces the technological and
economic development of each of the industries and the evolution of public
control within each of three time periods—the promotional epoch to 1860,
the competitive epoch to 1900, and the monopolistic and planning epoch in
the twentieth century. While the dominant characteristic of each period is thus
emphasized, the discontinuity in the treatment of individual utilities leaves
something to be desired. Supplementing the historical chronology, although
not abandoning the method completely, are interesting chapters on our energy
economy, the life history, accounting and financial aspects of "going con-
cerns," the evolution of the public utility concept and the control of the
market, and the constitutional basis of public utility regulation. The author
leaves little unsaid concerning the roots of public utility enterprises and the
growth of legal doctrine. He finds that the essence of the public utility prob-
lem from the institutional point of view "resides in the degree of essentiality
of the service and the degree of control of the market accorded a given con-
cern" (p. 233). With the emphasis upon the monopoly feature of electric, gas,
and communication utilities, however, he fails at times to give adequate
recognition to the place of competition in promoting control over such busi-
nesses as milk and transport. These industries do not fit neatly into the gen-
eralization that "public utility enterprises represent the monopolistic segment
of the economy where for reasons of public policy monopoly rights are accorded
them" (p. 215). Missing in the background treatment is a careful analysis
and comparison of the operating, cost, and service features of each of the
transport agencies which bear on the current problems of competition, rate
control, and coordination in the transport field.

The consideration of the administrative aspects of public utility regulation
embraces accounting and financial policies, valuation, depreciation, rate of
return, and the theory and practice of determining specific rates. In an informa-

tive and concisely written account of the administrative commission, the author finds that the main essential for success is adequate financial support and cautions against the exaggeration of the judicial function at the expense of investigational activities.

The significance of accounting principles and methods in the exercise of control is properly emphasized prior to a lengthy consideration of the rate-making function. All the facets of the valuation problem are dealt with comprehensively, including depreciation, with original cost favored over replacement cost as a standard for determining the base for the general level of rates. To assure a return on the actual investment adequate to attract and to hold capital in the face of a changing price level, a flexible rate of return is advocated. A careful explanation is made of the modern practice of fixing a differentiated rate of return based on the actual cost of debt capital and earnings-price ratios for the common equity. To what extent a variable rate of return thus calculated reflects a change in the purchasing power of money is not indicated. The author's suggestion that further study of the need for an inflation adjustment is well taken, for it is not at all apparent that the growth utilities have suffered under current regulatory practice in competing with other industries in the capital market.

Planning and coordination is the characterization given to the movement for public and cooperative ownership and the public projects for the development of regional resources. All of the regional projects are dealt with at length, perhaps with too much historical detail in the case of Los Angeles and the Colorado River. Particularly good are the chapters on the Tennessee Valley and the St. Lawrence projects. The projects are viewed as successful efforts to achieve maximum power development consistent with necessary flood protection and maximum benefits to navigation and land utilization. Power rates of the regional projects are not regarded as satisfactory yardsticks for measuring private utility rates, primarily owing to the marked inequality in tax burdens, but the promotional methods of the public agencies are seen to have had a salutary effect on private utility operations. Manifestations of the current "partnership policy" are not recognized as threats to the continued development of regional resources.

While this book provides an adequate, competent, and highly readable treatment of the economics and regulation of the electric, gas, and communication utilities, it does not live up to expectations with respect to transportation. Inadequate attention is given to rate policies and rate control, entry, consolidation, subsidy, and the whole problem of coordination in the transport industry. Except for a few pages devoted to the crisis in transport policies and the report of the Presidential Advisory Committee, transportation is treated only incidentally in the long section devoted to planning and coordination. Perhaps fuller consideration of transport problems would have created an inordinately large volume, but space could have been economized in the historical sections to produce a more useful book for the student of regulated industries.

MARTIN L. LINDAHL

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Land Economics; Agricultural Economics; Economic Geography; Housing

The World's Sugar—Progress and Policy. By V. P. Timoshenko and B. C. Swerling. Stanford: Stanford University Press, 1957. Pp. xii, 364. $6.50.

This is an excellent summary of the facts necessary for appraising national and international sugar policies, but a politico-economic analysis—also necessary for such an appraisal—is not provided. There is much to be said for presenting the story and leaving the reader to draw his own conclusions on future policy.

After a brief discussion in Part I of the demand and supply of sugar, Part II is devoted to the development of techniques in both cane and beet sugar production. Economists will be very grateful for such a clear outline of a sometimes complicated subject, but they will be less grateful for the isolated references to the works of Ricardo, Schumpeter and Galbraith, which do not really add anything to the technical discussion. The technical merits of sugar-beet production are striking; it supplies a cash crop and valuable by-products of cattle-feed; it has supported a depressed European agriculture and enabled more rational crop rotations to be introduced. On the other hand, the main technical advantage of cane sugar is its higher yield of sugar per unit of land.

However, the important economic question concerns costs, not technical merits. Cane and beet sugar are perfect substitutes, yet the former is cheaper. Nor is this cost differential likely to be eliminated by technical progress in beet sugar production; technical development in cane sugar has been equally impressive. If this is true, why is so much beet sugar produced? Why are not the underdeveloped cane-sugar countries flourishing with expanded crops, prosperous sugar-refineries, and all the other industrial development ancillary to sugar production?

Answers to these questions are in Part III, which describes the past policies of the United States, the United Kingdom (contributed by R. J. Hammond), Continental Europe and Russia. Each study maintains the very high standard set by Parts I and II, though the work remains descriptive rather than analytical. There is some justification for this in so far as political rather than economic factors have determined these policies. For political reasons, countries protect domestic refiners against imported refined sugar, protect domestic raw sugar production against imported raw sugar, and protect raw sugar imports from colonies and offshore possessions against imports from elsewhere. All the familiar arguments for protection have been used by the importing countries and it would be interesting to know whether the authors think they are valid.

The final chapter on the International Sugar Agreement of 1953 shows the absurdities which can arise when committees are substituted for free markets: high-cost production is encouraged at the expense of low-cost production; the leading importers favor high prices, and the leading exporting country favors low prices; in times of world excess supply the policies of the leading importing countries are designed to discourage consumption, while in times of excess demand they encourage consumption. Since this agreement does not deal with

the fundamental problems, the question of future policy is important. Could a completely free market, within a suitable legal framework to counteract any instability, solve them? Even if it could, is it practical politics? After reading the present book, I am inclined to answer, "Yes," to both questions. More important would be the answers of Timoshenko and Swerling. Of course, these questions raise a host of difficult problems, economic and political, but nobody is more qualified to answer them than are the authors of this book. Their unparalleled knowledge of the industry and their tactful description of existing policies lead us to hope that they will soon find time to write on future policy.

P. E. HART

University of Glasgow

Land Tenure and Land Taxation in America. By AARON M. SAKOLSKI. New York: Robert Schalkenbach Foundation, 1957. Pp. xii, 317. $3.50.

If land economists have been remiss in assimilating the growing literature in their field, Dr. Sakolski* would appear to have administered a needed corrective by undertaking a historical synthesis of our land tenure and land taxation systems. It might be said that he is concerned with America as a special case in the evolution of land institutions, for in introductory chapters he ranges over the nature of these institutions among primitive peoples and the civilizations of the ancient and medieval worlds. The broad theme running through the book is the change from a "collective concept, wherein absolute title to the soil was held by no individual or group . . . to a legal status, whereby individuals or groups, through political or economic power, were able to hold, use, transfer, and transmit its use and tenure for their own benefit or aggression, without any necessary regard for public welfare."

While in Europe this transition was slow and relatively peaceful, the New World was the scene of violent and unequal conflict between the property concepts of immigrant Europeans and those of the aborigines. Sakolski's book is largely about land disposal policies and practices from the period of early colonization to the present day, with Indian policy and land taxation and reform as minor themes. In three chapters he surveys the transfer to and subsequent modification of European—largely English—land tenure concepts in the thirteen colonies. A chapter on "Land and the American Revolution" is followed by five chapters on the period before the Homestead Act of 1862. In these chapters the author develops such topics as post-Revolutionary state-land disposal, the early history of the public domain, the Louisiana Territory, and the Preemption Act.

Historians who observe a marked contrast between colonial and early national land policies and practices may be surprised at the author's insistence upon continuity in the face of change. By focusing upon the institution of private property in land (alodial tenure in fee simple), he maintains that, though certain feudal practices of land-ownership and control had largely disappeared, an essentially aristocratic land system was perpetuated—a system

* Dr. Sakolski met his untimely death in an automobile accident on December 29, 1955, while type for his book was being set.

comprising such features as absentee ownership, land engrossment, speculation, and wasteful utilization of natural resources. Even the Homestead and subsequent acts, though ameliorative in certain respects, failed to achieve a democratic land system, he believes. For the period since 1860 the author draws heavily upon the work of Paul W. Gates and other authorities in nine chapters which discuss such topics as the disposal of agricultural, forest and mineral lands; railroad land grants; farm tenancy; and the rise of urban real estate values.

Land taxation and land reform are inextricably linked in the last three chapters of the book. After summarizing the history of land taxation in Europe and America, the author comes to the central issue of his study, namely a consideration of possible land reform policies. Here he first looks to the past for possible guides to action. Beginning with the physiocrats in France, he discusses land reform movements in Europe and America. Much the greater part of the discussion concerns Henry George and his single-tax movement. Georgian economics is treated not only from a historical standpoint, but also as a possible means of reforming our present land system. While eschewing land nationalization, Dr. Sakolski comments favorably upon the following proposals: "(1) absorption of the economic rent of land and simultaneously untaxing improvements (i.e., primary buildings) on land as advocated so earnestly by Henry George as a matter of justice; (2) progressive taxes on land; (3) limitation of private landownership; and (4) social control of land use."

In many respects this book satisfies the need for a one-volume history of land tenure and land taxation in America, considered in the broad perspective of antecedent and contemporary theories and practices. It is written in an interesting and thought-provoking manner. Excellent use has been made not only of the writings of well-known scholars and statesmen, but also of lesser-known writers—both American and European. Though much of the subject matter will be familiar to land historians, what may not be so familiar is the conscious effort to view the problem from the standpoint of land reformers, and especially the theories of Henry George. While this approach has much to commend it, it may also have certain shortcomings. Among other things one reform movement may be emphasized to the virtual neglect of others. The reform of federal land policy since the closing of the land frontier— a subject singularly neglected in this study—would appear to be a case in point.

RICHARD B. SHERIDAN

University of Kansas

The National Policy and the Wheat Economy. By VERNON C. FOWKE. Toronto: Toronto University Press, 1957. Pp. x, 312. $5.50.

This is the seventh volume in a series on the background and development of Social Credit in Alberta. Though Social Credit has held political power in the western wheat province for over twenty years, neither the party, its founders, nor its doctrines are given even a passing reference in this study of the wheat economy. This is probably not a coincidence, since Social Credit

played no part in the development of the wheat economy. Fowke leaves it to
other authors in the series to show how national policy and the development
of the wheat economy influenced the growth of the Social Credit movement in
Alberta.

National policy, as defined by Fowke for the purposes of this study, "com-
prises collectively that group of policies and instruments which were designed
to transform the British North American territories of the mid-nineteenth
century into a political and economic unit" (p. 8). This book gives both a
discussion of national policy as it related to the development of the wheat
economy and an historical and analytical treatment of grain marketing
problems in Western Canada.

The first third of the book, dealing with the origins and development of
the national policy, is largely a restatement of the author's earlier work,
Canadian Agricultural Policy. In this section he describes and evaluates the
federal tariff, transportation, land, and immigration policies as they related
to the development of the wheat economy. Fowke finds that, to be acceptable
federally, policies had to serve to tie together commercially the central and
western provinces. Alternative policies that did not promise to serve this
end were rejected even though they promised more rapid development in a
form more profitable to westerners.

In the second and third parts, comprising over one-half of the book, he
discusses two stages of the history of grain marketing in Western Canada.
The first stage involves the problems of technical and economic imperfec-
tions in the marketing structure; the second includes the long succession of
steps involved in the farmer's fight to eliminate the open market. In the first
stage Fowke detects two elements of national policy, one amenable to western
pressure—"to encourage maximum economic development," and the other
immutable—"to assure integration of western development into the national
economy" (p. 93). The accounts of the controversies which arose in grain
marketing include some of the best expositions in print of the economic im-
perfections in the developmental stage of the grain marketing system. In the
struggle to remove these imperfections more effective competition was needed.
Federal policy was amenable to producer demands in this regard.

After 1920, producer criticisms of the marketing system were directed at
the open-market system and competitive pricing. Fowke has been quite suc-
cessful in distilling, from the tidal wave of opposition to the open market,
the essence of producer criticisms of that institution. He is less successful at
finding justifications for specific criticisms. On this question he concludes
that grower demands were "diametrically opposed to the free enterprise tenets
underlying the national policy" (p. 191).

In Part IV, Fowke makes his major analytical contribution when he
evaluates developments in national policy since 1930, by which date western
development had been secured through policies initiated before 1900. He
suggests that national policy in regard to grain marketing grew out of a
philosophy of economic liberalism which accorded "equal tolerance to freedom
of combination and freedom of competition," a policy which led to "disregard
of the competitive inferiority of agriculture within the price system" (p. 290).

In an examination of federal agricultural legislation of the last quarter century
he finds no clear indication that national policy has been modified in favor of
the wheat economy. However, he limits his search to measures which affect
the "competitive excesses of agriculture." The only two measures of this
type, the Agricultural Prices Support Act and the Wheat Board Act, either
served multiple purposes or were political necessities. In Fowke's opinion, the
strongest evidence in support of the thesis that national agricultural policy
has not changed permanently lies in "its lack of theoretical or conceptual
content." Fowke provides a basis for a new policy that appears to capture
the essence of western discontent—"a clear recognition of the competitive
disabilities of agriculture within the price system" (p. 296). The adoption
of this suggestion by a major political party could, in the opinion of this
reviewer, be instrumental in effecting a change in national policy.

ARTHUR W. WOOD

University of Manitoba

Die Futtergetreidewirtschaft der Welt 1900-1954. By HANS-BRODER KROHN.
    Berichte über Landwirtschaft, N. S., Special Issue No. 165. Hamburg and
    Berlin: Paul Parey, 1957. Pp. 144. DM 12,50.

This publication presents the results of a detailed and constructive attempt
to measure in quantitative terms the usually diverging trends in the demand
for pork, beef, milk and eggs and in the production of these products and of
grains and other nonroughage crops available for animal and poultry feeding.
Statistics reflecting these trends are presented for (1) the United States, (2)
Europe, Canada, Argentina and Australia (a somewhat illogical grouping of
grain exporting and importing countries), and (3) all of these combined, for
1899-1913 and 1913-1928; for the United States and group (2) for 1929-
1939; for the United States for 1927/29-1950/53 and 1937/41-1950/53; and
for group (2) for 1934/38-1954. (Ranges for periods with multiyear begin-
nings and endings are those between the respective averages of the two termi-
nal groups of years.)

The publication consists of four main parts. In the first, the author outlines
general or basic considerations, including the determinants of the market
prices of feed grains, hogs and pork; functions of price and income elasticity
of demand for feed grains and pork; and the structural weaknesses of the
feed-grains and animal-products economy—the "frictions" which hinder the
price mechanism in closing the gap between the diverging trends, such as the
inelasticity of the supply of agricultural products, particularly during periods
of declining prices.

In Part II the influences responsible for increasing production of feed
grains and edible animal products are explained. Krohn insists that by far the
most important cause of the long-term increases in output of agricultural
products have been developments in production techniques and efficiency. He
believes that the influence of economic forces, such as prices, in this regard
has been very limited, but he admits that farmers will not adopt improved
production techniques unless the price relationships between their costs and
incomes are such as to enable them to operate in the black.

Part III names factors which in the long run cause shifts in the demand curve for all foods in general and in that for edible animal products in particular. It includes a table listing annual percentage rates of increase in the demand for pork in (1) the United States, (2) Europe, Canada, Argentina and Australia and (3) all of these areas for 1899-1913 and 1913-1929, for (1) and (2) for 1929-1939 and 1939-1954, and for only the United States for 1928-1954. These rates were estimated upon the basis of the respective percentage rates of increase in population and per capita real income, and the respective coefficients of income elasticity of demand, assuming proportional increases in supplies of pork and constant retail prices. Part III also has pertinent data on price elasticity of demand, with a discussion of the usefulness and the basic determinants of price elasticity data.

Part IV points out that in the period 1899-1913, owing primarily to a relatively slow rate of technical development, production of both feed grains and the edible animal products derived from them generally did not keep pace with the increase in demand for animal products in the areas under consideration. The increase in demand was due mainly to unusual increases in population in the United States and in per capita real income in Europe. In the period from 1913-1929, a general slowing down of economic development, particularly in the rate of increase in per capita real income, and various other influences, left the demand for animal products and the suppliers of feed grains and of animal products generally in equilibrium in these areas.

Part IV describes 1929-1939 as a period during which there was a tendency toward overproduction in feed grains, and at the end of the period surpluses had accumulated in the United States and Canada. The situation during this period in regard to incomes and population generally was such that demand for animal products increased very little, and if there had not been a number of extensive crop failures in North and South America, much larger surpluses of feed grains would have accumulated. In the United States, reductions in corn acreage by Agricultural Adjustment Administration were offset by increases in corn yields and in production of feed grains not under A.A.A. control (barley, oats and soy beans).

Krohn found that for the period 1927/29-1950/53, data in satisfactory form were available only for the United States and for this period his investigation is confined largely to this country. He states that this period is one during which the average annual rates of increase in production of animal products did not keep pace with rates of increase in feed production and in the demand for animal products. Among circumstances to which this is attributed are a significant reduction in the number of pigs on farms after the unusually low corn harvest of 1947 which was followed by a good harvest in 1948, and, most important of all, the support of the producer prices of corn above the market levels after the 1948 harvest had produced a large surplus and market prices had fallen.

In this reviewer's opinion, Krohn's brief discussions of or references to the details and effects of the programs which many countries, including his own, have in operation directly or indirectly to control producer prices and domestic production, limit imports or stimulate exports of feed grains, and the

reasons those countries have for operating those programs are too limited. Those programs certainly have done much to control the levels of production and consumption of feed grains, livestock and livestock products in these countries. With the exception of this major deficiency, this treatise is in general a very creditable job. It is amply "quantified" throughout, and includes a number of interesting charts. It shows that the author is closely familiar with the feeds and animal-products economy and is widely read in the econometrics literature of his own and other countries.

CARL F. WEHRWEIN

U. S. Department of Agriculture

Toward Responsible Government: An Economic Appraisal of Federal Investment in Water Resource Programs. By EDWARD F. RENSHAW. Chicago: Idyia Press, 1957. Pp. xii, 164. $2.50.

Federal water resource programs, which reflect complex political, social, and economic forces, are generally initiated and vigorously supported by sectional interests on the ground of improving the national well-being. Possibly because the economic justification for such undertakings is sometimes questionable, the federal agencies most active in river basin development—such as the Army Corps of Engineers, the Bureau of Reclamation, and the Department of Agriculture—have developed a type of project economic evaluation ("cost-benefit" analysis) that purports to simulate the allocating mechanism of the private economy in deciding among alternative investment opportunities. Whether the cost-benefit ratio—the final summary form of the agency analysis —was originally intended to confer the sanction of the private market upon public investment is not clear, but there is increasing evidence that agency studies do not lead to decisions that are comparable with those of the private market.

In reviewing the procedures of the Bureau of Reclamation, the Army Corps, and the Department of Agriculture for evaluating benefits and costs of selected projects, Renshaw concludes that in virtually every case these analyses have been biased in favor of public investment by a dual overstatement of project benefits and an understatement of project costs. Renshaw finds public investment in water resource programs generally carried well beyond the point where the federal undertaking achieves a return equivalent to investment in the private economy, and indeed beyond the point of a significant contribution to national output. Reclamation projects, for example, neither use resources efficiently nor add importantly to agricultural output, whereas the Army Corps is found guilty of gross exaggeration of benefits in justifying flood control projects and navigation improvements. The Soil Conservation Service, in evaluating the worth of watershed protection, suscribes to overvaluation of capital use in agriculture.

The remedy for the misallocation of resources that results from public overinvestment in water resource projects is, according to Renshaw, (1) in general to impose more rigorous standards upon federal agencies seeking Congressional appropriations and (2) in particular to strip the self-interested agency of

authority to make cost-benefit studies that influence Congressional decisions on appropriations. The establishment of an independent appraisal board is also suggested as one means of increasing the validity of project economic evaluation in some cases.

Toward Responsible Government is an impassioned criticism of federal water investment policies. An unquestioning and almost reverent acceptance of the private market standard as a basis for appraising the worth of public investment, however, leads at times to oversimplification or avoidance of important features of the water resources problem.

LAWRENCE G. HINES

Dartmouth College

Residential Finance, 1950. By RICHARD U. RATCLIFF, DANIEL B. RATHBUN, and JUNIA H. HONNOLD. A Census-Social Science Research Council Monograph. New York: John Wiley; London: Chapman & Hall, 1957. Pp. x, 180. $6.00.

Some time ago the Social Science Research Council in cooperation with the Bureau of the Census commissioned the preparation of a series of Census Monographs. This study—Volume VII in the series—deals with the financing of residential real estate.

Persons primarily interested in the last Census will find this volume a useful work of reference. In 192 tables, spread over an almost equal number of pages, it summarizes the important Census findings on the financing of all types of homes. There are cross-classifications for nearly every conceivable characteristic of mortgage borrower and lender—by type of mortgage, type of lender, and type of geographic location. And every page has descriptive text to explain the findings of the tables.

But there is not the analysis in depth, the isolation of important variables, and the boldness and insight which distinguish an exciting exploration. The authors have done a competent statistical job, but their study does not meet the broad objectives of this series. As stated in the foreword to the book, the purpose of these monographs is to analyze not merely the results of the 1950 Census, but to "include broad exploration of new questions suggested by the new information, as well as the narrowing of doubt and controversy."

The main shortcoming of this volume—its lack of concern with raising new questions and challenging hypotheses—was the inevitable result of the way the authors conceived of their assignment. In the introduction, they disclaim any attempt to single out important relationships which might illuminate the financial practices of borrowers and lenders. They admit that "detailed examination of a limited number of arbitrarily selected hypotheses would lend analytical elegance to our work and avoid the tiresome recitation of fact inherent in extensive treatment." At the same time, they insist that "such an approach would run the risk of ignoring more important material." The authors are not inclined to take this risk.

The reader whose interest centers in long-term trends and relationships will also miss comparison with earlier statistics. Except in a few places, the authors

are reluctant to introduce earlier census and noncensus statistics for fear of "straining" the data unduly. While there are, no doubt, many statistical and conceptual difficulties in reconciling previously collected data on residential financing with the latest Census results, the development of some historical perspective is indispensable to a study designed to be analytical, not merely descriptive.

The authors include no data on the changing pattern of home financing since 1950. Thus, persons engaged in current research on housing problems are likely to find this volume of only limited usefulness.

The Social Science Research Council is to be commended for underwriting extensive research on the 1950 Census. But if the results are to be worth the effort, the authors must be encouraged to work with observations over a period of time, to extricate strategic factors from the mass of data, and boldly to suggest hypotheses to explain observed trends and relationships.

WILLIAM C. FREUND

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Labor Economics

Printers and Technology; A History of the International Printing Pressmen and Assistants' Union. By ELIZABETH FAULKNER BAKER. New York: Columbia University Press, 1957. Pp. x, 545. $7.00.

Mrs. Baker's book carries us back to the heroic age of American unionism, and then brings us down again to the troublesome technological and jurisdictional problems of the present. It may be regarded as an extension and amplification of her earlier study, *The Displacement of Men by Machines,* which also related to the Pressmen and which was much consulted during the great depression. Being based very largely upon the files of *The American Pressman* and the proceedings of union conventions, and giving a fair and commendatory account of the policies and activities of the Pressmen's Union, it should go very far to fulfill the long-cherished hope of George Berry and other officers of that body for an authentic and continuous history which might be placed in the hands of apprentices and others in need of a proper background or indoctrination. Perhaps the style and method of presentation may be a trifle academic and literary for that group; but particularly for the period of Berry's presidency from 1907 to 1948, the record is often quite dramatic and enlivened by interesting personalities and quotations from debates, letters and articles. If the general reader or student of labor problems finds the treatment somewhat too detailed in places, he should reflect that the history of such an old and important organization offers, in epitome, an understanding of the whole labor movement.

Some mild quibbling might be indulged in as to the use of the word "printers" in the author's title, but only for the purpose of emphasizing the complexity of the industry and of the relationships among different crafts. In the earlier period when workmen, especially in the smaller shops, often performed more than one operation, when all or almost all were members of the

Typographical Union and dominated by the typesetters or compositors as the most numerous contingent, the general public seems to have lumped them all together under the term "printers." But with the rise of larger plants, increased division of labor, and technological progress, there was more differentiation and more dissatisfaction among the minority groups, until in 1889 the pressmen broke away and formed an autonomous union, to be followed by the bookbinders, photoengravers, and other trades. At several points in the history there are references to the relative skill required in the different branches, as to whether the average pressman could be considered a "practical printer," and a bit later as to whether in highly mechanized work like that of the web-pressman any real skill and intelligence is needed. To all this the pressmen are cited as making reply, that with so much equipment in their charge they have the greatest responsibility, and that they are the actual printers because they alone bring the type in contact with the paper and make the final product. None the less, the term as applied to the pressmen alone seems too inclusive.

More strictly on the technology side, which is the author's main concern throughout, she leaves no doubt but that the Pressmen have been subjected to a greater amount of change than the lordly compositors who were fairly immune until the coming of the linotype in 1890 and following. There was the growing use of power-presses after 1830, the revolutionary web-press in the 1890's, mechanical press-feeders after 1900, and more recently the great development of offset printing, and the breakup of the trade into specialties. Fortunately, except during periods of depression, the industry was growing rapidly and could generally take up the slack over limited periods. Along with the elasticity of demand, mention might have been made here of the concomitant improvements that were being made in the paper industry. The amount of capital involved and union-management cooperation helped to insure that changes be made more gradually. If skill was still necessary, it was of a type which implied adaptability and versatility—hence the wisdom of the Technical Trade School started in 1913 for the training and retraining of apprentices and foremen. Overcoming craft pride and rivalry, and adaptation of union structure to the altered conditions, have proved much more difficult. Berry favored federation of all the printing trades and joint bargaining. The Pressmen have given representation to the growing number of specialists. But amalgamation and industrial unionism do not seem to be making rapid progress even since the uniting of the A.F. of L. and the C.I.O.

WARREN B. CATLIN

Bowdoin College

Population; Welfare Programs; Standards of Living

Population Theories and the Economic Interpretation. By SYDNEY H. COONTZ. New York: The Humanities Press, 1957. Pp. 200. $5.00.

The purpose of S. H. Coontz's book—a doctoral dissertation written with D. V. Glass at the London School of Economics—is to put the analysis of population growth under the law of supply and demand, and thus to return population theory to its natural habitat, the field of economics. In addition

to its theoretical contribution the book is an exercise in polemics against those theorists who do not agree with this view. Population is seen as a dependent variable in that "both long-run and short-run changes in fertility are a function of changes in the demand for labor" (p. 195). The fundamental "determinant of fertility patterns is demand for labor and not democratization of the knowledge of efficient means of contraception."

The work thus represents a theoretical gloss on one of the less inspired dicta in Adam Smith's *Wealth of Nations* (Cannan edition, New York 1937, p. 80): namely, "that the demand for men, like that for any other commodity, necessarily regulates the production of men . . . and determines the state of propagation in all of the different countries of the world."

To establish this interpretation, the author makes quick dispatch of biological theories represented among others, by Raymond Pearl, J. de Castro, and Herbert Spencer, and of cultural theories represented, for example, by A. Dumont and Frank Fetter. Malthus, he holds, equates the demand for labor with the means of subsistence, thus drawing conclusions much more pessimistic than justified by analysis of the demand factor. Coontz finds some support for his position among classical economists, little among the neoclassical school and most of all among the Marxists, partly because the Marxists dispute the Malthusian position. Of the great theorists, Coontz cites Alfred Marshall as best supporting his position.

The author does not contend that economists have done the analysis necessary to establish labor demand as the force back of the birth rate. But from the work they have done and left undone, the author formulates his own interpretation. It is best seen in Chapters 6 and 8 and may be summarized as follows: Ideally a theory of population should explain both historic changes in fertility and class differences and provide the basis for predicting future fertility patterns, given the stage of economic development and the type of social organization (p. 13). This can be done in a demand-for-labor analysis. Increases in aggregate demand will increase births. Decreases in total demand or increases in demand for higher quality labor (cost factor) will decrease births. Differential fertility is due to the differences between raising more expensive and less expensive units of labor for the market. The Industrial Revolution furnished the great population increase of transitional growth (1) by increasing the aggregate demand for labor and (2) by lowering the quality demanded (women and children wanted in mines and factories). In capitalist countries a tendency exists for wages to equal the cost of production of labor power.

In the demand-for-labor analysis, a vast immigration substitutes for fertility, and in the United States fertility has thus fallen in spite of economic progress because the demand for aggregate labor supply was met by immigration and the demand for increased quality (which raises cost) lowered the birth rate of the native population. Decline in mortality also substitutes for increased fertility in meeting labor demand. Continued fertility decline, however, is a function of the slowing down of the rate of economic progress and we are left with the thought that the failure of recent population projections was due

to the fact that they involved a passive assumption of continued decline in the rate of economic expansion.

Coontz, it seems to me, is in position to make a specific prediction as to employment rates eighteen to twenty years hence. Given the rising birth rate as a response to long-run labor demands, we should expect high rates of employment in 1979-1990. The author does not make this prediction, but it is to his credit that he strives with the problem of short-run and long-run analysis and—unlike Joseph S. Davis after decrying the misguided population projectionists—he manfully strives to show where they failed in not expecting an increased demand for labor. This is anything but an agnostic position, and I for one admire him for taking it. One of Coontz's greatest regrets is that notable economists like J. M. Keynes, Paul Sweezy, A. H. Hansen and George Terborgh have been led down the garden path by following the projections of W. S. Thompson and P. K. Whelpton. If they had analyzed the demand for labor rather than giving way to raw empiricism, they, it is thought, could have avoided these fallacies.

On the other hand it must be said that the support for this heavy theoretical apparatus is slight indeed. There is very little empirical analysis in the book; not a single demand and supply curve is given in evidence to show the effect of labor demand on birth rates. It is interesting to compare this theoretical approach from economics with the empirical approach from demography that Whelpton used in his *Cohort Fertility* (Princeton 1954) in which he found births postponed, made up, and anticipated in line with the schedule of depression and prosperity. An analysis of this data would, it seems to me, offer some of the empirical support which Coontz's position so greatly needs.

What is the conclusion of the whole matter? First, one can say that in the unsure and ambiguous theoretical footing on which demography now stands, we are allowed to be grateful for a work which dares to take a stand and be controversial in so doing. But is the position well taken? Is it established by the analysis? To these questions the reviewer, if he is honest with himself, must reply with the verdict that J. D. Bury once rendered on *The Idea of Progress* (1921). "It is true or it is false," he wrote, "and [like the doctrine of Providence or personal immortality] cannot be proved true or false. Belief in it is an act of faith." Coontz has that faith.

In passing it can be pointed out that some of Coontz's statements will bear citation in refutation of his general point of view. Thus he writes: "A demand-for-labor analysis is required for a further understanding of family limitations among the poor—why among the propertyless masses, the group least able to afford children, make a relatively greater contribution" (p. 100). In a footnote elsewhere he states: "It should be obvious that the economic interpretation . . . does not assume an existing economic motive for procreation" (p. 152).

Economic determinism, under which the labor-demand theory falls, is, it seems to me, a special case of psychological theory in which man's actions are seen as rational with his responses especially sensitive to one class of stimuli— the economic. The trouble with transferring the profit-and-loss calculus of

supply and demand to population growth—"the production of men in response to the demand for men"—is that another kind of hedonistic calculus is involved, that is, the affectional-sexual relations of the human species. It would take Freud and Marx together to resolve this calculus and that is precisely the advance that Malthus made over Adam Smith. In teaching population courses, demographers have looked for ideological opponents on whom they might exercise their proclivities for polemics. In writing this book Coontz has, I believe, furnished this object. If so, shall we sit back and watch the straw fly?

RUPERT B. VANCE

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TITLES OF NEW BOOKS

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GROSSMAN, M. C., HANSEN, R. R., HENDRIKSEN, E. S., AND OTHERS, ed. Readings in current economics. Homewood, Ill.: Irwin, 1958. Pp. xii, 452. $395.

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METAXAS, B. N. Introduction to mathematical economic analysis. (In Greek.) Athens: Author, 1955. Pp. 217.

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"It is about time that scientists should point out the fallacy of existing economic theories, and thus help economists to formulate a new theory which corresponds more closely to actual contemporary conditions.
"The present work has in view a two-fold purpose: first, to acquaint scientists with some economic theories; second, to draw the attention of the economists to their fallacy of disregarding science. It will also point out ways in which modern economic concepts might be altered or modified in the light of scientific development." (From the introduction.)

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NOTES

SEVENTY-FIRST ANNUAL MEETING OF THE AMERICAN
ECONOMIC ASSOCIATION

Palmer House, Chicago, Illinois, December 27-29, 1958

Preliminary Announcement of the Program, July 15, 1958

The central theme about which most of the papers are organized is "The United States
Economy in a World of Competitive Co-existence." The development of this theme in-
cludes papers analyzing the prospects and problems of the communistic economies, the
problems facing underdeveloped countries and the American policy towards them, and the
problem of maintaining a strong and stable American economy without sacrificing the free
enterprise system.

Friday, December 26, 1958

6:30 P.M. Executive Committee Dinner Meeting

Saturday, December 27, 1958

9:30 A.M. SOVIET ECONOMIC TRENDS AND PROSPECTS
        Chairman: ABRAM BERGSON, Harvard University
        Papers: Industrial Growth
                FRANCIS SETON, Oxford University
            Agriculture under Krushchev
                LAZAR VOLIN, U. S. Department of Agriculture
            Soviet Foreign Economic Competition
                JOSEPH BERLINER, Syracuse University
        Discussants: RAYMOND P. POWELL, Yale University
                ROBERT L. ALLEN, University of Virginia
                CHAUNCEY HARRIS, University of Chicago

9:30 A.M. THE FUNDAMENTALS OF ECONOMIC PROGRESS IN UNDERDEVELOPED COUNTRIES
        Chairman: THEODORE W. SCHULTZ, University of Chicago
        Papers: Using the Resources at Hand More Efficiently
                ARNOLD C. HARBERGER, University of Chicago
            Adding to the Stock of Physical and Human Capital
                RICHARD B. GOODE, International Monetary Fund
            Maintaining Order and Accommodating Economic Change
                WILLIAM H. NICHOLLS, Vanderbilt University
        Discussants: BENJAMIN HIGGINS, Massachusetts Institute of Technology
                ALEXANDRE KAFKA, United Nations
                GEORGE E. BRITWELL, University of Saskatchewan

2:30 P.M. SOVIET ECONOMIC PLANNING
        Chairman: EVSEY D. DOMAR, Massachusetts Institute of Technology
        Papers: Industrial Price Formation
                GREGORY GROSSMAN, University of California
            Industrial Reorganization of 1957
                OLEG HOEFFDING, Rand Corporation
        Discussants: ROBERT CAMPBELL, University of Southern California
                DAVID GRANICK, Carnegie Institute of Technology

2:30 P.M. THE ROLE AND CHARACTER OF FOREIGN AID
        Chairman: WILLARD L. THORP, Amherst College
        Papers: Problems of Foreign Aid Viewed from the Inside
                C. TYLER WOOD, International Cooperation Administration
                Problems of Foreign Aid Viewed from the Outside
                HARLAN CLEVELAND, Maxwell Graduate School of Citizenship and
                        Public Affairs
                Agricultural Surplus and Foreign Aid
                JOHN H. DAVIS, Harvard University
        Discussants: To be announced

2:30 P.M. ROUND TABLE ON THE ORGANIZATION AND FINANCING OF ECONOMIC RESEARCH
        Chairman: SEYMOUR E. HARRIS, Harvard University
        Participants: THOMAS H. CARROLL, Ford Foundation
                PHILIP H. COOMBS, Fund for Advancement of Education
                SOLOMON FABRICANT, National Bureau of Economic Research
                TJALLING KOOPMANS, Yale University
                LLOYD REYNOLDS, Yale University
        Discussion from the floor.

5:00 P.M. Cocktail Hour

8:00 P.M. THE NON-RUSSIAN COMMUNIST ECONOMIES
        Chairman: MAX F. MILLIKAN, Massachusetts Institute of Technology
        Papers: Structural Changes in the Economy of the Chinese Mainland, 1933-
                1957
                TA-CHUNG LIU, Rand Corporation and Cornell University
                The Polish Economy after October 1956
                ALEXANDER ERLICH, Columbia University
                Integration of European Satellite Economies with the Russian Econ-
                omy
                EDWARD AMES, Purdue University
        Discussants: NICOLAS SPULBER, Indiana University
                WILLIAM W. HOLLISTER, Washington, D.C.
                THAD P. ALTON, Columbia University

8:00 P.M. MAINTAINING FULL EMPLOYMENT AND ECONOMIC STABILITY
        Chairman: ROBERT AARON GORDON, University of California, Berkeley
        Papers: The 1957-58 Business Contraction in the Light of its Predecessors
                GEOFFREY H. MOORE, National Bureau of Economic Research
                The Problem of Price Stabilization
                JOHN P. LEWIS, Indiana University
        Discussants: MARTIN R. GAINSBRUGH, National Industrial Conference Board
                J. HOWARD CRAVEN, Bank of America, San Francisco
                FRANK E. NORTON, University of California, Los Angeles

8:00 P.M. THE ECONOMICS OF GOVERNMENT EXPENDITURES
        Chairman: CHARLES J. HITCH, Rand Corporation
        Papers: Using Market Mechanisms in Making Government Expenditure
                Decisions
                O. H. BROWNLEE, University of Minnesota
                An Economist Looks at Defense
                ALBERT WOHLSTETTER, Rand Corporation
        Discussants: KLAUS E. KNORR, Princeton University
                JACK HIRSHLEIFER, University of Chicago

9:00 A.M. STUDIES IN THE CLASSICAL ECONOMICS
        Chairman: JOHN PERRY MILLER, Yale University
        Papers: What Was the Labor Theory of Value?
                DONALD F. GORDON, University of Washington
              The Relevance of Classical and Contemporary Theories of Growth
                to Economic Development
                JOHN M. LETICHE, University of California, Berkeley
        Discussants: ROBERT E. BALDWIN, University of California, Los Angeles
                WILLIAM J. FELLNER, Yale University
                FRIEDRICH A. HAYEK, University of Chicago

9:00 A.M. POWER BLOCS AND THE OPERATION OF ECONOMIC FORCES
        Chairman: GEORGE J. STIGLER, University of Chicago
        Papers: Economics by Admonition
                BEN W. LEWIS, Oberlin College
              Economics by Negotiation
                GEORGE H. HILDEBRAND, University of California, Los Angeles
        Discussants: CALVIN B. HOOVER, Duke University
                Second discussant to be announced.

9:00 A.M. SPECIAL PROBLEMS FACING UNDERDEVELOPED COUNTRIES
        Chairman: KENT T. HEALY, Yale University
        Papers: Transportation's Role in Economic Development
                WILFRED OWEN, Brookings Institution
              Labor Force Development in Industrializing Countries
                BEN S. STEPHANSKY, Bureau of Inter-American Affairs, Depart-
                    ment of State
        Discussants: To be announced.

2:30 P.M. ADMINISTERED PRICES RECONSIDERED
        Chairman: FRITZ MACHLUP, Johns Hopkins University
        Papers: Administered Prices and the Inflationary Process
                GARDNER ACKLEY, University of Michigan
              Administered Prices: A Phenomenon in Search of a Theory
                JOHN M. BLAIR, Chief Economist, Subcommittee on Antitrust and
                    Monopoly, Committee on the Judiciary, U. S. Senate
        Discussants: RICHARD T. SELDEN, Vanderbilt University
                MARTIN J. BAILEY, University of Chicago
                WROE ANDERSON, Alderson Associates, Inc., Philadelphia, Penn-
                    sylvania
                GARDINER C. MEANS, Washington, D. C.

2:30 P.M. INTERNATIONAL TRADE AND PAYMENTS IN AN ERA OF CO-EXISTENCE
        Chairman: GARDNER PATTERSON, Princeton University
        Papers: Commercial Policy in the Underdeveloped Countries
                RAOUL PREBISCH, United Nations, Economic Commission for
                    Latin America
              Disequilibrium in the Balance of Payments
                RANDALL W. HINSHAW, Oberlin College
        Discussants: HENRY G. AUBREY, National Planning Association
                EUGENE R. SCHLESINGER, New York University

2:30 P.M. REGIONAL ECONOMICS AND INDUSTRIAL LOCATION (Joint session with Regional
            Science Association)
        Chairman: To be announced
        Papers: A Statistical and Analytical Technique for Regional Analysis
                EDGAR S. DUNN, University of Florida

Changes in the Location of United States Manufacturing Since 1929
VICTOR FUCHS, Columbia University
Discussants: To be announced

8:00 P.M. PRESIDENTIAL ADDRESS
Chairman: To be announced
Presidential Address
GEORGE W. STOCKING, Vanderbilt University

Monday, December 29, 1958

9:00 A.M. BALANCED ECONOMIC GROWTH IN HISTORY: A CRITIQUE (Joint session with
Economic History Association)
Chairman: ALEXANDER GERSCHENKRON, Harvard University
Papers: Foreign Trade and Balanced Economic Growth: The Historical
Framework
JONATHAN R. T. HUGHES, Columbia and Purdue University
Domestic Aspects of Balanced Economic Growth in History
GORAN OHLIN, Stanford University
Discussants: RUDOLPH C. BLITZ, Vanderbilt University
THEODORE W. MORGAN, University of Wisconsin

9:00 A.M. CURRENT CRITICAL ISSUES IN WAGE THEORY AND PRACTICE (Joint session
with the Industrial Relations Research Association)
Chairman: JOSEPH SHISTER, University of Buffalo
Papers: Wage Structure and Resource Allocation
HARRY M. DOUTY, U. S. Bureau of Labor Statistics
Wage-Push Inflation
WALTER A. MORTON, University of Wisconsin
Wage Policy and Business Activity
To be announced.
Discussants: To be announced.

9:00 A.M. Joint session with American Farm Economic Association
Chairman: ROBERT L. CLODIUS, University of Wisconsin
Details to be announced

9:00 A.M. SELECTED PAPERS—OPEN COMPETITION
Chairman: WILLIAM J. FELLNER, Yale University
Papers: To be announced

12:30 P.M. Joint Luncheon Session with American Statistical Association
Chairman: RALPH E. BURGESS, American Cyanamid Company
Address: The Economic Outlook
To be announced

2:30 P.M. SELECTED PROBLEMS IN ECONOMIC THEORY (Joint Session with Econometric
Society)
Chairman: EDWIN KUH, Massachusetts Institute of Technology
Papers: Economic Growth, Fluctuation, and Stability
ALBERT K. ANDO, Massachusetts Institute of Technology
FRANCO MODIGLIANI, Carnegie Institute of Technology
The Demand for Money—Some Theoretical and Empirical Results
MILTON FRIEDMAN, University of Chicago
Discussants: To be announced

2:45 P.M. CURRENT TRENDS IN PUBLIC POLICY TOWARD BUSINESS (Joint session with
American Marketing Association)

Chairman: CORWIN D. EDWARDS, University of Chicago
Papers: The Present Legal Environment of Growth Through Merger
PAUL W. COOK, JR., Harvard University
The Law and Economics of Price Discrimination—A Reappraisal
MARK MASSEL, Brookings Institution
The Role of Cost in Public Policy Towards Business
H. F. TAGGART, University of Michigan
Discussants: HOWARD WESTING, University of Wisconsin
JOHN S. McGEE, University of Chicago
JOHN T. WHEELER, University of California, Berkeley

5:00 P.M. Business Meeting
6:00 P.M. Executive Committee Dinner Meeting

AMENDMENTS TO THE BYLAWS OF THE AMERICAN ECONOMIC ASSOCIATION

The following proposed amendments were approved at the Executive Committee meeting of the Association, April 4-5, 1958.

Section III. *Officers.*

Amend Paragraph 1 to read:

1. The Association shall have the following officers who shall be elective officers: a President, a First Vice-President, two additional Vice-Presidents, and six elected members of the Executive Committee. The terms of office of the President and the three Vice-Presidents shall each be one year. The terms of office of the six elected members of the Executive Committee shall each be three years, two of the six terms to expire each year. The First Vice-President elected in any year shall succeed to the presidency in the following year. Each regular term of office shall coincide with the calendar year or a multiple thereof.

Delete Paragraph 2 and substitute the following paragraphs, respectively, for Paragraphs 3, 4, 5.

2. Elective officers shall be chosen through elections to be held during the last three months of the terms of office of their predecessors. Each member shall be given the opportunity to vote by mail. The results of the election shall be certified and announced by the Secretary at the annual business meeting.

3. The Association shall have the following officers who shall be appointed by the Executive Committee: a Secretary, a Treasurer, a Managing Editor, and a Counsel. The terms of office of each of these officers shall be three calendar years.

4. The Executive Committee shall consist of the President, the three Vice-Presidents, the Secretary, the Treasurer, the Managing Editor, the two ex-Presidents who have last held office, and six elected members, provided that the Secretary, the Treasurer, and the Managing Editor shall not be entitled to vote in the Committee's meetings.

Section IV. *Duties of Officers.*

Amend by the insertion of a new paragraph 2 to read:

2. Before October 1 of each year, the First Vice-President of the Association shall appoint a Nominating Committee for the following year, this Committee to consist of a past officer as Chairman and not less than five other members of the Association. The names of the Committee shall be published in the issue of the *American Economic Review* immediately following their appointment, together with an invitation to the general membership that suggestions of nominees for the various offices other than the presidency be sent to the Chairman of the Committee. The Nominating Committee for each year shall be instructed to present to the Secretary of the Association on or before September 1 a nominee for the first vice-presidency and two or more nominations for each other elective office

to be filled, except the presidency, all these nominees being members of the Association. The members of the Nominating and Executive Committees shall constitute an Electoral College which shall consider the nominee of the Nominating Committee for the first vice-presidency and select a single candidate for that office; but space shall be provided on the ballot for that office for the individual voter's alternative choice.

The First Vice-President may, at his discretion and with the advice and consent of the Executive Committee, appoint a Program Committee for the annual meeting of the succeeding year.

After this new Paragraph 2 to Section IV would follow the present Paragraphs 2, 3, 4, 5, and 6, but renumbered 3, 4, 5, 6, and 7 except to amend the new Paragraph 6 to read:

The Managing Editor shall, with the advice and consent of the Executive Committee, appoint members to an Editorial Board to assist him. He shall be ex-officio member and chairman of this Board. The Editorial Board shall have charge of the publication of the *American Economic Review*.

It was voted that the proposed amendments to the Charter and Bylaws be approved and become effective upon ratification by the members and will be controlling in the election of Presidents, beginning with the President for the year 1961, and for all other elective officers whose terms of service begin in the year 1960.

It was voted to authorize the Secretary to prepare a mail ballot concerning the amendments, to be included with the ballots for the election of the officers in November, and to report the result at the December meeting of the Association.

FELLOWSHIPS AND GRANTS

The Social Science Research Council is again offering during the coming year predoctoral and postdoctoral research training fellowships, grants-in-aid of research and faculty research grants. Prospective candidates are urged to write to the Social Science Research Council, 230 Park Avenue, New York 17, N.Y. as early as possible to be assured of receiving a detailed announcement.

In addition to these offerings, for which individuals may apply, nominations are invited for: faculty research fellowships, providing part-time release from teaching for a term of three years and available to a few college and university faculty members; senior research awards in American government affairs, providing maintenance and research expenses for one year to about five distinguished scholars; and auxiliary research awards to be awarded to about 25 younger social scientists selected on the basis of past achievement and future promise of significant research.

NEW PUBLICATION

The Department of Economic and Social Affairs, United Nations, published in April 1958 the first issue of a new publication *Industrialization and Productivity*. It will concentrate on "the practical problems of the planning and execution of industrial projects," and is designed particularly to serve the interests of "officials in governmental and nongovernmental organizations, such as finance and economic ministries, development boards and technological institutes concerned with industrialization, as well as . . . technical assistance experts, plant managers and others actively involved in promoting industrialization." Columbia University Press is the distribution agent.

ANNOUNCEMENTS

The Universities-National Bureau of Economic Research is currently exploring the possibilities of a conference on Economic Aspects of Research and Technological Development. Any information as to research projects, underway or planned, that might contribute to

such a conference would be appreciated. Communications should be addressed to Richard R. Nelson, Economics Division, the RAND Corporation, 1700 Main Street, Santa Monica, California.

The Division of Forest Economics and Policy of the Society of American Foresters is compiling a directory of people in the United States and Canada who are working currently in the general area of the economics of forestry. Anyone whose present work or interest is related to the economics of production, harvesting, processing, marketing or consumption of forest products and services should write to Professor A. C. Worrell, 360 Prospect Street, New Haven 11, Connecticut, for a data sheet.

The University of Illinois has purchased the economics library of Jacob Hollander, late professor of the Johns Hopkins University. This collection ranks among one of the best private collections of works in economics.

Visiting Foreign Scholars

R. G. D. Allen of the London School of Economics, University of London, has been appointed visiting research professor of economics at the University of California, Berkeley, for the current academic year.

Assar K. E. Lindback of the University of Stockholm has been appointed visiting assistant professor of economics at the University of Michigan for the first semester of the current academic year.

James Potter of the London School of Economics has been appointed acting associate professor of economics at the University of North Carolina for the current academic year.

J. Denis Sargan of Leeds University, England, is serving as visiting professor of economics at the University of Michigan for the current year.

J. H. Brian Tew of the University of Nottingham has been appointed visiting professor of economics at Yale University for the first semester of the current academic year.

Peter Vandome of the Oxford Institute of Statistics has been appointed visiting lecturer at the University of Kentucky.

Aydin Yalcin of the University of Ankara has been appointed visiting associate professor of economics at Columbia University for 1958-59.

Retirements

A. Bruce Anthony has been named professor emeritus of economics at Mercer University.

Z. Clark Dickinson, professor of economics at the University of Michigan, has retired.

Cecil R. Glaves, associate professor of accounting and finance at the Illinois Institute of Technology, has retired.

H. Gordon Hayes has retired from Tulane University. He is now professor of economics, emeritus, at both Tulane University and Ohio State University.

Samuel M. Levin has been named professor of economics, emeritus, at Wayne State University.

Robert L. Masson has been named professor emeritus at Harvard Graduate School of Business Administration.

Broadus Mitchell has retired as professor of economics from Rutgers—The State University.

Carl F. Remer, professor of economics at the University of Michigan, has retired.

Hazel V. Roberts has retired from Hunter College.

Lawrence Smith, Stephen Greene professor of economics at Wellesley College, has retired as of June 1958.

Marie W. Spencer has retired from the Illinois Institute of Technology.

Albion G. Taylor has been appointed emeritus Chancellor professor of political economy at the College of William and Mary.

Francis D. Tyson, professor of industry and economics, has retired from the School of Business Administration, University of Pittsburgh.

Ross G. Walker has become professor emeritus at the Harvard Graduate School of Business Administration effective August 31, 1958.

Leo Wolman has retired as professor of economics at Columbia University.

Deaths

James Baster died August 23, 1957.

Hiram L. Jome of DePauw University died last spring.

Edwin A. Lamke died in September 1957.

Cleona Lewis, who served on the staff of the Brookings Institution until her retirement in 1950, died May 5, 1958.

Edmond E. Lincoln, economist for the Du Pont Company until his retirement in 1953, died May 15, 1958.

M. O. Phillips of Washington and Lee University died March 28, 1958.

Lloyd P. Rice, professor emeritus of economics at Dartmouth College, died May 10, 1958.

Appointments and Resignations

Warren E. Adams, formerly at Swarthmore College, has been appointed assistant professor of economics at The University of Texas.

J. P. Allen has been appointed associate professor of economics at DePauw University.

Laird B. Allison has been promoted to assistant professor of industrial management at Los Angeles State College.

Arthur T. Anderson of Harvard University has been appointed instructor in economics at the University of Maryland.

Edward M. Anson is executive director of the Iowa College Foundation.

Hector Anton, formerly of the University of California, Berkeley, is a visiting professor at the University of Minnesota.

David A. Baerncopf has been appointed assistant professor of business economics in the School of Business Administration, University of Oregon.

Eric N. Baklanoff of Ohio State University has been appointed assistant professor of foreign trade at Louisana State University.

Alexander S. Balinky has been promoted to associate professor of economics at Rutgers—The State University.

George B. Baldwin has resigned from Vanderbilt University to accept a position as general economist and assistant field project supervisor with Harvard's Iran Advisory Project in Tehran.

William L. Baldwin has been promoted to assistant professor of economics at Dartmouth College.

Louis B. Barnes has been appointed assistant professor of business administration in the Harvard Graduate School of Business Administration.

William P. Baxter has been appointed professor of financial accounting in the Graduate School of Business, Columbia University.

J. B. Bearnson, retired from the University of Utah in 1953, has been reappointed professor of economics at Birmingham-Southern College for the current academic year.

Garnett Beazley has been appointed assistant professor of accounting in the School of Business Administration of the University of Pittsburgh for 1958-59.

Carolyn S. Bell has been promoted to associate professor of economics at Wellesley College.

James R. Bentley, formerly of the University of Washington, has joined the faculty of the University of Minnesota as lecturer.

William Bentsen of the University of Wisconsin has been appointed assistant professor of economics at Beloit College.

Joseph S. Berliner has been promoted to associate professor in the department of economics, Syracuse University.

R. Glen Berryman, formerly of the University of Illinois, has joined the faculty of the University of Minnesota as assistant professor.

John W. Birch of The Johns Hopkins University has been appointed instructor in the department of economics, University of Illinois.

J. O. Blackburn has been appointed interim instructor in economics at the University of Florida.

Arthur I. Bloomfield has resigned from the Federal Reserve Bank of New York to become professor of economics and finance at the University of Pennsylvania.

Francis M. Boddy of the University of Minnesota participated in the Salzburg Seminar in American Studies during the winter 1958.

Joseph M. Bonin has accepted an appointment as assistant professor of economics at the University of Arkansas.

Gene Booker of the University of Indiana has been appointed assistant professor of economics at Washington and Jefferson College.

Neil H. Borden, Jr. has been promoted to research associate in business administration in the Harvard Graduate School of Business Administration.

Bodo Böttcher has resigned from the International Monetary Fund to accept an appointment as assistant to the executive president of the National Association of Electrical Industries in Frankfurt am Main, Germany.

James B. Boulden has been appointed assistant professor of transportation and production management in the School of Business Administration, University of California, Los Angeles.

Roger L. Bowlby has been appointed assistant professor in the department of economics and Labor and Industrial Relations Center, Michigan State University.

James E. Boyce has been appointed lecturer on business administration in the Harvard Graduate School of Business Administration.

Stanley E. Boyle of the University of Wisconsin has been appointed assistant professor of economics at Saint Louis University.

Floyd S. Brandt has been promoted to research associate in business administration in the Harvard Graduate School of Business Administration.

J. Herman Brasseaux is associate professor of accounting at Louisiana Polytechnic Institute.

Elmer C. Bratt has been appointed head of the department of economics and sociology, succeeding Herbert M. Diamond, at Lehigh University.

William G. Brokaw has accepted a position as instructor in economics at Lafayette College.

Martin Bronfenbrenner, formerly of Michigan State University, has been appointed professor of economics at the University of Minnesota effective December 1958.

Robert C. Brooks, Jr. of the University of Georgia will join the faculty of Vanderbilt University as assistant professor of business administration February 1, 1959.

Milton P. Brown, 2d has been promoted to professor of business administration in the Harvard Graduate School of Business Administration.

Yale Brozen has been appointed director of research in the School of Business, University of Chicago.

Heinrich H. Bruschke has been appointed instructor in economics at the University of Minnesota.

Anthony J. Bryski of Lehigh University has become head of the Bureau of Research and Statistics, Department of Labor and Industry, State of Pennsylvania.

Dale E. Butz is visiting research professor in the Harvard Graduate School of Business Administration.

Carl C. Cabe, formerly of the University of Illinois, has been appointed associate professor of economics at the University of Kentucky.

James Cairns has been appointed instructor in the department of political economy at the Johns Hopkins University.

James A. Caldwell has accepted an appointment as associate professor at Georgia Institute of Technology.

Rondo E. Cameron of the University of Wisconsin is at the Center for Advanced Study in the Behavioral Sciences, Stanford, California, in the current academic year.

Thomas C. Campbell has been promoted to professor of economics in the College of Commerce, West Virginia University.

David S. Carlson has been appointed instructor of urban land studies, School of Business Administration, University of Pittsburgh.

Paul M. Carrick, Jr., formerly of San Diego State College, has been appointed acting assistant economist in the Bureau of Economic and Business Research, State College of Washington.

John J. Carroll has been promoted to professor and chairman of the department of economics and business administration at St. Lawrence University.

Edward H. Chamberlin has resigned as editor of the *Quarterly Journal of Economics* as of June 1958. Arthur Smithies will be the new editor.

Paul W. Cherington has been promoted to professor of business administration in the Harvard Graduate School of Business Administration.

Frank C. Child has been promoted to associate professor of economics at Michigan State University.

Carl R. Christensen has been promoted to professor of business administration in the Harvard Graduate School of Business Administration.

James V. Clark has been promoted to assistant professor of business administration in the Harvard Graduate School of Business Administration.

Richard V. Clemence has been promoted to professor of economics at Wellesley College.

Meredith O. Clement has been promoted to assistant professor of economics at Dartmouth College.

Ronald H. Coase has been appointed professor of economics at the University of Virginia. He will be on leave in 1958-59 at the Center for Advanced Study in the Behavioral Sciences, Stanford, California.

E. Kennedy Cobb has been appointed assistant professor of accounting at Los Angeles State College.

Edward Coen, on leave from the University of Minnesota this year, is visiting associate professor of economics at the University of California, Berkeley.

David C. Cole of the University of Michigan has accepted an appointment as assistant professor of economics and associate director of the Graduate Training Program in Economic Development at Vanderbilt University.

Alan B. Coleman has been appointed research associate in business administration in the Harvard Graduate School of Business Administration.

Lorne D. Cook has been promoted to associate professor of economics at Pomona College.

Albert R. Cox of Baylor University has accepted an appointment as assistant professor of accounting at Los Angeles State College.

Rupert Craig has been appointed assistant professor of finance at Los Angeles State College.

Joseph Cropsey has resigned from the City College.

Donald E. Cullen has been promoted to associate professor of industrial and labor relations at Cornell University.

C. C. Curtis, formerly at Rensselaer Polytechnic Institute, has been appointed assistant professor of real estate at the University of Florida.

Darwin W. Daicoff has been appointed instructor in economics at the University of Michigan.

Donald C. Darnton has been appointed instructor in economics at the University of Michigan.

Ansis L. Darzins has been appointed instructor in economics at the University of Minnesota.

Paul Davidson has been appointed assistant professor of economics at Rutgers—The State University.

Sidney Davidson has been appointed professor of accounting in the School of Business, University of Chicago.

Keith Davis has resigned from Indiana University to accept an appointment at Arizona State College.

John S. deBeers has been appointed lecturer in economics at the University of Maryland.

Walter H. Delaplane has resigned from Texas A & M to accept the deanship of the College of Arts and Sciences of Southern Methodist University.

Harold Demsetz of Northwestern University has been appointed lecturer in economics at the University of Michigan.

Nancy Depew has been appointed instructor at Carnegie Institute of Technology.

C. T. Devine, on a year's leave from the University of Florida, is visiting professor at the University of California, Berkeley.

Salvatore F. Divita has been named research associate in the Harvard Graduate School of Business Administration.

C. H. Donovan of the University of Florida has accepted a Fulbright lectureship at Karnatak University, India.

Robert Eisner of Northwestern University is at the Cowles Foundation for Research in Economics, Yale University, in the fall quarter.

James R. Elliott, Jr. has resigned from Denison University to enter private business.

Paul T. Ellsworth, now in Paris completing work as chief of a World Bank Economic Survey Mission in Thailand, will return to the University of Wisconsin in January 1959.

Emerson C. Erb, Jr. has resigned from the School of Business, Indiana University.

Herman Erickson has been promoted to professor of labor and industrial relations in the Institute of Labor and Industrial Relations, University of Illinois.

Franklin B. Evans has been appointed instructor in marketing in the School of Business, University of Chicago.

Tibor Fabian has resigned from the School of Business Administration, University of California, Los Angeles to accept a position in the firm of Lybrand, Ross Bros. and Montgomery.

Oladunjoye Fashola has been appointed instructor in economics at Baldwin-Wallace College.

John Fayerweather has been appointed adjunct associate professor of international business in the Graduate School of Business, Columbia University.

William W. Fearnside has been appointed assistant professor of business administration in the Harvard Graduate School of Business.

Rashi Fein, who has been promoted to associate professor of economics at the University of North Carolina, has been given a year's leave of absence to take a position as statistician with the Department of Commerce, Washington, D.C.

Max Fessler has been promoted to a full professorship in the School of Business, University of Kansas.

Thomas Finn has been promoted to assistant professor of economics at Dartmouth College.

Gerald P. Foster has accepted an appointment as assistant professor of management at Los Angeles State College.

W. M. Fox of the University of Florida is a Fulbright lecturer at the Helsinki School of Economics, Helsinki, Finland.

George N. Francis has been promoted to professor of accounting at Los Angeles State College.

Andrew G. Frank has been appointed assistant professor of economics at Michigan State University.

Helmut J. Frank has been appointed instructor in economics at Princeton University.

Earl S. Fullbrook has resigned from the deanship of the College of Business Administration, University of Nebraska, but is continuing teaching as professor of marketing.

Eirik Furubotn has resigned from Rensselaer Polytechnic Institute to accept a position as instructor in economics at Lafayette College.

Daniel R. Fusfeld has been promoted to associate professor of economics at Michigan State University.

George Garvy, senior economist at the Federal Reserve Bank of New York, has been appointed adviser. He is now a part-time member of the staff of the Economic Development Institute, Washington, D.C.

R. C. Geary, of the UN Statistical Office has been appointed visiting professor in the department of economics of the Graduate Faculty, New School for Social Research, for the fall term.

Franz Gehrels has been promoted to associate professor in the department of economics, Indiana University.

Dwight L. Gentry has been promoted to professor of marketing administration in the College of Business and Public Administration, University of Maryland.

Nicholas Georgescu-Roegen of Vanderbilt University is doing research and lecturing at the Institute of European Studies in Turin, Italy, under a Guggenheim fellowship and Fulbright grant.

James A. Gherity, Jr. has been promoted to assistant professor of economics at Michigan State University.

Charles Gilbert has resigned from New York University to accept a position as chairman of the economics department and director of the graduate division of business administration of the University of Hartford.

Nicholas A. Glaskowsky has been promoted to assistant professor at the University of Minnesota.

William S. Gomberg has been appointed visiting professor of industrial relations, Graduate School of Business, Columbia University.

Bernard Goodman has been promoted to associate professor of economics at Wayne State University. He is on leave this year for research and study at Yale University.

Wendell C. Gordon has been promoted to professor of economics at the University of Texas.

Robert Graves has been appointed assistant professor of applied mathematics in the

School of Business and associate director of the Operations Analysis Laboratory, University of Chicago.

Seymour Gray has been promoted to assistant professor of accounting in the department of business and economics, Illinois Institute of Technology, Chicago, Illinois.

Charles J. Grayson, Jr. has been named lecturer on business administration in the Harvard Graduate School of Business.

Leo Grebler has been appointed professor of real estate and urban land economics in the School of Business Administration, University of California, Los Angeles.

David Green has been promoted to associate professor of accounting and appointed director of the downtown program, School of Business, University of Chicago.

William C. Greene has been promoted to research associate in business administration in the Harvard Graduate School of Business Administration.

Peter Gregory of Yale University has been appointed assistant professor of economics at the University of Minnesota.

Frank C. Hachman has been appointed acting instructor in economics at the State College of Washington.

Josef Hadar has been appointed instructor in economics at the University of Minnesota.

Herbert R. Hahn has been appointed visiting assistant professor in the department of economics and business administration, Duke University.

A. Stuart Hall has been appointed chairman of the department of economics in the College of Business Administration, University of Nebraska.

Marcia L. Halvorsen has been appointed instructor in economics at the University of Minnesota.

C. Lowell Harris has been promoted to professor of economics at Columbia University. He is serving as U. S. correspondent for the Institut d'Etudes du Developpement Africain in which connection he recently participated in a conference in Paris on financing Algerian economic development.

Delbert C. Hastings, formerly with the Federal Reserve Bank of St. Louis, has joined the faculty of the University of Minnesota as an associate professor.

Paul G. Hastings has been named to the Fort Worth National Bank Chair of Finance in the School of Business of Texas Christian University.

Victor C. Heck has been named John D. Stetson professor of economics at Mercer University.

W. A. Heffelfinger has accepted an appointment with the Bureau of Business and Economic Research in the College of Business Administration, University of Arkansas.

Eduard Heimann, professor emeritus of the New School for Social Research, has been appointed visiting professor of economics at the University of California, Berkeley, for the fall semester.

Richard M. Heins has resigned from the University of California, Los Angeles, to accept a position as professor of insurance, University of Wisconsin.

George Heitman has been appointed instructor in economics at The Pennsylvania State University.

Walter W. Heller is serving as chairman of the department of economics, University of Minnesota, 1957-59.

William F. Hellmuth, Jr. of Oberlin College will be visiting professor at the University of Wisconsin the second semester 1958-59 and the summer of 1959.

Irwin L. Herrnstadt, formerly of the Massachusetts Institute of Technology, has been appointed assistant professor of economics in the College of Commerce, West Virginia University.

Benjamin Higgins has been granted a leave of absence for the fall semester from the Center for International Studies, Massachusetts Institute of Technology, to serve as visiting professor of economics at the University of Texas.

Donald R. Hodgman of the University of California, Berkeley, has been appointed associate professor of economics at the University of Illinois.

Morton Hoffman has resigned from the Baltimore Urban Renewal and Housing Agency to become an urban and economic consultant in the same city.

James R. Holcomb has been appointed instructor in industry, School of Business Administration, University of Pittsburgh.

Hendrik S. Houthakker of Stanford University has been appointed visiting professor of economics at Harvard University for the year 1958-59.

John A. Howard has resigned from the School of Business, University of Chicago, to become professor of marketing at the University of Pittsburgh.

Charles W. Howe has been appointed assistant professor of economics at Purdue University.

John M. Hunter has been promoted to professor of economics at Michigan State University. He is on leave this year to be director of the newly organized Institute of Economic Research, University of the Andes, Bogotá, Colombia.

Patrick Huntley has resigned from Duke University to be lecturer in economics and assistant to the dean at the University of Arizona.

John R. T. Hughes has been promoted to associate professor of economics at Purdue University.

William Iulo, formerly of the University of Wisconsin, has been appointed assistant economist in the Bureau of Economic and Business Research, State College of Washington.

James E. Jacobson has been appointed instructor at the University of Minnesota.

John E. Jeuck of the Harvard Graduate School of Business Administration has been appointed professor of business administration and Robert Law professor of business administration at the University of Chicago School of Business.

Dudley W. Johnson has been promoted to assistant professor of economics at Lehigh University.

C. Clyde Jones has been promoted to associate professor in the department of economics, University of Illinois.

Howard L. Jones has been appointed professor of statistics in the School of Business, University of Chicago.

Dale W. Jorgenson of Harvard University will be acting assistant professor of economics at the University of California, Berkeley, in the spring semester 1959.

M. J. Kafoglis of Ohio State University has been appointed assistant professor of economics at the University of Florida.

Alfred E. Kahn has been appointed chairman of the department of economics, Cornell University.

C. Harry Kahn has been appointed associate professor of economics in the College of Arts and Sciences, Rutgers—The State University.

D. L. Kemmerer of the University of Illinois has been teaching at the University of Melbourne, Australia, since June.

Peter Kenen has been promoted to assistant professor at Columbia University.

Maurice D. Kilbridge has been appointed professor of production in the School of Business, University of Chicago.

James Kindahl has been appointed assistant professor in political economy at the Johns Hopkins University.

Philip A. Klein has been promoted to assistant professor of economics in The Pennsylvania State University.

Anthony Y. C. Koo has been promoted to professor of economics at Michigan State University.

Adamantia P. Koslin is visiting professor of economics at the University of Oklahoma.

Lawrence B. Krause has been promoted to assistant professor of economics at Yale University.

Leon E. Krouse has been promoted to assistant professor of finance at Lehigh University.

David S. Landes has been appointed professor of history and economics at the University of California, Berkeley.

Henry W. Laurant is industrial economist at Stanford Research Institute, Menlo Park, California.

Warren A. Law has been appointed lecturer on business administration in the Harvard Graduate School of Business.

Leonard L. Lederman is now staff economist with the Chamber of Commerce of the United States.

John D. Lehman has been appointed assistant professor of economics at Denison University.

Abba P. Lerner of Roosevelt University has been appointed visiting professor of economics for 1958-59 at the University of California, Berkeley.

Selig D. Lesnoy has been appointed instructor in economics at the University of Michigan.

Stuart Levow of the University of Michigan has been appointed instructor in economics at Washington and Jefferson College.

H. H. Liebhafsky has been promoted to associate professor of economics at the University of Texas.

Ta-Chung Liu has been appointed professor of economics at Cornell University.

Kullervo Louhi has resigned from the University of Chicago to become professor of business administration and associate dean of the College of Business and Public Service, Michigan State University.

Michael C. Lovell has been appointed instructor in economics at Yale University.

John W. Lowe has resigned from the University of Florida to accept a professorship at Arizona State College.

Stanley T. Lowry is assistant professor of economics at East Carolina College.

Myles L. Mace has been appointed professor of business administration in the Harvard Graduate School of Business Administration.

Duncan MacIntyre has been promoted to professor of industrial and labor relations in the New York State School of Industrial and Labor Relations, Cornell University.

H. David Maloney has been promoted to associate professor of economics at DePauw University.

Karl O. Mann has accepted an appointment as associate professor of personnel and industrial relations in the College of Business Administration, University of Toledo.

Charles F. Marsh has resigned from the College of William and Mary to become president of Wofford College.

David D. Martin has been appointed visiting associate professor of business economics and public policy in the School of Business, Indiana University.

Norman H. Martin has been promoted to professor of industrial relations in the School of Business, University of Chicago.

Mark S. Massel, formerly with Bell, Boyd, Marshall, and Lloyd of Chicago, has joined the senior staff of the Brookings Institution.

John B. Matthews, Jr. has been promoted to associate professor of business administration in the Harvard Graduate School of Business Administration.

Raymond R. Mayer has been appointed associate professor of management in the department of business and economics, Illinois Institute of Technology.

John F. McLaren of the University of Ohio has accepted an appointment as assistant professor of accounting at Los Angeles State College.

Walter S. Measday is on leave from the University of Maryland to serve as a member of the professional staff of the Subcommittee on Antitrust and Monopoly of the Senate Judiciary Committee.

John A. Menge has been promoted to assistant professor of economics at Dartmouth College.

Frederic Meyers of the University of Texas has been appointed professor of Industrial relations in the School of Business Administration, University of California, Los Angeles.

Charles S. Miller has been appointed dean of the College of Business Administration, University of Nebraska.

Jerry Miner of the University of Michigan has been appointed assistant professor of economics at Syracuse University.

Hyman P. Minsky has resigned from Brown University to accept an appointment as assistant professor of economics at the University of California, Berkeley.

C. Clyde Mitchell has been a member of the Harvard Advisory Group to the Pakistan Planning Board since October 1957. Since February 1958 he has been acting field supervisor of the Group in addition to adviser in finance and international trade.

Broadus Mitchell has been appointed John Hay Whitney professor of economics at Hofstra College for 1958-59.

Wiley S. Mitchell has been promoted to a full professorship in the School of Business, University of Kansas.

John M. Montias has been promoted to assistant professor of economics at Yale University.

William M. Morgenroth has been appointed instructor in the School of Business Administration, University of Pittsburgh.

Anthony R. Morici has been appointed instructor in accounting in the department of business and economics at Illinois Institute of Technology.

Morris D. Morris has been promoted to associate professor at the University of Washington.

Irving Morrissett has been promoted to associate professor of economics at Purdue University.

Jacob L. Mosak of the United Nations has been appointed visiting professor of economics at Columbia University for 1958-59.

Robert A. Mundell of the University of British Columbia has been appointed acting assistant professor of economics at Stanford University.

Roger F. Murray has been appointed professor of finance in the Graduate School of Business, Columbia University.

Richard A. Musgrave of the University of Michigan has served as adviser to the Government of Burma in the past summer.

Simon Naidel is serving as acting chairman of the department of economics at American University.

Walter C. Neele has been appointed assistant professor of economics at the University of Texas.

Boyd L. Nelson has been promoted to associate professor of business administration in the College of Business and Public Administration, University of Maryland.

Edward G. Nelson has been appointed director of the Center for Research in Business at the University of Kansas.

Paul E. Nelson, Jr. has been promoted to professor of economics at Denison University.

Marc Nerlove has been appointed associate professor of economics and agricultural economics at the University of Minnesota.

John Neter has been promoted to a full professorship at the University of Minnesota.

W. E. Newbolt, formerly of Berea College, has been appointed instructor in accounting at the University of Florida.

Monroe Newman has been promoted to associate professor of economics and head of the department of economics, The Pennsylvania State University.

Sherwood W. Newton has been promoted to associate professor in the School of Business, University of Kansas.

James B. Nilsen has been appointed instructor at the University of Minnesota.

John C. Norby has been promoted to professor of economics at Los Angeles State College.

James H. Noren has been appointed instructor in economics at Princeton University.

David Novack has been appointed instructor in economics at Columbia University.

G. Warren Nutter has been promoted to professor of economics at the University of Virginia.

E. A. Nyquist has been promoted to associate professor in the department of commerce and economics, University of Vermont.

Bernard Okun, formerly with the RAND Corporation, has accepted an appointment as assistant professor of economics at Princeton University.

Allan L. Olson has been appointed instructor in economics at the University of Minnesota.

Donald R. Olson has been appointed instructor, University of Minnesota.

Alex Orden has been appointed professor of applied mathematics in the School of Business and director of the Operations Analysis Laboratory, University of Chicago.

Clifford F. Owen of the University of Virginia has been appointed associate professor of economics at the College of William and Mary.

Garland C. Owens has been appointed associate professor of accounting, Graduate School of Business, Columbia University.

Donald R. Paden has been promoted to professor in the department of economics, University of Illinois.

Hugh T. Patrick has been appointed lecturer in economics at the University of Michigan.

Benedict J. Pedrotti has been appointed instructor in economics at the University of Michigan.

Richard F. Peirce has resigned from the School of Business Administration, University of California, Los Angles, to accept a business position.

Mark Perlman has been promoted to associate professor of political economy at the Johns Hopkins University.

Richard L. Pfister has been appointed instructor in economics at Dartmouth College.

Ralph W. Pfouts has been promoted to professor of economics at the University of North Carolina.

Clinton A. Phillips has resigned from the department of economics, The University of Tennessee.

Janus Poppe, formerly of Georgetown University, has been appointed lecturer in economics in the Far Eastern program of the University of Maryland.

Richard C. Porter has been promoted to assistant professor of economics at Yale University. He is on leave this year serving as visiting lecturer in the department of economics at the University of Bombay.

James T. S. Porterfield has been promoted to associate professor of Business Administration in the Harvard Graduate School of Business Administration.

James P. Quirk, formerly of the University of Minnesota, has been appointed instructor in economics at Purdue University.

Elton Rayack has resigned from the department of economics at The Pennsylvania State University.

Jim E. Reese is on a year's leave of absence from the University of Oklahoma to serve as economist for the Joint Council of Economic Education.

William C. Reher has been appointed assistant professor in economics at the State University of Iowa.

Archibald N. Reid has resigned from the department of economics and political science, University of Saskatchewan.

Richard Ridilla has been appointed assistant professor of accounting in the School of Business Administration, University of Pittsburgh.

Roderick H. Riley has been executive director of the Joint Economic Committee of the Congress of the United States since April 1958.

Warren Robinson has been appointed assistant professor of economics at The George Washington University.

David C. D. Rogers has been named assistant professor of business administration in the Harvard Graduate School of Business Administration.

Henry Rosovsky of the University of Chicago has been appointed acting assistant professor in the department of economics, University of California, Berkeley.

Van E. Rothrock has been appointed assistant professor in the School of Business, University of Kansas.

George G. Sause has been promoted to associate professor of economics at Lafayette College.

Eric Schenker has been appointed assistant professor, department of economics and Highway Traffic Safety Center, Michigan State University.

Richard Scheuch has been promoted to associate professor of economics at Trinity College.

Guy A. Schick from Harvard University has been appointed instructor in the department of economics, University of Illinois.

James R. Schlesinger has been promoted to associate professor of economics at the University of Virginia.

Edward B. Schmidt has resigned from the chairmanship of the department of economics to devote full time to research and teaching at the University of Nebraska.

Hans O. Schmitt, who has been in Indonesia under the University of California-University of Indonesia Economics Project, has accepted an appointment as instructor in economics at the University of Wisconsin.

Wilbert M. Schneider is treasurer of the Loma Linda Food Company in California.

M. C. Schnitzer of the University of Arkansas has been appointed instructor in economics, University of Florida.

Eli Schwarz has been promoted to associate professor of finance at Lehigh University.

Ira O. Scott has been appointed associate professor of finance in the Graduate School of Business, Columbia University.

Norton Seeber of the University of California is assistant professor of economics at Carnegie Institute of Technology.

Martin Segal has been appointed assistant professor of economics at Dartmouth College.

Joel Seidman has been appointed professor of social sciences in the School of Business, University of Chicago.

Richard T. Selden of Vanderbilt University has been appointed research associate at the National Bureau of Economic Research for 1958-59.

Lawrence H. Seltzer of Wayne University will be visiting professor of economics at the University of Michigan the second semester of the current academic year.

Ronald A. Shearer of Ohio State University has been appointed lecturer in economics at the University of Michigan.

David H. Shelton has been appointed assistant professor of economics and business administration at the University of Delaware.

Gerald W. Siegel has been appointed lecturer on business administration in the Harvard Graduate School of Business Administration.

Fred S. Silander has been appointed assistant professor of economics at DePauw University.

William B. Simpson, former editor of *Econometrica*, has been appointed assistant professor of economics and statistics at Los Angeles State College.

Richard H. Slavin has been appointed instructor in finance in the School of Business Administration, University of Pittsburgh.

Walter L. Slifer, of Carson-Newman College, has been appointed professor of economics at Mercer University.

Theodore H. Smith has resigned as dean of the School of Business Administration, Montana State University, to become dean of the Graduate School of Business of the Air Force Institute of Technology, Air University, Wright-Patterson Air Force Base, Ohio.

Vernon L. Smith has been promoted to associate professor of economics at Purdue University.

Warren L. Smith, on leave from the University of Michigan, is visiting lecturer at Harvard University.

Benson Soffer has been appointed assistant professor of industry, School of Business Administration, University of Pittsburgh.

Egon Sohmen has been appointed assistant professor of economics at Yale University.

Gerald F. Sorrensen has accepted an appointment as assistant professor of economics at Los Angeles State College.

Jared Sparks has resigned from Purdue University to accept an appointment as assistant professor of economics at the University of Arkansas.

Eldred C. Speck has been appointed assistant professor of accounting in the School of Business Administration, University of Miami.

George A. Spiva, Jr. has been appointed assistant professor of economics in the College of Business Administration, University of Tennessee.

Darrell Spriggs has been promoted to professor of economics in the College of Business Administration, University of Arkansas.

W. J. Stankiewicz of the University of British Columbia served this summer as senior economist with the Department of Transport, Government of Ontario.

Stanley W. Steinkamp of the University of Michigan has been appointed assistant professor of economics at the University of Illinois.

Harold W. Stevenson, formerly of Northwestern University, has joined the faculty of the University of Minnesota as an associate professor.

R. Stansbury Stockton has been appointed associate professor of management in the School of Business, Indiana University.

Wolfgang F. Stolper is on leave from the University of Michigan this year to direct a research project on the economy of Africa at the Center for International Studies, Massachusetts Institute of Technology.

Kenneth T. Strand, formerly of the University of Wisconsin, has been appointed acting instructor in economics at the State College of Washington.

Paul J. Strayer has been promoted to professor of economics at Princeton University.

Robert L. Strider has been appointed lecturer in accounting in the department of political economy, Johns Hopkins University.

Ronald L. Stucky has been appointed assistant dean of the newly created School of Industrial Management at Purdue University.

Daniel B. Suits has been given leave of absence from the University of Michigan to serve as director of the Seminar in American Studies at Kyoto in the summer and fall 1958.

Barry E. Supply has been named assistant professor of business administration in the Harvard Graduate School of Business Administration.

Joseph F. Talarico has been promoted to assistant professor of economics in the College of Arts and Sciences, Rutgers—The State University.

James Tang, formerly with the Bureau of the Census, is now an analytical statistician with the Bureau of Mines, Department of Interior, Washington, D.C.

Jack Taylor of the University of Rochester has accepted an appointment as associate professor of economics at the University of Buffalo. He is also lecturer in economics at the University of Rochester.

Overton H. Taylor of Harvard University is visiting scholar at the Thomas Jefferson Center for Studies in Political Economy at the University of Virginia in the first semester of this year.

Robert G. Taylor has been appointed instructor in accounting in the School of Business, University of Chicago.

Lester G. Telser has been appointed assistant professor of marketing in the School of Business, University of Chicago.

David W. Thompson has resigned from the School of Business, Indiana University.

James H. Thompson has been promoted to professor of economics in the College of Commerce, West Virginia University.

Philip H. Thurston has been named lecturer on business administration in the Harvard Graduate School of Business Administration.

Wendell P. Trumbull has been named head of the department of accounting at Lehigh University.

Lloyd Ulman of the University of Minnesota has been appointed professor of economics and industrial relations in the department of economics, University of California, Berkeley.

Melville J. Ulmer of The American University is a Fulbright professor for the year 1958-59 at the Netherlands School of Economics in Rotterdam.

Roger B. Ulvestad has been appointed acting assistant professor of marketing and transportation in the School of Business Administration, University of California, Los Angeles.

John H. Urban has been promoted to associate professor of economics at Lehigh University.

Richard F. Vancil is an instructor in business administration in the Harvard Graduate School of Business Administration.

Robert F. Vandell has been promoted to assistant professor of business administration in the Harvard Graduate School of Business Administration.

William J. Vatter has resigned from the School of Business, University of Chicago to become professor of accounting at the University of California, Berkeley.

William S. Vickrey has been promoted to professor of economics at Columbia University.

Dwight D. Vines has accepted an appointment as assistant professor of Northeast Louisiana State College.

Lloyd O. Wadleigh has resigned from Baldwin-Wallace College to become chairman of the department of economics at Carroll College, Waukesha, Wisconsin.

Martin Wagner, executive director of the Louisville, Ky. Labor-Management Committee, has been appointed director of the Institute of Labor and Industrial Relations at the University of Illinois.

Haskell P. Wald has been given a year's leave of absence from the Federal Reserve Bank of New York to accept a United Nations Technical Assistance appointment as economic adviser to the Bank of Greece in Athens.

Claude Walker has been appointed professor of management in the School of Business Administration, University of Miami.

Edward L. Wallace has resigned from the School of Business, University of Chicago, to become professor of accounting and business administration at the University of Buffalo.

Sherwood G. Walters has been promoted to associate professor of marketing at Lehigh University.

Richard J. Ward of Fordham University has taken a position with Caltex Oil Company, New York.

Harold W. Watts has been promoted to assistant professor of economics at Yale University.

Richard S. Weckstein has been appointed assistant visiting professor at the University of Rochester.

Emanuel T. Weiler has been appointed dean of the newly created School of Industrial Management at Purdue University.

Paul Weiner has been promoted to assistant professor of economics at Denison University.

W. Keith Weltmer has been promoted to a full professorship in the School of Business, University of Kansas.

Thomas Whisler has been promoted to associate professor of industrial relations in the School of Business, University of Chicago.

C. Arthur Williams, Jr. has been promoted to the rank of professor at the University of Minnesota.

Ernest W. Williams, Jr. has been promoted to professor of transportation in the Graduate School of Business, Columbia University.

Frederick Williams has resigned from the University of Illinois to accept an appointment as associate professor at the University of Missouri.

J. Earl Williams has been appointed assistant professor of economics in the College of Business Administration, The University of Tennessee.

James R. Williams has been appointed instructor in economics at the University of Minnesota.

David M. Winch has been appointed special lecturer in economics at the University of Saskatchewan for the current academic year.

Ronald H. Wolf has been appointed assistant professor of economics in the College of Business Administration, The University of Tennessee.

Paul Wolotkin has been appointed assistant professor in the department of commerce and economics, University of Vermont.

Paul Wonnacott of Princeton University has been appointed instructor in economics at Columbia University.

C. Dow Worley of Baylor University has been named assistant professor of management at Los Angeles State College.

Shih-Yen Wu has been appointed visiting lecturer in economics at the University of Minnesota.

John H. Young has been promoted to associate professor of economics at Yale University.

FIFTY-FIFTH LIST OF DOCTORAL DISSERTATIONS IN POLITICAL ECONOMY IN AMERICAN UNIVERSITIES AND COLLEGES

The present list specifies doctoral degrees conferred during the academic year terminating June 1958, and theses undertaken in the same period.

General Economics; Methodology

Degrees Conferred

JAMES F. BECKER, Ph.D. Columbia 1957. On the explanation concept in economics.

JOHN A. DAVIS, Ph.D. Alabama 1957. Ethics, economics and individualism.

JAMES B. QUINN, Ph.D. Columbia 1958. The measurement and evaluation of research results.

LESLIE P. SINGER, Ph.D. Indiana 1958. Aspects of the theory of normative economics.

Theses in Preparation

REV. F. J. BUCKLEY, B.A. Holy Cross 1941; M.S.S.W. Boston College 1950. A study of the economic concepts and attitudes of seminarians of the Boston Archdiocesan Seminary and of some economic factors in their backgrounds. *Boston College.*

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Degrees Conferred

OWEN F. ALDIS, Ph.D. Harvard 1958. The basis of welfare economics.

VINCENT F. BOLAND, Ph.D. California (Los Angeles) 1958. Pigou's optimum conditions for production and exchange.

MEYER L. BURSTEIN, Ph.D. Chicago 1957. The demand for household refrigeration in the United States.

ARNOLD P. COLLERY, Ph.D. Princeton 1958. Some theoretical implications of the wealth-saving relationship.

JOHN L. CORNWALL, Ph.D. Harvard 1958. Economic implications of some dynamic models.

JOHN P. DOLL, Ph.D. Iowa (Ames) 1958. Evaluation of alternative algebraic forms for production functions.

JACQUES DREZE, Ph.D. Columbia 1958. Individual decision making under partially controllable uncertainty.

TIBOR FABIAN, Ph.D. California (Los Angeles) 1957. Process analysis of the U.S. iron and steel industry; a linear programming model.

ABRAM C. FLORA, JR., Ph.D. North Carolina 1958. Economic thought in South Carolina—1820-1860.

JAMES A. GHERITY, Ph.D. Illinois 1958. Alfred Marshall and economic development.

ARTHUR S. GOLDBERGER, Ph.D. Michigan 1958. Properties of an econometric model of the United States.

CRAUFURD D. W. GOODWIN, Ph.D. Duke 1958. Canadian economic thought, 1814-1914.

YEHUDA GRUNFELD, Ph.D. Chicago 1958. Determinants of corporate investment.

ABRAHAM HIRSCH, Ph.D. Columbia 1957. Reconstruction in economics: the work of Wesley Clair Mitchell.

AUSTIN C. HOGGATT, Ph.D. Minnesota 1957. Simulation of the firm.

ROBERT C. JONES, Ph.D. Pennsylvania 1958. Income expectations and consumer spending.

FRANCIS T. JUSTER, Ph.D. Columbia 1957. Analysis of the saving function with special reference to the role of consumer durable goods.

ANNA L. KRUEGER, Ph.D. Wisconsin 1958. An evaluation of some growth theories in light of United States economic experience 1946-47.

DUDLEY G. LUCKETT, Ph.D. Texas 1958. A theoretic and historical study of the term structure of interest rates.

JOHN MONTIAS, Ph.D. Columbia 1958. Prices of producers' goods in post-war Poland.

RICHARD F. MUTH, Ph.D., Chicago 1958. The demand for non-farm housing.

DON V. PLANTZ, Ph.D. Indiana 1957. A reappraisal of Cournot's *Recherches* and its influence upon the development of economic thought.

KYOHEI SASAKI, Ph.D. Columbia 1958. A western influence on Japanese economic thought: the Marxian non-Marxian controversies of the 1920's and their significance for today.

HARRY G. SHAFFER, Ph.D. New York 1958. The economic functions of government in early English classical economic thought.

STANLEY W. STEINKAMP, Ph.D. Michigan 1958. Some factors which influence investment decisions.

HERMANN STOLLER, Ph.D. Virginia 1958. The very short run in economic theory.

ROBERT M. WILL, Ph.D. Duke 1958. Some aspects of the development of economic thought in Chile (ca. 1778-1878).

FREDERICK WILLIAMS, Ph.D. Northwestern 1958. A statistical analysis of the demand for industrial molasses.

Theses in Preparation

K. JANAKI KUTTY AMMA, B.A., Univ. of Travancore 1944; M.A. 1946; M.A. Fletcher School 1957. United Nations technical assistance: a study in decision-making for allocation of resources. *Fletcher School.*

RALPH ANSPACH, B.A. Chicago 1948; Dipl. Univ. of Paris 1951. The economics of growth of Sismondi. *California (Berkeley).*

LEO I. BAKONY, B.A. British Columbia 1944. A quarterly econometric model of the Canadian economy. *Washington.*

HAROLD F. BREIMYER, B.S. Ohio State 1934; M.S. 1935. Analysis of factors affecting the determination of prices of meat and meat animals, in both the short and long run. *American.*

IRENE H. BUTTER, B.A. Queens 1953; M.A. Duke 1955. Some aspects of Dutch economic thought during the nineteenth century. *Duke.*

NAI-RUENN CHEN, B.A. National Taiwan 1950; M.S. Illinois 1955. Factors influencing investment expenditures. *Illinois.*

EDWIN K. CLICKNER, B.S. American 1948; M.A. 1955. The proletarian development in classical economic theory (1775-1874). *American.*

KALMAN COHEN, B.A. Reed 1951; B.Litt. Oxford 1953; M.S. Carnegie Inst. Technology 1956. A computer model of the shoe, leather, and hide industries. *Carnegie Inst. Technology.*

PIERRE R. CROSSON, B.A. Texas 1948. Gross savings, growth models and stagnation. *Columbia.*

PETER DANNER, B.A. St. Louis 1945; M.A. 1949. Inquiry into the origins of Adam Smith's theory of value. *Syracuse.*

LOUIS A. FOURT, B.A. Missouri 1939. Empirical income elasticities for food and its component values produced by farmers, manufacturers and other marketing agencies in the United States, 1929-1956. *Chicago.*

FRED M. GOTTHEIL, B.A. McGill 1954; M.A. Duke 1957. Economic predictions of Karl Marx. *Duke.*

MARK A. HASKELL, B.S. Rutgers 1949; M.S. Cornell 1950. David Ricardo: An investigation of J. H. Hollander's uncompleted manuscript. *Rutgers.*

EDGAR P. HICKMAN, B.A. North Carolina 1954. A regional income growth model. *North Carolina.*

ALBERT A. HIRSCH, B.A. Oberlin 1955; M.A. Duke 1958. A classification of real Keynesian models on the basis of their fundamental assumptions. *Duke.*

T. EDWARD HOLLANDER, B.S. New York 1952; M.B.A. 1953. Economic significance of measures of capital formation and capital consumption in the business factor of the economy. *Pittsburgh.*

CHARLES W. HOWE, B.A. Rice 1952. Corporate savings behavior. *Stanford.*

HARRY L. JOHNSON, B.A. Emory and Henry 1952; M.A. Virginia 1957. Price behavior and product differentiation. *Virginia.*

TOSHINOSUKE KASHIWAZAKI, B.A. Waseda (Tokyo) 1951; M.E., 1953. A critical study of recent contributions to the theory of welfare economics. *North Carolina.*

SISTER MARTHA JULIE KEEHAN, S.N.D., B.A. Trinity 1951; M.A. Catholic 1953. An analysis of the content and influence of the economic writings of George Tucker. *Catholic.*

SUSUMU KOIZUMI, B.A. Osake 1953; M.A. Michigan 1956. Econometric models in the U.S. wiith simple industrial relations. *Michigan.*

MICHAEL C. LOVELL, B.A. Reed 1952; M.A. Stanford 1954. Manufacturers' inventories. *Harvard.*

WILL LYONS, B.S. Bucknell 1939; M.A. Harvard 1955. The balanced-budget multiplier. *Harvard.*

ALBERT A. MONTGOMERY, B.S.C. Iowa 1953; M.A. 1955. A critical analysis of the balance sheet approach to the theory of the firm. *Iowa.*

AMOS M. MOORE, B.A. Howard 1949; M.S. Alabama Polytechnic Inst. 1957. The use of Engel curves as welfare indicators. *North Carolina.*

FREDERICK C. SCHADRACK, B.A. Buffalo 1952; M.A. California (Berkeley) 1954. The determinants of expenditures on commercial construction in the United States, 1920-1955. *California (Berkeley).*

KARL A. SCHELD, B.S.C. Iowa 1952; M.A. 1954. Evaluation of the concept of the economic base. *Iowa.*

FRANKLIN R. SHUPP, B.S. Lafayette 1954; M.A. Princeton 1957. Progamming and games as a guide for national problems. *Princeton.*

DAVID H. STERN, B.A. California 1955. Operational gaming and economics. *Princeton.*

ERMIS A. THOMPSON, B.S. Florence State Teachers 1949; M.A. Alabama 1952. A study of recent economic theories of profit. *Alabama.*

JOHN W. L. WINDER, B.Comm. Toronto 1954; M.A. 1955. The supply of stocks—copper. *Chicago.*

Economic History; Economic Development; National Economies

Degrees Conferred

ALEC P. ALEXANDER, Ph.D. California (Berkeley) 1957. Economic change in Turkey, 1948-1955.

JOSEPH AMRHEIN, Ph.D. New York 1958. Burlington, Vermont: The economic history of a northern New England City.

RUBEN V. AUSTIN, Ph.D. Iowa 1958. The development of economic policy in Mexico with special reference to economic doctrines.

RUBAN C. BELLAN, Ph.D. Columbia 1958. The development of Winnipeg as a metropolitan centre.

JAMES J. BERNA, S.J., Ph.D. Columbia 1958. Entrepreneurship in Madras Province, India.

JAMES H. BLACKMAN, Ph.D. Columbia 1958. Soviet transport and the process of industrialization.

WILLIAM B. BURKE, Ph.D. Georgetown 1958. The role of the entrepreneur in Japanese economic development.

DAVID E. CARNEY, Ph.D. Pennsylvania 1958. Public agencies and economic development in British West Africa—1947-1955.

HAROLD O. CARTER, Ph.D. Iowa (Ames) 1958. Regional input-output analysis of agriculture and industry.

PENG CHANG, Ph.D. Washington 1957. The distribution and relative strength of the provincial merchant groups in China, 1842-1911.

K. J. CHARLES, Ph.D. McGill 1958. A study in economic history and theory.

RIBHI ABU EL-HAJ, Ph.D. Columbia 1957. Oil industry; a strategic factor in the economic development of Iraq.

KAMEL ABBAS EL-HALAWANI, Ph.D. Pennsylvania 1958. State and industry in Egypt. 1818-1952.

THEO H. ELLIS, Ph.D. Florida 1957. Optimum programs in Columbia and Suwannee counties, Florida.

MARGERY J. FOSTER, Ph.D. Radcliffe 1958. The economic history of Harvard College in the Puritan period (1635 to 1712).

ANDREW G. FRANK, Ph.D. Chicago 1957. Growth and productivity in Ukranian agriculture and industry, 1928 to 1955.

FAWZI HABIB, Ph.D. Duke 1958. The course and problems of an export economy: the case of El Salvador.

CHARLES HAMMOND, JR., Ph.D. Illinois 1958. Factors affecting economic growth in France, 1913-1938.

MAHBUB UL HAQ, Ph.D. Yale 1958. Planned capital formation in an underdeveloped economy: the case of Pakistan.

MOSTAFA FATHY HASSAN, Ph.D. Wisconsin 1957. The role of the government in the economic development of Egypt.

EBENEZER OGUGUA IWUAGWU, Ph.D. Wisconsin 1957. Capital formation in Nigeria: some financial and economic implications.

KENNETH M. KAUFFMAN, Ph.D. Harvard 1958. Some nineteenth century contributions to the study of economic development.

HERBERT KISCH, Ph.D. Washington 1958. The crafts and their role in the industrial revolution: the case of the German textile industry.

DUNCAN M. McDOUGALL, Ph.D. Johns Hopkins 1958. The economic growth of Canada and the United States, 1870-1955: a quantitative analysis of selected aspects.

RUSSELL U. McLAUGHLIN, Ph.D. Pennsylvania 1958. The economic development of Liberia between 1940 and 1955: a study of the role of the United States public and private investment in economic development.

ORLANDO J. MENEZES, Ph.D. Princeton 1958. Agricultural stagnation as an obstacle to industrial growth in India, 1920-1950.

OM PRAKASH NIJHAWAN, Ph.D. Nebraska 1957. Economic development and India's five-year plans.

BEN U. NZERIBE, Ph.D. Cornell 1958. The economic development of Eastern Nigeria.

RAYMOND F. PELISSIER, Ph.D. American 1958. The contribution of certain American business firms to the economic development of Mexico since World War II.

JOHN E. PERKINS, Ph.D. Texas 1958. Theories of economic development considered in relation to agricultural growth.

A. DAVID REDDING, Ph.D. Columbia 1958. Nonagricultural employment in the USSR, 1928-1955.

HARVEY H. SEGAL, Ph.D. Columbia 1956. Canal cycles, 1834-61. Public construction experience in New York, Pennsylvania and Ohio.

GEORGE A. SPIVA, JR., Ph.D. Texas 1958. Economic development in modern Greece: a study of institutional resistances.

NORMAN W. TAYLOR, Ph.D. Yale 1958. Entrepreneurship in French Canada.

RAPHAEL DE J. TORO, Ph.D. Syracuse 1957. Inducing entrepreneurship in underdeveloped countries.

ALI ASGHAR VAHABZADEH, Ph.D. Southern California 1956. Conceptual problems in the inter-industry analysis and linear projection techniques as a tool of national planning in underdeveloped countries.

C. C. VELAY, Ph.D. McGill 1957. Some aspects of the problems of economic development in underdeveloped countries.

FRANKLIN V. WALKER, Ph.D. Harvard 1958. An estimate of growth in the Texas Louisiana Gulf Coast area.

WILLIAM WOLMAN, Ph.D. Stanford 1958. The development of manufacturing industry in the State of Washington, 1899-1947.

MANUEL ZYMELMAN, Ph.D. Mass. Inst. Technology 1958. Economic history of Argentina (1933-1952).

Theses in Preparation

CECIL ALTMANN, B.A. Harvard 1955; M.A. 1957. The recovery of Austria, 1945-1955. *Harvard.*

LAWRENCE W. BARSS, B.A. Princeton 1950. Political development and economic growth. *Mass. Inst. Technology.*

ELLIOT J. BERG, B.A. New York 1949; M.A. Columbia 1955; M.A. Harvard 1957. Development of tropical Africa. *Harvard.*

H. T. BOLAND, JR., B.A. U.S. Military Academy 1949; M.A. Columbia 1956. Developmental effects of United States military assistance in selected countries. *Columbia.*

ROBERT T. BROWN, B.A. Wesleyan 1955; M.A. Harvard 1957. Criteria for determining the allocation of capital in transportation. *Harvard.*

NANDA KUMAR CHOUDHRY, B.A. Patna (India) 1948; M.S. Wisconsin 1951. Deficit financing and its effects on the economy with special reference to underdeveloped areas. *Wisconsin.*

MICHAEL J. COONEY, B.A. Columbia 1948; M.A. 1950. Motives for individual savings in Puerto Rico. *Columbia.*

ALBIN J. DAHL, B.A. California (Berkeley) 1948; M.A. 1953. The role of British investment in the economic development of California. *California (Berkeley).*

MANUEL O. DIAZ, B.A. Univ. Puerto Rico 1942; M.A. Clark 1943. The Spanish average principle as practiced in the trade between Spain and the Indies. *Pennsylvania.*

FRED DURR, M.A. Univ. Miami 1955. Regional development and commodity flows. *Kansas.*

EDWARD G. EMERLING, B.B.A. St. Bonaventure 1951; M.A. Catholic 1953. Community efforts at promoting economic development in the New England area. *Catholic.*

THOMAS O. ENDERS, B.A. Yale 1953; M.A. Harvard 1957. Capital and colonization. *Harvard.*

MARION FORRESTER, B.S. New York 1945. Capital accumulation in Kenya. *Bryn Mawr.*

ROBERT M. GELMAN, B.A. New York 1942; M.A. Catholic 1956. Economic development of the western frontier under a planned and a free economy: U.S.S.R. and Canada. *Catholic.*

GINO GIUSTI, B.S. Pittsburgh 1949; M.S. 1953. An economic study of research and development expenditures in American industry. *Pittsburgh.*

GEOFFREY B. HAINSWORTH, B.S. London School of Economics 1955. Theory and practice of colonial economic development. *California (Berkeley)*.

JOHANNES HIRSCHMEIER, B.A. St. Augustine Seminary 1951; M.A. Harvard 1957. Time horizon on the Japanese entrepreneurs. *Harvard*.

WILLIAM C. HOEKENDORF, B.A. Washington 1944; M.A. 1952. The money supply in Japan during the Meiji era. *Washington*.

IRA HOROWITZ, B.A. Johns Hopkins 1955. Industrial research in the United States. *Mass. Inst. Technology*.

MOHAMMED H. JAFRI, B.A. Allahabad (India) 1949; LL.B. 1951; M.A. California (Berkeley) 1955. Economic planning in Pakistan. *California (Berkeley)*.

PHIMOL JITTEMANA, LL.B. Thammasant (Thailand) 1952. Agriculture in a developing economy of Thailand—A mid-century appraisal. *Wisconsin*.

MARVIN E. LEE, B.A. California 1950. The economic development of the Southeastern United States. *North Carolina*.

BETTY R. McLEOD, B.A. McMasters 1945; M.A. Duke 1948. A history of immigration, settlement and economic development of Canada, 1600-1957. *Duke*.

RAMON H. MYERS, B.A. Washington 1954; M.A. 1956. Economic development of Manchuria under the Japanese—1932-1945. *Washington*.

HANEEF AHMED NASEEM, B.A. Punjab 1945; M.A. Delhi 1951. Progress of economic growth in Pakistan (1947-1957)—A critical study in retrospect. *American*.

I. D. PAL, M.A. Univ. Panjab 1948; M.Sc. London Univ. 1952. Commercial policy and economic development with reference to Pakistan. *McGill*.

SAMUEL PAUL, B.A. Madras Christian College 1950. Investment criteria in the first and second five year plans in India. *Syracuse*.

RICHARD L. PFISTER, B.A. Kansas 1948; M.A. 1950. The commodity balance of trade of the Pacific Northwest. *Mass. Inst. Technology*.

WARNASENA RASAPUTRAM, B.A. Ceylon 1950; M.A. Wisconsin 1957. National income and foreign trade of Ceylon—A structural analysis. *Wisconsin*.

BEATRICE G. REUBENS, B.A. Brooklyn College 1937; M.A. Columbia 1938. The state and private enterprise in the economic development of early New York. *Columbia*.

JULIUS RUBIN, B.A. Brooklyn College 1949; M.A. Columbia 1955. Imitation by canal or innovation by railroad: a comparative study of the response to the Erie Canal in Boston, Philadelphia, and Baltimore. *Columbia*.

JOGINDER SAHOTA, B.S. Oregon State 1952; M.A. 1954. A study of the interaction between productivity and the process of capital formation in the agricultural sector of the Indian economy. *Oregon*.

S. A. SHAH, B.Sc. Oregon State 1952; M.Sc. 1953. Structural obstacles to economic development in underdeveloped countries—India—a case study. *McGill*.

DONALD SOLAR, B.A. Wisconsin 1951. Techniques of planning economic development. *Columbia*.

SHANTI S. TANGRI, B.Sc. Univ. Panjab 1947; M.A. 1949. Patterns of investment and rates of growth, with special reference to India. *California (Berkeley)*.

GEORGE VARGHESE, M.S. Univ. College, Trivandrum, India, 1948. Technology and economic development. *New School for Social Research*.

HAZEL J. WALDROP, B.A. Montana State 1951; M.A. Southern California 1953. Theories of economic decline. *Southern California*.

ELIEZER ZVIELE, M.A. Israel Univ. 1954. Thailand's trade—patterns, objectives, and policies. *Cornell*.

Statistical Methods; Econometrics; Social Accounting

Degrees Conferred

JOHN A. BRITTAIN, Ph.D. California (Berkeley) 1958. The size distribution of income in the United Kingdom since the mid-thirties.

WILFRED V. CANDLER, Ph.D. Iowa (Ames) 1957. Linear programming with stochastic yields.

JACOB MINCER, Ph.D. Columbia 1957. A study of personal income distribution.

ALFRED P. THORNE, Ph.D. Columbia 1958. The Jamaican economy: portrayal and analysis by sector and national accounts.

Theses in Preparation

MOHAMMED A. Y. AL-AKEL, Dipl. Syrian Univ. 1951; M.S. Columbia 1956. National income accounts for Syria. *Columbia.*

ROBERT A. BANDEEN, B.A. Western Ontario 1952. State per capita automobile expenditures and income, 1930, 1940, and 1950. *Duke.*

WILLARD L. EASTMAN, B.A. Cornell 1953; M.A. Harvard 1955. The solution of programming problems containing pattern constraints. *Harvard.*

FRANKLIN M. FISHER, B.A. Harvard 1956; M.A. 1957. Selective estimation: A priori information and the analysis of time series. *Harvard.*

SEYMOUR GOODMAN, B.B.A. City (New York) 1954; M.A. Brown 1956. Income inequality in states and regions. *Johns Hopkins.*

FRANCIS X. HAMMETT, B.A. Catholic 1953; M.A. 1957. History of American statistics, 1900-1935. *Catholic.*

JAMES E. HOLSTEIN, B.A. Iowa 1952; M.A. 1955. Variable exponent production functions for manufacturing. *Iowa.*

WILLIAM J. HORNE, B.S. Boston College 1951; M.A. 1953. The economic aspects of electronic computers. *Boston College.*

Economic Systems; Planning and Reform; Cooperation

Degrees Conferred

RONALD G. RIDKER, Ph.D. Wisconsin 1958. National budgeting in Norway: a study in economic policy formation.

CHARLES J. TOBIN, Ph.D. Georgetown 1957. A theory of socio-economic organization: the economics of profit sharing.

J. HART WALTERS, JR., Ph.D. Pennsylvania 1958. The distribution of chemical fertilizers in India: a case study of the compatibility of privately-planned market distribution with public economic planning in India.

Theses in Preparation

WALTER G. MELLON, B.A. Virginia 1953; M.A. Princeton 1956. Some problems and methods in the assignment of economic priority indicators in the absence of an effective price situation. *Princeton.*

Business Fluctuations

Degrees Conferred

MEREDITH O. CLEMENT, Ph.D. California (Berkeley) 1958. The efficacy of automatic stabilizers.

MORRIS COHEN, Ph.D. Harvard 1958. Forecasting consumer and business behavior.

JOHN S. DE CANI, Ph.D. Pennsylvania 1958. Planned investment as an economic predictor.

GERALD GARB, Ph.D. California (Berkeley) 1957. Postwar inflation in Sweden, 1945-1953.

ALBERT L. GRAY, JR., Ph.D. Pennsylvania 1958. Secular movements and cycles in financial contributions to ten selected Prostestant denominations, 1900 to 1954.

MILLARD HASTAY, Ph.D. Columbia 1958. The formation of business expectations about operating variables.

DAVID T. LAPKIN, Ph.D. Columbia 1957. Building construction and building cycles.

SEYMOUR H. MILLER, Ph.D. Columbia 1957. Inflation in France, 1939-1952.

HOWARD E. MITCHELL, Ph.D. Washington 1958. The Employment Act of 1946 and the economic role of government: trends in economic thinking and prescription.

BURKE A. PARSONS, Ph.D. Texas 1958. British trade cycles and American bank credit.

HENRY M. PLATT, Ph.D. Columbia 1957. Short-run forecasting.

NEWTON Y. ROBINSON, Ph.D. Columbia 1958. The acceleration principle: department store inventories, 1920-1956.

MURRAY N. ROTHBARD, Ph.D. Columbia 1957. The panic of 1819: contemporary opinions and policy.

NORBERT D. WARREN, Ph.D. Columbia 1957. Forecasting inventory changes in the private sector of the American economy.

Theses in Preparation

PETER EILBOTT, B.A. Columbia 1953; M.A. 1956. A study of automatic stabilizers. *Columbia.*

DAVID E. NOVACK, B.A. New York 1952; M.A. Columbia 1957. Economic fluctuations in the United States, 1903-1910. *Columbia.*

ALAN A. SPIRO, B.A. Brooklyn College 1953. The business cycle and the rate of interest. *Columbia.*

HERMAN STEKLER, B.A. Clark 1955. Essays in economic forecasting. *Mass. Inst. Technology.*

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Degrees Conferred

JAMES B. BOULDEN, D.B.A. Indiana 1958. The organization structure of commercial banks.

DONALD C. BRIDENSTINE, Ph.D. Southern California 1957. Commercial banking in Arizona —past and present.

JOHN T. BURKE, Ph.D. Michigan State 1958. Commercial bank treatment of bad debt losses, its relationship to the economic and accounting concepts of income measurement, and the effects of the federal income tax requirements.

CONO CASELLA, Ph.D. New York 1958. The reorganization of the Italian investment banking system: 1922-1936.

EDWARD COOK, Ph.D. Fordham 1958. Commercial bank failures in New York State 1930 to 1940.

RAY E. DAWSON, Ph.D. Northwestern 1957. Monetary policy and sales finance and small loan companies' funds, 1949-1954.

FAWZI ABDULLA EL KAISSI, Ph.D. Southern California 1957. Critical analysis of central banking in Iraq.

OSCAR R. FLYNN, Ph.D. Columbia 1958. The positive aspect of bank capital.

PETER FOUSEK, Ph.D. Columbia 1957. Foreign central banking: the instruments of monetary policy.

WILLIAM C. FREDERICK, Ph.D. Texas 1958. Introduction to a culutral theory of money.

THOMAS J. GARDNER, Ph.D. New York 1958. Problems in the development of financial institutions among Negroes, current trends and the future in business.

ROBERT E. HARRIS, Ph.D. Pennsylvania 1958. Federal margin requirements: a selective instrument of monetary policy.

ROBERT E. HILL, Ph.D. Alabama 1957. The growth and development of foreign banking in three commercial banks in Mobile, Alabama.

PAUL M. HORVITZ, Ph.D. Mass. Inst. Technology 1958. Concentration and competition in New England banking.

BYONG KUK KIM, Ph.D. Wisconsin 1957. Central banking in a dependent economy with a special reference to Ceylon.

IRVING O. LINGER, Ph.D. Texas 1958. The role of clearing under the Federal Reserve System.

GEORGE MACESICH, Ph.D. Chicago 1958. Monetary disturbances in the United States 1834-45.

HARRY D. MALONEY, Ph.D. Indiana 1958. Monetary policy and the recession of 1953-54.

ADNAN J. MARDINI, Ph.D. American 1958. Monetary policy for economic development; a case study: Syria during the last decade.

ALLAN H. MELTZER, Ph.D. California (Los Angeles) 1957. Determinants, empirical tests and some implications of the money supply function: France 1938-1954.

ROGER F. MILLER, Ph.D. California (Berkeley) 1958. A monetary model.

JERRY MINER, Ph.D. Michigan 1958. Consumer personal debt: an intertemporal cross-section analysis.

PAUL S. NADLER, Ph.D. New York 1958. Federal Reserve policy since the March, 1951 accord.

EGON NEUBERGER, Ph.D. Harvard 1958. Central banking in semi-planned economies—Yugoslav case.

ROBERT H. PARKS, Ph.D. Pennsylvania 1958. Portfolio operations of commercial banks in the U.S. Treasury securities market.

ROBERT W. PEASE, Ph.D. New York 1957. Background for currency control.

HUBERT F. SCHIFFER, S.J., Ph.D. Fordham 1958. The modern Japanese banking system.

S. C. SCHMIDT, Ph.D. McGill 1958. Models of cyclical fluctuations in farm mortgage credit.

LAWRENCE L. WERBOFF, Ph.D. Stanford 1958. Consumer installment credit terms, consumer behavior, and monetary policy.

Theses in Preparation

YOSSEF ATTIYEH, B.A. Hebrew Univ. 1952; M.A. 1956. Wage-price spiral inflation versus demand inflation. *Chicago*.

ANATOL BALBACH, B.A. California (Los Angeles) 1951; M.A. 1952. Money supply and the price level: U.S. experience. *California (Los Angeles)*.

IRWIN I. BASKIND, B.B.A. City (New York) 1948. Post-war monetary policy in Belgium. *Chicago*.

WILLIAM R. BELMONT, B.A. DePaul Univ. 1952; M.A. George Washington 1954. Cost-price relationships and inflationary pressures. *George Washington*.

GORDON F. BOREHAM, B. Comm. Ottawa 1952; M.A. 1955. Significant changes in the monetary structure of Canada, 1953-58. *Columbia*.

DAVID CHAMBERS, B.A. Oxford 1953; M.A. Cornell 1954; M.S. Carnegie Inst. Technology. Differential effects of credit restriction in periods of high activity. *Carnegie Inst. Technology*.

KENNETH D. COURTNEY, B.A. Washington 1951; M.B.A. 1954. The intermediate-term financing of small retail business enterprises. *Ohio State*.

THEODORE R. ECK, B.A. Michigan State 1952; M.A. Michigan 1953. Comparative liquid saving flows. *Michigan State*.

RACHEL FLOERSHEIM, M.A. Hebrew Univ. 1953. Financial intermediaries in Israel, 1950-1954. *Johns Hopkins*.

Louise Freeman, B.A. Wellesley 1954; M.A. Columbia 1957. Non-bank financial inter-mediaries and monetary policy. *Columbia.*

J. A. Galbraith, B. Comm. McGill 1948; M. Comm. 1951. A study of the Canadian banking system. *McGill.*

Harry P. Guenther, B.A. Dartmouth 1955; M.B.A. Amos Tuck 1956. The effects of restrictive monetary policy on the loan and investment policies of commercial banks and life insurance companies: A case study in the St. Louis and Chicago Federal Reserve districts. *Indiana.*

Harmon H. Haymes, B.A. Lynchburg 1954; M.A. Virginia 1956. Relative effects of quantitative credit controls. *Virginia.*

Charles S. Holloway, B.A. Lincoln (Missouri) 1940; M.A. Catholic 1942. Certain aspects of the development of banking in British West Indies. *Catholic.*

Edward J. Kilberg, B.A. Hofstra 1949; M.A. Duke 1952. Commercial bank asset holdings and the liquidity trap. *Chicago.*

Ernest Kohn, B.B.A. City (New York) 1946; M.A. Columbia 1951. New York City's relative position as a commercial banking center, 1896-1957. *Columbia.*

John R. Kreidle, B.S. Grove City College 1951; M.B.A. Miami Univ. 1952. Theories of inflation and consumer instalment credit. *Ohio State.*

Basil J. Moore, B.A. Toronto 1955. The effects of monetary policy upon the financial position of Canadian chartered banks, 1935-1957. *Johns Hopkins.*

Hugh T. Patrick, B.A. Yale 1951; M.A. Michigan 1955. Credit policy of the Bank of Japan since 1949. *Michigan.*

John C. G. Peret, B.S. Missouri 1947; M.A. 1950; M.A. Harvard 1954. Some effects of changes in the rate of interest on the share of interest income in national income. *Harvard.*

Ira J. Rapson, Jr., B.A. Michigan 1951; M.A. 1952. Consumer credit and the small claims court in Madison. *Wisconsin.*

Gunter Rischer, B.A. Free University, Berlin, 1954; M.A. Harvard 1957. The impact of institutional investors on the stock market. *Harvard.*

Karl W. Roskamp, B.A. Frankfort 1956; M.A. Michigan 1955. Monetary policy in Western Germany. *Michigan.*

Donald H. Sauer, B.S. Indiana 1954; M.B.A. 1955. The supply of and demand for mortgage funds 1960-1970. *Indiana.*

Arthur T. Taitt, B.S., B.A. Denver 1948; M.B.A. 1949. Case studies and executive development programs in commercial banking since 1946. *Indiana.*

Robert J. Trusk, B.S. St. Ambrose 1951; M.S. Columbia 1954. Money and banking in the Ottoman Empire in the 19th century. *Illinois.*

W. Okefie Uzoaga, M.A. Cornell 1953. The monetary and banking system of Nigeria. *Fordham.*

Leonard P. Vidger, B.A. Seattle Pacific 1948; M.S. Southern California 1954. The Federal National Mortgage Association. (General background, appraisal and devaluation of this institution.) *Washington.*

Leroy S. Wehrle, B.A. Washington (St. Louis) 1953. A study of institutional demand for securities. *Yale.*

Gordon P. Wonnacott, B.A. Western Ontario 1955; M.A. Princeton 1957. Canadian monetary policy since 1950. *Princeton.*

Public Finance; Fiscal Policy

Degrees Conferred

George Babilot, Ph.D. Oregon 1958. An analysis of direct and indirect taxation: a reordering of some basic concepts in tax theory.

JOHN C. BOWEN, Ph. D. Michigan 1958. Some aspects of transfer taxation in the United States.

LESLIE CARBERT, Ph.D. Columbia 1958. The impact of state and local taxes in North Carolina and the southeastern states.

SIDNEY J. CLAUNCH, Ph.D. Wisconsin 1958. Urban decentralization, suburbanism and fiscal equity.

LEO COHEN, Ph.D. California (Los Angeles) 1958. A measurement and analysis of the built-in flexibility of the individual income tax.

ROBERT L. DARCY, Ph.D. Colorado 1957. Local income taxation: City of Denver as a case study.

NORMAN DRESSEL, Ph.D. New York 1958. Budgeting in the public economy: lessons of the TVA experience.

REED R. DURTSCHI, Ph.D. Washington 1957. Life insurance companies under the federal income tax.

TARIQ EL-MUTWALLI, Ph.D. American 1958. The tax system of Iraq: a study of taxation in a developing country.

LOUIS FIER, Ph.D. New York 1958. The fiscal economics of state bonuses for veterans.

HENRY J. FRANK, Ph.D. Columbia 1958. The New Jersey property tax—substitute for state aid?

GEORGE HANC, Ph.D. Columbia 1958. United States savings bonds and federal debt management.

HARVEY I. HENANN, Ph.D. New York 1958. Commodity taxes and stabilization policy.

JAMES W. JOHNSTON, Ph.D. Indiana 1958. Succession duties in Canada.

BONG HYOK KAY, Ph.D. Wisconsin 1958. Fiscal and taxation policy in a developing economy with special reference to the Republic of Korea.

SIDAGOUDA KHOT, Ph.D. Illinois 1958. Development of tax ideas in India.

LEON E. KRAUSE, Ph.D. New York 1958. Management of the federal debt.

SANG SOO KWAK, Ph.D. Wisconsin 1958. Selective consumption taxes.

MICHAEL E. LEVY, Ph.D. Columbia 1958. The effect of personal exemptions on the income tax structure.

HUGH H. MACAULEY, JR., Ph.D. Columbia 1957. Taxation of fringe benefits.

JACKSON R. E. PHILLIPS, Ph.D. Columbia 1957. New York City—New York State fiscal relations.

DONALD E. ROARK, D.B.A. Indiana 1958. The impact of the federal income tax on residential real estate developers.

CHING SHENG SHEN, Ph.D. North Carolina 1957. Theory of built-in flexibility: a study of the built-in flexibility of the North Carolina individual income tax and the prediction of North Carolina individual income tax yield.

WILLIAM M. SHENKEL, Ph.D. Washington 1957. An evaluation of assessment ratio studies in selected states.

JARED SPARKS, JR., Ph.D. Illinois 1958. Deflationary aspects of excise taxes in Canada.

RICHARD E. SPEAGLE, Ph.D. Princeton 1958. Comparative public debt management: Canada and the United States, 1939-1946.

WILLIAM F. TAUCHAR, Ph.D. California (Berkeley) 1958. San Francisco: a study in local finance.

WILLIAM J. THOMAS, Ph.D. Michigan State 1957. Revenue bond financing.

WILLIAM A. VOGELY, Ph.D. Princeton 1958. A case study in the measurement of government output.

MURRAY L. WEIDENBAUM, Ph.D. Princeton 1958. Government spending: process and measurement.

PAUL J. WELLS, Ph.D. Stanford 1958. A theoretical analysis of excise tax incidence.

Theses in Preparation

JOSEPH M. BONIN, B.S. Spring Hill College 1950; M.A. Louisiana State 1952. Critical analysis of the state debt structure and debt management in Louisiana. *Louisiana State.*

GERALD BOYLE, B.A. Colorado College 1951; M.A. New Mexico 1952. Service charges as a source of public revenue. *Syracuse.*

ARLAND CHARLTON, B.A. Northeastern 1953; M.A. Ohio 1955. Grants in aid and the Highway Act of 1956. *Syracuse.*

RAJA CHELLIAH, B.A. St. Xaviar (India) 1952; M.A. Madras 1950. Some problems of fiscal policy in under-developed countries with special reference to India. *Pittsburgh.*

DAVID C. COLE, B.A. Cornell 1950; M.A. Michigan 1954. Provincial and local revenue of Vietnam. *Michigan.*

MAYNARD S. COMIEZ, B.A. Syracuse 1953; M.A. 1955. Income tax administration: A study of the income tax in the United States and Wisconsin and the auditing and enforcement techniques. *Wisconsin.*

JOHN D. COUPE, B.S. Worcester Polytechnic Inst. 1953; M.A. Clark 1957. Structural blocks to countercyclical action at state and local levels of government in Massachusetts. *Clark.*

IRVING J. GOFFMAN, B.A. McGill 1954; M.A. Duke 1957. Erosion of the income tax base in Canada and the United States. *Duke.*

SEVIM GORGUN, B.A. Am. College for Girls, Istanbul, 1947. Classical, welfare and Keynesian tax theory. *Syracuse.*

JOHN F. GRAHAM, B.A. British Columbia 1947; M.A. Columbia 1948. An optimum local-aid program in a low-income province: Nova Scotia. *Columbia.*

WALTER T. GREANEY, JR., B.A. Boston College 1942; L.L.B. Boston Univ., 1948; L.L.M. 1949; M.A. Harvard 1954. An analysis of the mass distribution and budget systems. *Harvard.*

CLIFTON M. GRUBBS, JR., B.A. Texas 1948; M.A. 1955. Motor carrier taxation. *Harvard.*

WILLIAM E. GUSTAFSON, B.A. Williams 1955; M.A. Harvard 1957. A model for predicting local government receipts and expenditures. *Harvard.*

WILLIAM R. HENDLEY, B.A. Yale 1956. Swedish fiscal policy, 1946-1956. *Duke.*

BADRI D. JAIN, B.A. Panjab—India, 1939; M.A. 1942; M.A. Vanderbilt 1948. Deficit financing and economic development—A case study of India. *Vanderbilt.*

HARRY H. LANDRETH, JR., B.S. Oklahoma 1950; M.A. 1951; M.A. Harvard 1957. Intrastate equalization: theory and practice. *Harvard.*

SUSAN J. LEPPER, B.A. Swarthmore 1955. Relation of tax structures to individuals deposition of wealth. *Yale.*

ALLYN O. LOCKNER, B.A. Huron 1954. State taxation of selected metalliferous mines and mineral resources in Colorado. *Colorado.*

HANUMANTHA RAO MACHIRAJU, B.A. Andhra Univ. 1952; M.A. 1954; LL.B. Osmania Univ., India, 1954. The expenditure tax in India. *Indiana.*

HAROLD F. McCLELLAND, B.A. Hastings 1939; M.A. Nebraska 1940; M.S. Denver 1942; M.A. Harvard 1957. Tax aspects of the variable annuity. *Harvard.*

JOHN C. METTLER, B.A. Washburn 1938; M.A. Rutgers 1939. Proposals for improved budgetary control. *Clark.*

PUSHKAR N. PANT, B.A. Patna 1952; M.A. Benares Hindu 1955. A study on the budgetary problems of the underdeveloped countries with special reference to Nepal. *Vanderbilt.*

JAMES A. PAPKE, B.A. Wayne 1954; M.A. Michigan 1955. An empirical analysis of the relative impact on corporations of a tax on value-added product. *Cornell.*

CHARLOTTE D. PHELPS, B.A. Radcliffe 1955. The effects of tight money on capital improvement programs in municipalities which maintain capital budgets. *Yale.*

W. DOUGLAS POE, B.S. Oklahoma A. & M. 1939. Municipal sales tax in the United States. *Indiana.*

THE AMERICAN ECONOMIC REVIEW

ARNOLD H. RAPHAELSON, B.A. Brown 1950; M.S. Columbia 1951; M.A. Clark 1956. The countercyclical effectiveness of unemployment compensation benefits and taxation in Massachusetts, 1948-1957. *Clark.*

HARVEY SHAPIRO, B.A. City (New York) 1953; M.A. Wisconsin 1954. Metropolitan finance in Madison, Wisconsin. *Wisconsin.*

NED SCHILLING, B.S. Purdue 1949; M.A. Columbia 1955. Theory of monopoly regulation through taxation. *Columbia.*

ROBERT L. SLIGHTON, B.A. Princeton 1953. The treatment of losses and amortization of capital in the federal corporate income tax; A study of incentive taxation. *Johns Hopkins.*

DONALD F. SWANSON, B.A. Knox 1950; M.A. Florida 1956. The English origins of Hamilton's fiscal policies. *Florida.*

EDSEL E. THRASH, B.A. Louisiana State 1950; M.B.A. 1951. Public financing of higher education in Louisiana. *Louisiana State.*

RAYMOND VALENTI, B.S. Fordham 1938; M.B.A. Syracuse 1954. Life insurance company taxation. *Syracuse.*

RAY O. WERNER, B.A. Hastings 1942; M.A. Nebraska 1947. Some economic aspects of major legislative proposals for the federal support of primary and secondary education in the United States, 1946-1958. *Nebraska.*

GEORGE S. ZARKOS, Law Degree, Athens, Greece 1950; M.A. Indiana 1956. The Greek system of sales taxation. *Indiana.*

International Economics

Degrees Conferred

NAN L. AMSTUTZ, Ph.D. Fletcher School 1958. The establishment of an Indonesian importing class, 1950-1953.

ZARKO BILBIJA, Ph.D. Chicago 1958. American long-term investments abroad and the export of capital goods: Latin American experience 1944-54.

RICHARD E. CAVES, Ph.D. Harvard 1958. Recent developments in general-equilibrium international trade theory.

DALE L. CRAMER, Ph.D. Louisiana State 1958. Relationship between monetary and fiscal policy and external policy, and their effect upon the national income and price levels in Latin America.

NADAV HALEVI, Ph.D. Columbia 1956. Estimates of Israel's international transactions, 1952, 1953 and 1954.

ELMER HARMON, Ph.D. Columbia 1956. Bejamin Graham and Jon Gondriaan's international commodity reserve currency proposal: a critical evaluation and comparison with other solutions to the problem of raw materials.

GERALD E. HARRIMAN, Ph.D. Cincinnati 1958. The economics of the transition between protectionism and free trade.

ANDREW IMRIK, Ph.D. Saint Louis 1958. Development of the idea of European economic integration.

HARRY G. JOHNSON, Ph.D. Harvard 1958. International trade and economic growth.

PETER B. KENEN, Ph.D. Harvard 1958. Monetary policy and the British balance of payments 1954-56.

THOMAS M. KLEIN, Ph.D. Michigan 1958. The United Kingdom's 1947 balance of payments crisis.

GUENTER KNACKSTEDT, Ph.D. Cincinnati 1958. The special position of the under-developed countries in GATT.

LAWRENCE B. KRAUSE, Ph.D. Harvard 1958. Current balance of payments problems of industrial countries: an empirical study.

MILDRED G. MASSEY, Ph.D. Oregon 1958. The Italian balance of payments, 1945-1955.

GUENTER H. MATTERSDORFF, Ph.D. Harvard 1958. Considerations for the economic integration of Western Europe.

RODNEY H. MILLS, Jr., Ph.D. Columbia 1958. Italy's international economic transactions, 1946-1955.

JERZY Z. MIRSKI, Ph.D. Georgetown 1958. The dollar problem.

AZIZALI FARRUKH MOHAMMED, Ph.D. George Washington 1958. Some aspects of the economic impact of foreign aid on an underdeveloped country: the case of Pakistan.

FRANCIS MURANS, Ph.D. Michigan State 1957. The Reciprocal Trade Agreement program, 1945-1956.

ERNEST W. OGRAM, Jr., Ph.D. Illinois 1957. Canada's postwar balance of payment adjustments.

ROBERT W. OLIVER, Ph.D. Princeton 1957. The origins of the International Bank for Reconstruction and Development.

LESLIE C. PEACOCK, Ph.D. Texas 1958. Policies and operations of the International Monetary Fund: 1947-1956.

BELINDA K. PEARSON, Ph.D. Fletcher School 1958. Commodity reserve currency with special reference to monetary aspects.

ROLF PIEKARZ, Ph.D. Johns Hopkins 1958. Proportion of foreign trade in national product, and economic growth.

JEROME M. PINES, Ph.D. Columbia 1958. United States economic policy toward Germany, 1945-1949.

GERALD A. POLLACK, Ph.D. Princeton 1958. The effect on imports from Canada of United States tariff reductions under the Reciprocal Trade Agreements Program.

ROBERT J. SCHWARTZ, Ph.D. American 1958. Obstacles to U.S. private foreign investments 1946-1953.

CLAUDIO SEGRÉ, Ph.D. Yale 1958. An economic analysis of the financing of capital goods exports from European countries.

ARTHUR O. SHARON, Ph.D. American 1958. An analysis of Japan's external disequilibrium 10 years after the end of World War II.

DAVID W. SLATER, Ph.D. Chicago 1957. The growth and structure of Canadian imports, 1926-1955.

JOHANNES G. SNAAUW, Ph.D. New School for Social Research 1958. Problems of industrial adjustment in the European Coal and Steel Community.

EGON SOHMEN, Ph.D. Mass. Inst. Technology 1958. Economics of flexible exchanges.

BABETTE S. SOLON, Ph.D. Mass. Inst. Technology 1958. International trade in automobiles during 1946-1956: a study of the charges in the composition of trade.

ROBERT M. STERN, Ph.D. Columbia 1958. World food exports and United States agricultural prices.

JOSEPH F. TALARICO, Ph.D. Rutgers 1958. The postwar pattern of commerce and finance between the United States and Canada, 1946-1953.

ERICK THORBECKE, Ph.D. California (Berkeley) 1957. The tendency towards regionalization in international trade, 1928-1954.

JAROSLAV VANEK, Ph.D. Mass. Inst. Technology 1958. Natural resource content of the United States foreign trade, 1870-1955.

JOE R. WILKINSON, Ph.D. Fletcher School 1958. Politics and trade policy: a study in the nature of the political support for the Reciprocal Trade Agreements Program, 1934-1955.

HAROLD A. WOLF, Ph.D. Michigan 1958. The United States sugar policy and its impact upon Cuba: a reappraisal.

ABDUL-HASSAN ZALZALAH, Ph.D. Indiana 1957. Iraq in the Sterling Area, 1932-1954: a study in economic-political relationships.

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EVANGELOS DEVLETOGLOU, Dipl. Univ. Athens 1954. Conditions of equilibrium in the foreign exchange markets. *Columbia.*

WILLIAM E. ESCOUBÉ, Ph.B. Chicago 1949; B.A. Harvard 1951; M.A. California (Berkeley) 1955. International trade in capital goods. *Columbia.*

RONALD FINDLAY, B.A. Rangoon 1954. Economic development and the terms of trade. *Mass. Inst. Technology.*

ISAIAH FRANK, B.S.S. City (New York) 1936; M.A. Columbia 1938. The European common market and third countries. *Columbia.*

MARJORIE S. GALENSON, B.A. Barnard 1937; M.A. Columbia 1943. Some problems involved in the European common market. *California (Berkeley)*

RICHARD L. GORDON, B.A. Dartmouth 1956. Problems of coal pricing under the European coal and steel community. *Mass. Inst. Technology.*

ROBERT F. HALTMIER, B.A. Hofstra 1950; M.A. Columbia 1951. Economic aspects of U.S. petroleum imports. *Columbia.*

BERNARD B. HANON, Dipl., Faculté de Droit, Paris 1955; M.B.A. Columbia 1956. The impact of the common market on the development of the automobile industry among the common market signatories. *Columbia.*

ERNEST E. HEIMBACH, M.S.Sc. New School for Social Research 1943; M.B.A. New York. 1946; M.A. 1951. Reciprocal trade policy of United States with Latin America. *New School for Social Research.*

WILLIAM R. HOSKINS, B.A. Washington 1952; M.B.A. Indiana 1953. Profit opportunities for American subsidiaries in Western Europe. *Indiana.*

STEPHEN HYMER, B.A. McGill 1955. Direct foreign investment. *Mass. Inst. Technology.*

CHUAY KANNAWAT, B. Laws Thammasat (Thailand) 1949; B.A. Chicago 1954; M.A. Wisconsin 1957. Structural change in the pattern of world trade after World War II as a cause of world payment problems. *Wisconsin.*

ITSUO KAWAMURA, B.A., M.A. Nagoya Univ. (Japan). Foreign trade and economic development of Japan. *Johns Hopkins.*

MARION KRZYZANIAK, M.Econ. Poznan 1932; M.A. Alberta 1954. Homomorphic index-numbers and elasticity theorems in an open economy. *Mass. Inst. Technology.*

KLAUS NETTER, B.S. Syracuse 1950. German foreign economic relations since World War II. *California (Berkeley).*

JAMES W. NORDYKE, B.A. Stanford 1952; M.A. Princeton 1957. The role of private capital export from the United States in the determination of the importance of New York as a center of international finance. *Princeton.*

ALAN R. PLOTNICK, B.A. Temple 1949; M.A. Pennsylvania 1951. The Canadian government foreign policy—its reorientation 1946-1958. *Pennsylvania.*

DAVID C. SMITH, B.A. McMaster 1953; B.A. Oxford 1955; M.A. Harvard 1957. Some aspects of the economic inter-connections between United States and Canada. *Harvard.*

JEANETTE STOOPS, B.S. Purdue 1949; M.S. 1954. The effect of bulk purchase contracts on the New Zealand economy. *Bryn Mawr.*

MAHMOOD TABATABAI, Lic. Fribourg (Switzerland) 1950. Balance-of-payments aspects of economic development in Iran. *Columbia.*

MARIANNE Y. VICTORIUS, B.A. Guilford 1950. The role of the exchange rate in a non-viable economy: Korea. *Columbia.*

DONALD F. WAHL, B.A. Middlebury 1953; M.A. Harvard 1955. An application of international trade theory to Canada's foreign trade. *Harvard.*

RONALD J. WONNACOTT, B.A. Western Ontario 1955; M.A. Harvard. An input output analysis of the interrelation of the Canadian and United States economics. *Harvard.*

Business Finance; Investment and Security Markets; Insurance

Degrees Conferred

VICTOR L. ANDREWS, JR., Ph.D. Mass. Inst. Technology 1958. Investment practices of corporate pension funds.

JOHN C. DAWSON, Ph.D. Cornell 1958. Fluctuations in United States corporate investment and finance. 1931-1950. A source and uses of funds analysis.

DANIEL E. DIAMOND, Ph.D. New York 1958. The economic impact of life insurance investments on the American economy.

PHILIP ELKIN, Ph.D. Pennsylvania 1958. The financial experience of beneficiaries of personal living trusts in Philadelphia, 1920-1954.

MAX E. FESSLER, Ph.D. Columbia 1958. Determining pure premium rates for hail insurance on Kansas wheat.

ROBERT L. HILL, Ph.D. Georgetown 1958. Aggregate investment behavior of personal trusts administered by corporate trustees.

RICHARD W. HOOLEY, Ph.D. Columbia 1957. Natural gas financing: a study of the adaptability of life insurance investment policies.

DONALD P. JACOBS, Ph.D. Columbia 1957. Sources and costs of funds of large sales finance companies.

HENRY KAUFMAN, Ph.D. New York 1958. The problem of funds for small business.

HENRY A. LATANE, Ph.D. North Carolina 1957. Rational decision making in portfolio management.

ARTHUR W. MASON, JR., Ph.D. Pennsylvania 1958. Marketing practices and newly organized life insurance companies.

ROBERT B. McCOSH, D.B.A. Indiana 1958. Investment opportunities in the United States 1800-1836.

SUZANNE S. McWHORTER, Ph.D. Ohio State 1957. An analysis and evaluation of the marketing organizations, policies, and procedures of selected accident and health insurance companies.

REX C. OLSSON, Ph.D. New York 1958. Techniques of financial planning in United States manufacturing corporations.

CLIFFORD F. OWEN, Ph.D. Toronto 1958. Business financing and taxation policies.

KARL W. SCHLUBACH, Ph.D. New York 1958. The rate of return on investment in financial management and investment analysis.

HAROLD W. STEVENSON, Ph.D. Michigan 1957. Common stock financing in 1955.

FRANCIS L. STUBBS, Ph.D. Wisconsin 1958. Financing of small and medium-sized Wisconsin manufacturing corporations, 1946-1950.

FRANK L. TURGEON, Ph.D. New York 1958. An investment analysis of the common stocks of the domestic agricultural equipment industry.

FESTUS J. VISER, Ph.D. New York 1958. A study of economic analysis of corporate savings.

LELAND C. WHETTEN, Ph.D. Alabama 1957. The proxy contest—its nature, cause and significance.

Theses in Preparation

DAVID ABNER III, B.A. Texas Southern 1950; M.A. 1951. Some aspects of the current status and comparative growth of Negro legal reserve life insurance companies 1930-1956. *Indiana*.

DAVID A. BAERNCOPF, B.A. Indiana 1942; M.A. Stanford 1956. The structure of California bond rates, 1900-1957; an historical study of municipal bond yields. *Stanford*.

WINSTON C. BEARD, B.A. Ouachita College 1953; M.B.A. Arkansas 1954. Financial and economic effects of geographical restrictions upon the investment policies of life insurance companies. *Illinois*.

HASKEL BEN-ISHAY, M.A. Chicago 1955. Determinants of the differences in rates of return on corporate equity. *Chicago*.

DAVID BICKELHAUPT, B.S. Pennsylvania 1951; M.S. Columbia 1952. Multiple-line insurance. *Pennsylvania*.

RAYMOND C. BOLY, B.S. New York 1950; M.B.A. 1953. An investment appraisal of natural gas transmission company common stocks. *New York*.

SISTER MARTIN BYRNE, B.A. Marymount (New York) 1946; M.B.A. California (Los Angeles) 1953. Religious organizations and religious body financing. *California (Los Angeles)*.

JOHN A. CLINKENBEARD, B.A. Chicago 1947; M.B.A. 1947. An evaluation of short-term investment policies and practices of large nonfinancial corporations and their interrelationships to the money market. *Chicago*.

JACK W. COLEMAN, B.S. Kansas 1946; M.B.A. Michigan 1952. An analysis of the working capital fund concept. *Indiana*.

RUPERT M. CRAIG, B.A. Atlantic Union 1941; M.A. Boston Univ. 1947. The use of sale and leaseback devices for financing. *California (Los Angeles)*.

LYNN E. DELLENBARGER, JR., B.A. Duke 1953; M.B.A. Florida 1956. A study of terms of mergers of listed industrial corporations 1950-1957. *Florida*.

JOHN P. DEVEREAUX, B.S. Rutgers 1940; M.A. Columbia 1941. History and analysis of accounting reserves. *Pittsburgh*.

ROBERT L. HAMMAN, B.S. Mass. Inst. Technology 1949; M.S. 1954; M.A. Harvard. Capital budgeting. *Harvard*.

JAMES D. HAMMOND, B.A. Northwest Missouri State 1955; M.A. Pennsylvania 1956. Termination of pension plans. *Pennsylvania*.

MUSA YUNIS HUSSAYNI, B.B.A. Am. Univ. Beirut 1935; M.B.A. 1950. Corporate profits and venture capital in the postwar period. *Michigan*.

EASO JOHN, M.B.A. Northwestern 1955. Investment experience of twenty-five selected fire insurance companies, 1926-1956. *Northwestern*.

MICHAEL KEARNEY, B.S., M.A. California (Los Angeles). Title insurance. *Pennsylvania*.

KEDAR N. KOHLI, B.A. Delhi Univ. 1948; M.A. 1950; M.A. Vanderbilt 1958. Private capital markets and industrial development: a comparative study of India, Germany, United Kingdom, and the United States. *Vanderbilt*.

ROBERT H. MILLS, B.S. Colorado 1949; M.S. 1955. A study of the effect of price level changes on business income and business planning. *Wisconsin*.

FREDERICK E. MUELLER, Ph.B. Marquette 1948; M.B.A. Denver 1949. The causes of failure in asset liquidation in bankruptcy cases—Milwaukee County. *Wisconsin*.

J. RUSSELL NELSON, B.A. Pacific Union 1952; M.B.A. California (Los Angeles) 1957. The role of rights in corporate financial policy with particular attention to price effects on the stock. *California (Los Angeles)*.

LEROY L. PHAUP, JR., B.A. Randolph-Macon 1952. Juvenile insurance. *Pennsylvania*.

JAMES P. QUIRK, B.B.A. Minnesota 1948; M.A. 1949. Increasing risk and lender behavior. *Minnesota*.

RONALD M. REIFLER, M.B.A. Chicago 1948. Financial effects of a change in plant location. *Chicago*.

STUART SCHWARTZSCHILD, B.S. Univ. Richmond 1938. Creditor rights in life insurance and annuities. *Pennsylvania*.

BENJAMIN S. STEVENSON, B.A. Kenyon 1951. The common trust fund. *Ohio State*.

GLEN TAYLOR, B.B.A. North Texas State 1950; M.B.A. 1953. The nature and development of aircraft hull insurance in the U. S. *Pennsylvania*.

ROBERT D. TUCKER, B.A. Colorado College 1931; M.B.A. California (Los Angeles) 1948. The shortcomings of the Securities and Exchange Commission. *California (Los Angeles)*.

KENNETH J. WELLER, B.A. Hope 1948; M.B.A. Michigan 1949. An analysis & evaluation of the rights offering as a source of equity capital. *Michigan.*

EDMOND WOOLRYCH, B.A. Principia 1943; M.B.A. Harvard 1947. The effect of the growth of pension funds on other financial intermediaries. *Syracuse.*

Business Organization; Managerial Economics; Marketing; Accounting

Degrees Conferred

JAMES D. BENSON, Ph.D. Iowa 1958. A study of the application of certain statistical and cartographic tools to a quantitative problem in market analysis.

WILMAR F. BERNTHAL, D.B.A. Indiana 1957. A study of the foreman's committee role in the management of selected industrial plants.

WILLIAM T. BLOMQUIST, D.B.A. Indiana 1957. The relationship between job satisfaction and personnel practices among selected Ohio hospitals.

RAYMOND D. BUTEUX, Ph.D. New York 1957. On the indirect extension of advertised ideas.

ROBERT D. BUZZELL, Ph.D. Ohio State 1957. Productivity in marketing: with special reference to drug and hardware wholesalers.

NORMAN S. CANNON, Ph.D. Columbia 1958. Some selected problems in accounting for executive compensation.

ROCCO CARZO, JR., D.B.A. Indiana 1958. Inventory decision-making and economic theory.

ROBERT J. CHAPEL, Ph.D. Pittsburgh 1958. The control of sales by manufacturers: a theory of successful sales management.

CLARK E. CHASTAIN, Ph.D. Michigan 1958. Depreciation policies of the steel industry for 1940-56 with special emphasis on the adequacy of depreciation allowances.

RONDO A. CHRISTENSEN, Ph.D. Cornell 1957. A business management study of seventy-three retail farm equipment firms, New York, 1955.

ROBERT G. COOK, D.B.A. Indiana 1958. Liaison in management organization theory.

JOHN W. DARR, Ph.D. Alabama 1957. Selected problems of personnel administration.

HARRISON L. GRATHWOHL, D.B.A. Indiana 1957. The impact of trading stamps on retail advertising and pricing practices in Indianapolis, Indiana.

CHADWICK J. HABERSTROH, Ph.D. Minnesota 1958. Processes of internal control in firms.

MARVIN HOFFMAN, Ohio State 1957. Supplementary wage benefits for outside salesmen.

ALBERT HOLZMAN, Ph.D. Pittsburgh 1958. Some aspects of the problem of unplanned plant maintenance.

ALLEN F. JUNG, Ph.D. Chicago 1957. Estimation of the variation in costs to retail buyers of automatic washing machines.

BARKEV KIBARIAN, Ph.D. New York 1958. A definitive study of the women's costume-jewelry industry from a managerial viewpoint.

RONALD R. LARSON, D.B.A. Indiana 1957. The impact of trading stamps on consumers in Indianapolis, Indiana.

JOHN MASEK, Ph.D. Chicago 1957. A study of the usefulness of motivation research in improving the efficiency of direct mail selling efforts.

FREDERICK E. MAY, Ph.D. Michigan 1958. Attributes of car owners influencing their car purchasing behavior.

BERT C. McCAMMON, JR., D.B.A. Indiana 1957. The effect of trading stamps on the operations of Indianapolis retailers.

EDMUND J. McCARTHY, Ph.D. Minnesota 1958. An analysis of the use of marketing research in product development.

BRUCE D. McSPARRIN, JR., D.B.A. Indiana 1958. A study of the role of the purchasing executive and his function in materials management in manufacturing firms.

HENRY L. MUNN, Ph.D. Chicago 1957. An exploratory investigation of brand perception by specified classes of consumers for specified classes of consumer goods.

JEAN NAMIAS, Ph.D. New York 1958. A study of the relationship between consumers' intentions and their actions in the market for household goods.

WARREN B. NATION, Ph.D. Alabama 1957. Schwegmann Brother—a study of supermarket growth and its economic significance.

WALTER G. O'DONNELL, Ph.D. Columbia 1958. The value structure of corporate decisions.

ERIC C. OESTERLE, Ph.D. Purdue 1957. A financial analysis of independent food stores in Indiana.

LEONARD W. PRESTWICH, Ph.D. Ohio State 1957. Store hours in retailing with particular emphasis on night openings.

HAROLD F. PUFF, D.B.A. Indiana 1957. Organization practices and problems of a selected group of small manufacturing companies in southwestern Ohio.

LORENZO T. REEVES, JR., Ph.D. Northwestern 1958. A study of changes occurring in the wholesaling of major appliances since 1920, with emphasis on the period 1939-1956.

ISAAC N. REYNOLDS, Ph.D. North Carolina 1957. The impact of the choice of base and method of amortization of long-term cost on financial and other business policies.

MARGUERITE L. RITTENHOUSE, Ph.D. New York 1958. An examination of marketing research theories and practices for the introduction of new products.

WILLIAM G. SCOTT, D.B.A. Indiana 1957. The design and use of a human relations training program for middle management.

MALCOLM F. SEVERANCE, Ph.D. Wisconsin 1958. Budgeting, programming, and decision-making in excellently managed companies.

WILFRED N. SMITH, D.B.A. Indiana 1958. Production management and the laboratory method.

BURNARD H. SORD, Ph.D. Texas 1958. A study of planning and control policies and practices employed by selected American companies, with special emphasis on budgeting.

LYNN H. STOCKMAN, Ph.D. Northwestern 1957. The influence of consumer deals on urban household purchases of butter, margarine, vegetable shortening, and salad and cooking oils in metropolitan Chicago.

REED K. STOREY, Ph.D. California (Berkeley) 1958. Matching revenues with costs: an analysis of accounting adaptation to uncertainty.

DANIEL L. SWEENEY, Ph.D. Michigan 1958. Accounting and financial problems of executive stock option plans.

HARLON D. TRAYLOR, Ph.D. Cornell 1958. Some effects of film packages and uniform sizing on retail potato sales.

ROBERT T. TUSSING, Ph.D. Texas 1957. A survey of electronic data processing and its potential impact upon accounting procedures, personnel, and education.

LOUIS D. VOLPP, Ph.D. Iowa 1958. A theoretical approach to the determination and use of sales potentials for geographic allocation of marketing efforts.

JOHN S. WAGLE, Ph.D. Ohio State 1957. An analysis of the marketing of utility airplanes with emphasis on marketing practices and problems of manufacturers.

EDWARD L. WALLACE, Ph.D. Chicago 1957. Electronic data processing equipment in production planning and inventory control.

C. EDWARD WEBER, Ph.D. Princeton 1958. Managerial growth and development: a tentative explanation of the increasing use of managerial manpower in comparison to total manpower.

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DEAN S. AMMER, B.S. Mass. Inst. Technology 1948; M.B.A. New York 1956. The theory of materials management. *New York.*

SAMUEL R. ANDERSON, B.B.A. Michigan 1952; M.B.A. 1953. The impact of the "new" competition upon the small department store. *Michigan.*

THOMAS L. BERG, B.B.A. Minnesota 1951; MS. Columbia 1956 The management of marketing channels. *Columbia.*

CHARLES P. BONINI, B.A. Holy Cross 1955; M.S. Carnegie Inst. Technology 1956. A study of accounting and other information flows and their effects upon the decisions of a firm. *Carnegie Inst. Technology.*

FREDERICK A. BRETT, B.S. Alabama 1954; M.S. 1955. Problems encountered in using accounting data for economic analysis. *Alabama.*

LEONARD R. BURGESS, B.A. Brown 1942; M.B.A. Harvard 1947. Compensation of top executives in industry. *Columbia.*

PAUL W. BURNHAM, B.A. Hardin-Simmons 1934; M.B.A. Texas 1939. The integration of accounting with the economic development of the South—1900-1950. *Alabama.*

BUFORD A. CASEY, B.A. Bridgewater 1938; M.B.A. Harvard 1947. New industrial products marketing in theory and practice. *Ohio State.*

WILLIAM H. COOPER, B.S. Grove City 1937; M.B.A. Pennsylvania 1949. Comparisons and contrasts between the administrative philosophies of the Elton Mayo School and Frederick W. Taylor. *Pennsylvania.*

JACOB D. CORRIHER, JR., B.S. Bowling Green 1949; M.B.A. Indiana 1950. Application of generally accepted principles of accounting to railroad operations. *Alabama.*

ALFRED A. COX, B.S.E. Arkansas State Teachers 1949; M.B.A. Arkansas 1954. The marketing of new automobiles by franchised dealers. *Ohio State.*

EDWIN W. CROOKS, JR., B.S. West Virginia 1941; M.B.A. Harvard 1947. The history of the Halle Bros. Co. *Indiana.*

H. ROBERT DODGE, B.S. Ohio State 1951; M.B.A. 1954. Incentive compensation for sales executives. *Ohio State.*

FRANKLIN B. EVANS, B.A. Chicago 1943; M.B.A. 1954. Analysis of automobile purchasers: the discriminatory efficacy of demographic and psychological variables. *Chicago.*

PAUL F. FAGAN, B.A. Connecticut 1949; M.A. 1950. Profit control for decentralized divisions. *Columbia.*

WILLIAM A. FLINN, B.S. Davidson 1933; M.B.A. Harvard 1936. Retail Credit Company, Inc.—A case study in marketing management. *Ohio State.*

JAY R. GREENE, B.A. Los Angeles State 1949; M.B.A. California 1951. Use of general-purpose electronic computers in the solution of marketing problems. *Ohio State.*

GEORGE R. HALL, B.A. Claremont 1951; M.A. Harvard 1953. The lumber industry: workable competition in a purely competitive industry. *Harvard.*

BROTHER PATRICK J. HANCE, S.M., B.S. Dayton 1953; M.A. Catholic 1956. The history of accounting in the United States prior to 1925. *Catholic.*

KEITH HELLER, B.S. Lewis & Clark 1952; M.S. Oregon 1954. Development of accounting in the United States. *Minnesota.*

ROBERT M. JENNINGS, B.S. Louisville 1949; M.B.A. 1952. George S. Olive and Company—a business history. *Indiana.*

HOWARD E. JONES, JR., B.S. Alabama 1950; M.S.C. 1958. Earned surplus and earned surplus reserves. *Alabama.*

WALTER H. KRAMER, B.S. DePaul 1947; M.B.A. Oregon 1956. The agency in the marketing of air travel. *Indiana.*

PAUL G. LAGRONE, B.S. Bowling Green 1947; M.B.A. Denver 1948. Financial and accounting problems in the expansion of the natural gas industry. *Alabama.*

HAROLD B. LEGRANDE, M.B.A. Chicago 1948. An empirical analysis of the contribution of marketing surveys in providing information for management on consumer attitudes. *Chicago.*

ROBERT W. LITTLE, B.S. Indiana 1953; M.B.A. 1956. Selected major trends in wholesaling. *Indiana.*

WALLACE G. LONERGAN, B.A. College of Idaho 1950; M.B.A. Chicago 1955. Personnel administration at the executive level—a forecast of manpower needs in Swift and Company. *Chicago.*

CHARLES F. LOUIE, B.S. California 1954; M.B.A. 1955. Accounting for pension costs. *California (Berkeley).*

RAYFORD J. McLAURIN, B.S. Bowling Green 1948; M.B.A. Alabama 1951. The role of the accountant in anti-monopoly cases since World War II. *Alabama.*

DONALD MILLS, B.S. Alabama 1942; LL.B. West Virginia 1950. The analysis of recent trends in the presentation of net worth. *Alabama.*

WALTER G. MITCHELL, B.S. U. S. Military Academy 1943; M.B.A. Columbia 1953. The management of "fair trade." *Columbia.*

EDWARD J. MORRISON, B.S. West Virginia 1952; M.B.A. Indiana 1955. Applying management fundamentals to life insurance agencies. *Indiana.*

MELVIN S. MOYER, B.Comm. Toronto 1953; M.Comm 1956. Specification buying: a form of vertical integration. *Columbia.*

CHARLES S. NAGY, B.S. Indiana State 1947; M.S. 1949. An investigation of operations research and some of its effects upon accounting. *Alabama.*

DAVID W. ORTLIEB, B.S. Lawrence 1955; M.B.A. Indiana 1957. The relation of management philosophy to the corporate personality of a pharmaceutical company. *Indiana.*

PARLEY M. PRATT, B.A. Utah 1950; M.B.A. Harvard 1954. Marketing of rice. *Ohio State.*

JOSEPH C. SCHABACKER, B.S. Temple 1943; M.B.A. California (Los Angeles) 1952. Cash planning in the small manufacturing firm. *California (Los Angeles).*

RAYMOND H. SCOTT, B.A. Montana State 1938; M.B.A. Washington 1953. Technological and operational changes in general line grocery wholesaling. *Ohio State.*

LLOYD V. SEAWELL, B.B.A. Wake Forest 1953; M.B.A. Indiana 1954. An evaluation of selected annual reports of American industrial corporations for compliance with certain pronouncements of the American Institute of Certified Public Accountants. *Indiana.*

WADE P. SEWELL, B.S. Illinois 1947; M.S. 1949. A stochastic production function study of aircraft operation. *Chicago.*

RALPH E. SKELLEY, B.S. Missouri 1953; M.A. 1954. Comparative bank accounting practices in Missouri. *Alabama.*

ANDREW C. STEDRY, B.S. Carnegie Inst. Technology 1955; M.S. 1957. Budget control and cost behavior. *Carnegie Inst. Technology.*

LOUIS L. STERN, B.S.B.A. Marquette 1952; M.B.A. Northwestern 1955. The marketing of butter: a critical appraisal. *Northwestern.*

SERGIO TALACCHI, M.A. Michigan State 1956. Differential analysis of institutional processes and patterns. *Chicago.*

HILDA C. WASSON, B.S. Bowling Green 1940; M.B.A. Indiana 1953. Role of the markdown in retail pricing policies. *Indiana.*

JAMES H. WOLTER, B.S. Illinois 1949; M.B.A. Indiana 1955. An analysis of the process of new product idea evaluation by selected consumer goods manufacturers. *Indiana.*

Industrial Organization; Government and Business; Industry Studies

Degrees Conferred

WILLIAM L. BALDWIN, Ph.D. Princeton 1958. Changing concepts of the large firm and antitrust enforcement.

ROBERT A. BATTIS, Ph.D. New York 1958. Entrepreneurship, technological innovation, and economic change in the iron industry: a case study.

ABRAHAM S. BECKER, Ph.D. Columbia 1958. Economics of the cotton textile industry of the USSR.

ROBERT G. DEDERICK, Ph.D. Harvard 1958. The economics of Greater Boston's Metropolitan Transit Authority.

JOHN DOUTT, Ph.D. Pittsburgh 1957. Economic factors in the development of the paint and varnish industry with special attention to their marketing implications.

HUBERT C. EDGEWORTH, Ph.D. Alabama 1957. Development of national policy of financial aid to the American Merchant Marine.

NYLEN W. EDWARDS, D.B.A. Indiana 1957. The urban traffic congestion and a solution: with special reference to mass transit.

JOHN L. ENOS, Ph.D. Mass. Inst. Technology 1958. History of cracking in the petroleum refining industry: the economics of a changing technology.

RICHARD N. FARMER, Ph.D. California (Berkeley) 1957. Maritime operating differential subsidies.

JACK GUTTENTAG, Ph.D. Columbia 1958. Some studies of the post-war residential construction and mortgage markets.

R. HOLLBACH, Ph.D. McGill 1958. The Canadian primary aluminum industry.

LAWRENCE J. KAPLAN, Ph.D. Columbia 1958. Factors affecting productivity in the homebuilding industry.

BERNARD A. KEMP, Ph.D. Vanderbilt 1957. The merger component in the growth of a firm.

RALPH L. KOORENNY, Ph.D. Colorado 1957. Strategic sectors of the steel industry in the western U. S. in the light of location theory and the complex of industry concept.

T. E. KUHN, Ph.D. McGill 1957. The economics of road transport.

LEONARD W. MARTIN, Ph.D. Columbia 1957. Integration between manufacturing and retailing in shoes.

W. DAVID MAXWELL, Ph.D. Johns Hopkins 1958. Product rate discrimination or value-of-service pricing in motor trucking.

ROBERT S. M. NIELSEN, Ph.D. California (Berkeley) 1958. Ownership of tankers: the independents versus the integrated oil companies.

CHIU-HOCK QUAN, Ph.D. Colorado 1957. The economics of natural and synthetic rubber.

EDWARD P. REAGEN, Ph.D. Indiana 1958. The ceramic dinnerware industry in the U. S.

MIGUEL A. REGUERO, Ph.D. New York 1958. An economic study of the military airframe industry.

JACK C. ROTHWELL, Ph.D. Texas 1958. The conservation program of the Railroad Commission and the structure of crude oil prices in Texas.

ERIC SCHENKER, Ph.D. Florida 1957. A port authority for the State of Florida.

WILLIAM SCHUSLER, Ph.D. Pittsburgh 1958. Railroad commuter service in the Pittsburgh area: its history, present and future.

BERNARD SCHULL, Ph.D. Wisconsin 1957. Price discrimination in the corn products confectionery industrial pattern.

MILES H. SONSTEGAARD, Ph.D. Oregon 1958. Location-size theory for multiple-source, nonclustered industries.

DONALD STREET, Ph.D. Columbia 1958. Railroad equipment financing.

HARRY M. TREBING, Ph.D. Wisconsin 1958. Project-evaluation techniques for federal multiple purpose projects.

OTHEL DEV. TURNER, Ph.D. Texas 1958. Industrial location factors in Wyoming: A functional analysis.

ROBERT B. WILLIAMSON, Ph.D. American 1958. Electric retail rate increases and farm uses of electricity.

JOSEPH ZAREMBA, Ph.D. Harvard 1958. Some problems in forecasting future wood requirements.

Theses in Preparation

WILLIAM R. BEATON, B.S. Stetson 1950; M.S. Florida State 1952. An analysis of the philosophies prompting the establishment of governmental insurance programs, with particular emphasis on the atomic risk. *Ohio State.*

ELEANOR CRAIG, B.A. North Carolina 1947; M.A. 1950. A recent history of the North Carolina furniture industry, with special attention to locational factors. *Duke.*

JOHN R. DAVIDSON, B.S. Bowling Green 1933; M.S. New York 1940. Effect of the Robinson-Patman Act upon cooperative advertising. *Ohio State.*

CHARLES DEAKTOR, B.S. Pittsburgh 1951; M.L. 1952. The economic impact of the frozen food industry. *Pittsburgh.*

BETTY DERAN, B.A. Fresno State 1954. Western mobile home industry. *California (Berkeley).*

FRED DZIADEK, B.A. Columbia 1955. The determination of U. S. postal rates. *Johns Hopkins.*

CHARLES E. EDWARDS, B.S. Georgia Inst. Technology 1952; M.S. 1953. A study of the financial aspects of the recent merger movement among the small manufacturers in the automobile industry. *North Carolina.*

DAVID K. EITEMEN, B.B.A. Michigan 1952; M.A. California (Berkeley) 1956. Financial effect upon public utility companies of the type of rate base adopted by state regulatory commissions. *Northwestern.*

ORVILLE ELLIOTT, B.S. Oklahoma A. & M. 1955 M.S. 1956. An analysis of the production and distribution of electric power by the Grand River Dam Authority. *Oklahoma.*

EVELYN L. ENCHES, B.S. Minnesota 1923; M.A. Southern California 1928. Consumer protection in Great Britain. *California (Los Angeles).*

LIONEL EPSTEIN, B.S. New York 1953; M.A. Harvard 1956. Military procurement and its effect on the manufacturing industries of Connecticut and Massachusetts. *Harvard.*

DANIEL O. FLETCHER, B.A. Oberlin 1952; M.A. Michigan 1956. Economics of domestic Great Lakes water transportation. *Michigan.*

HELMUT J. FRANK, B.S. Columbia 1948; M.A. 1950. The pricing of Middle East crude oil. *Columbia.*

JOSEPH GOLDMAN, B.A. Brooklyn College 1947; M.A. Columbia 1950. Economic and social factors in the history of the southern iron and steel industry, 1850-1914. *Columbia.*

IRVIN GROSSACK, B.A. City (New York) 1956. A measurement of changes in industrial structure overtime. *Columbia.*

HENRY GROSSMAN, The problem of using reasonable market price in the federal regulation of natural gas production. *Georgetown.*

FRANK L. HENDRIX, B.S. Tennessee 1949; M.S. 1950. A study of monopoly power in the newsprint industry. *North Carolina.*

CHARLES F. HEYE, B.B.A. Texas 1943; M.B.A. Maryland 1947. Public utility regulation in the State of Maryland. *Pennsylvania.*

WILLIAM R. HUGHES, B.A. Maryland 1953; M.A. Harvard 1957. Firm size in electric power industry. *Harvard.*

HAROLD KATZ, B.A. Brandeis 1956. The effect of electronic data processing innovations on diseconomies of management and optimum firm size. *Columbia.*

ARTHUR J. KIRSCH, M.A. California (Los Angeles) 1954; B.S. California (Berkeley) 1952. The motion picture industry—an economic study of antitrust policy. *California (L.A.).*

DONALD W. LOEWECKE, B.A. Roosevelt 1954; M.A. Illinois 1955. The economic and financial implications of obsolescence. *Illinois.*

PAUL W. MACAVOY, B.A. Bates 1955. Price determination in natural gas industry. *Yale.*

DONALD A. MARKWALDER, B.S. Illinois State Normal 1952; M.A. Northwestern 1957. The flour milling industry—an economic study of excess capacity. *Northwestern.*

PETER MAX, B.A. Williams 1955. The regulation of the field price of natural gas. *Cornell.*

ALAN T. NICHOLS, B.A. Oklahoma 1952. Industrial concentration and census data. *Wisconsin.*

INGOLF H. E. OTTO, B.A. Cincinnati 1941; M.A. George Washington 1950. Public control of the insurance industry. *George Washington.*

EDWARD D. PETERSON, B.A. Iowa State 1951; M.B.A. Indiana 1953. Selected issues arising from government regulation of the public utility industries. *Indiana.*

PHILIP J. REINERTSEN, B.A. Dartmouth 1947; M.A. Chicago 1954. The pulp and paper industries in Sweden and Canada. *Chicago.*

ALLEN SELF, B.A. Texas A. & M. 1947; M.A. North Texas State 1949. Municipal electric utility systems in Oklahoma. *Oklahoma.*

NEIL SHIFFLER, B.S. Pennsylvania 1948; M.B.A. Michigan 1950. Distribution of milk under the Pennsylvania milk control law. *Pittsburgh.*

DOUGLAS J. STALLEY, B.Ec. Unv. Adelaide 1948; M.Ec. 1955. Monopoly and competition in Australia. *Columbia.*

ALBERT G. SWEETSER, B.A. Harvard 1937; M.B.A. New York 1942. Public warehouses in U.S.; their nature, regulation and legal position. *American.*

PERRY D. TEITELBAUM, B.S. City (New York) 1946; M.A. Columbia 1948. Nuclear energy and the U. S. fuel economy. *Chicago.*

MATTHEW VLAHAKIS, B.A. School of Economics, Athens, Greece 1949. Market behavior and governmental policy in the industries in Greece. *Columbia.*

HAROLD WEIN, B.S. City (New York) 1936; M.A. Columbia 1938. An analysis of economics and law of some recent merger cases in the steel industry. *Pittsburgh.*

RAYMOND WHITSON, B.A. Oklahoma 1952; M.A. 1957. Southwestern power administration and generation and transmission cooperatives. *Oklahoma.*

HENRY B. WILSON, B.S. Alabama 1949; M.S. 1951. The dynamics of industrial location. *Alabama.*

JAMES S. WORLEY, B.A. Vanderbilt 1949; M.A. 1950. Industrial research and the new competition. *Princeton.*

FRANK WRIGHT, B.S. Duquesne 1947; M.B.A. Pennsylvania 1949: The Pittsburgh railways: a valuation. *Pittsburgh.*

Land Economics; Agricultural Economics; Economic Geography; Housing

Degrees Conferred

BABU LAL AGRAWAL, Ph.D. Cornell 1958. A study of agricultural cooperatives in Western Uttar Pradesh (India) with special reference to agricultural credit.

MATTHEW AMAT, Ph.D. Columbia 1958. Economics of the forestry industry in Yugoslavia.

FRED B. ANDERSON, Ph.D. Florida 1958. An economic evaluation of custom harvesting of potatoes by packing plants.

HENRIK J. AUNE, Ph.D. Minnesota 1958. An economic analysis of labor inputs in dairying as affected by size of herd and types of equipment.

RAYMOND A. BAILEY, Ph.D. Ohio State 1957. Input-output data for the commercial swine enterprise in Ohio.

RICHARD BERE, Ph.D. Ohio State 1958. An economic analysis of the grading, packaging and marketing of apples, with special reference to pre-packaged apples.

CALVIN R. BERRY, Ph.D. Purdue 1957. An economic analysis of fertilizer marketing and pricing with particular reference to Indiana.

WALTER D. BOWLES, Ph.D. Columbia 1958. The economics of the Soviet logging industry.

GEORGE A. BRABB, Ph.D. Illinois 1958. The relation of prices and other factors to change in production of corn.

ROBERT K. BROWN, Ph.D. Pittsburgh 1958. The legislative background of public housing and its financial impact, especially on the Pittsburgh area.

VERE E. BUFTON, Ph.D. Wisconsin 1958. Wisconsin vegetables for commercial processing production: production areas and markets.

TZE-I. CHIANG, Ph.D. Florida 1958. Marketing Florida asparagus plumosus ferns.

SHIH AN CHEIN, Ph.D. Ohio State 1957. Frozen foods with special reference to consumer use in Columbus, Ohio.

GEORGE R. DAWSON, Ph.D. Cornell 1958. Economics of forage production and utilization, north country region. New York, 1955-56.

JOHN A. DAWSON, Ph.D. Chicago 1957. The demand for irrigation water in the Ainsworth area of Nebraska.

GERALD W. DEAN, Ph.D. Iowa (Ames) 1957. Supply functions for hogs.

MICHELE DE BENEDICTIS, Ph.D. Iowa (Ames) 1957. Intratemporal resource efficiencies in leasing systems—an application of linear programming.

EDDIE V. EASLEY, Ph.D. Iowa (Ames) 1957. An application of linear programming to the study of supply response in dairying.

ROBERT M. FISHER, Ph.D. Columbia 1958. Economic aspects of the federal low-rent public-housing program.

RONALD GATTY, Ph.D. Cornell 1957. A market development program for cut flowers.

ABBAS GHEZELBASH, Ph.D. Ohio State 1957. An econometric analysis of the greenhouse tomato market in Ohio.

CONRAD GISLASON, Ph.D. Chicago 1953. The storage of grains with special reference to international trade.

WILLIAM G. GRIGSBY, Ph.D. Columbia 1958. The residential real estate market in an area undergoing racial transition.

H. ZVI GRILICHES, Ph.D. Chicago 1957. Hybrid corn: an exploration in economics of technological change.

DONN K. HAGLUND, Ph.D. Pennsylvania 1958. Economic geography of the Godthab, Greenland district.

EDNA L. HEBARD, Ph.D. New York 1957. The changing character and extent of urbanization in the United States.

HOWARD L. HILL, Ph.D. Wisconsin 1957. An economic appraisal of adjustments in conventional leasing arrangements to facilitate capital accumulation.

EDWARD B. HOGAN, Ph.D. Wisconsin 1958. An analysis of the rate of adoption of a differentiated commodity form: a case study of chicken meat marketing.

GANIYU JAWANDO, Ph.D. Minnesota 1958. The role of agriculture in the economic development of Nigeria.

LINLEY E. JUERS, Ph.D. Minnesota 1957. An economic analysis of the operating costs of butter-powder plants with particular reference to the problems of joint costs.

JERZY F. KARCZ, Ph.D. Columbia 1958. Soviet agricultural marketings and prices, 1928-1954.

SHOHEI KAWAKATSU, Ph.D. Wisconsin 1957. Some methods of estimating fertilizer response functions for refinement of diminishing returns analysis.

ELMER R. KIEHL, Ph.D. Harvard 1958. Consumer evaluation of the product characteristics of beef.

SIDNEY KLEIN, Ph.D. Columbia 1957. The patterns of land tenure reform in East Asia after World War II.

ARVID C. KNUDTSON, Ph.D. Minnesota 1957. An analysis of processing costs in specialized butter plants receiving whole milk.

DONALD LELONG, Ph.D. Syracuse 1957. Factors affecting supply and demand in the New York milk market.

LAUREL D. LOFTSGARD, Ph.D. Iowa (Ames) 1958. Linear programming of dynamic plans for an actual farm and household.

STANLEY T. LOWRY, Ph.D. Louisana State 1958. Analysis of early theories on natural resources.

JOHN W. MAMER, Ph.D. California (Berkeley) 1958. The generation and proliferation of agricultural hand-labor saving techniques: a case study in sugar beets.

GENE McMURTRY, Ph.D. Purdue 1957. The response of farmers to various soil bank proposals in selected areas of the corn belt.

CHAIM MENDELSOHN, Ph.D. California (Berkeley) 1957. International trade in oranges; competition for export markets.

JOHN A. MOLLETT, Ph.D. California (Berkeley) 1958. Britain's agricultural dilemma: a study of farm policies and programs, 1920-1957.

DONALD S. MOORE, Ph.D. Minnesota 1957. A study of the effect of individual motivations and of farm business-household relationship upon the organization and operation of 29 southeastern Minnesota farms, 1928-55.

HUGH L. MOORE, Ph.D. Wisconsin 1958. Adjustments to bulk procurement in federal order pricing in Chicago.

LEO J. MORAN, Ph.D. Wisconsin 1957. Opportunities for resource development of rural Wisconsin.

UMAR KHAN NIAZI, Ph.D. Ohio State 1957. Capital for regeneration of agriculture in Pakistan.

HEUNG KEUN OH, Ph.D. Ohio State 1957. An analysis of factors influencing soft red winter wheat production.

GOGULA PARTHASARATHY, Ph.D. Wisconsin 1958. Underemployment and the Indian agriculturist.

ROBERT H. PERSONS, JR., Ph.D. Columbia 1957. Agricultural land use in the south plains of Texas, 1890-1950.

LEE E. PRESTON, JR., Ph.D. Harvard 1958. An economic analysis of exploration for nonferrous metals.

EDWARD F. RENSHAW, Ph.D. Chicago 1953. An economic appraisal of public investment in irrigation.

WILLIAM RICHIE, Ph.D. Ohio State 1958. History and development of agricultural cooperatives in Ohio.

HOWARD F. ROBINSON, Ph.D. Ohio State 1957. History and evaluation of trading in futures in potatoes, 1930-1956.

RAYMOND W. ROBINSON, Ph.D. Wisconsin 1957. Costs and efficiency of wholesale milk distribution in Milwaukee with particular reference to problems of wholesale pricing.

LOY L. SAMMET, Ph.D. California (Berkeley) 1958. Economic and engineering factors in agricultural processing plant design.

FRED B. SAUNDERS, Ph.D. Iowa (Ames) 1958. Farm and nonfarm adjustment opportunities for specified resource situations for families on small owner-operated farms, Piedmont area, Georgia.

ADNAN S. SHUMAN, Ph.D. Ohio State 1957. A suggested plan for the Syrian agricultural cooperatives based upon a study of selected agricultural cooperatives in Ohio and in the United States generally.

WESLEY G. SMITH, Ph.D. Iowa (Ames) 1958. Dynamic linear programming of conservation alternatives, including household consumption.

JAMES F. TUCKER, JR., Ph.D. Pennsylvania 1957. British agriculture under protection and free trade, 1815-1895.

VIR VIRANJAN SINGH TYAGI, Ph.D. American 1958. Economic impact of partition on Indian agriculture.

LALGUDI SIVASUBRAMANIAN VENKATARAMANAN, Ph.D. Chicago 1958. A statistical study of Indian jute production and marketing with special reference to foreign demand.

ARTHUR J. WALRATH, Ph.D. Wisconsin 1957. Impacts of the expanding urban-rural economy in southeastern Wisconsin.

GARLAND P. WOOD, Ph.D. Wisconsin 1958. An economic analysis of range reseeding in northern Nevada.

GEORGE M. WOODWARD, Ph.D. North Carolina 1958. The economics of commercial fishing with special application to North Carolina.

WILLIAM D. WUBBEN, Ph.D. Claremont 1958. The economic aspects of the California liquefied petroleum gas industry.

RICHARD B. ZOLLER, Ph.D. Minnesota 1958. The vertical-block budgeting system—a new farm planning technique.

Theses in Preparation

GEORGE L. ALMOND, B.S. Ohio State 1951; M.A. 1955. Warehousing of food products. *Ohio State.*

RAYMOND L. ANDERSON, B.S. Minnesota 1951; M.S. 1954. Development of public recreational use of private land in Wisconsin. *Wisconsin.*

WALLACE BARR, JR., B.S. Ohio State 1943. Projection and analysis of long time economic trends in agricultural and related industries. *Ohio State.*

HAROLD D. BIRCKMAYER, B.A. Cornell 1952; M.B.A. 1956. The effect of incentives on petroleum exploration and production, with emphasis on percentage depletion. *Cornell.*

MELVIN BLASE, B.S. Missouri 1955; M.S. 1956. Erosion control in process in Western Iowa. *Iowa (Ames).*

MICHAEL F. BREWER, B.S. Yale 1953; M.S. Michigan 1955. Water pricing and allocation with particular reference to California irrigation districts. *California (Berkeley).*

MARTIN K. CHRISTIANSEN, B.S. Minnesota 1950; M.S. 1956. An analysis of costs of retail milk delivery in Minneapolis and St. Paul. *Minnesota.*

CLARENCE F. DAVAN, B.S. Purdue 1952; M.S. 1953. Effects of a beef price support program on producers, marketing agencies and consumers. *Purdue.*

CARLETON C. DENNIS, B.S. Michigan State 1955. Interregional competition in the frozen strawberry industry. *California (Berkeley).*

MAURICE DE YOUNG, B.C.S. Tulane 1956; M.A. 1957. Arborutculture in the Haitian economy. *Florida.*

WILLIS EICHBERGER, B.S. Nebraska 1940; M.A. 1954. Economic framework for analyzing land development in lower Mississippi Valley. *Iowa (Ames).*

R. VERN ELEFSON, B.S. Missouri 1953. The influence of landlords on major farm operating decisions. *Minnesota.*

JOSEPH R. EWERS, B.A. Bowling Green 1950; M.S. 1954. Possible housing policies of the federal government 1960-1970. *Indiana.*

SIDNEY P. FELDMAN, B.S. Indiana 1953; M.B.A. 1954. Demographic factors affecting the demand for housing 1960-1970. *Indiana.*

LONNIE L. FIELDER, B.S. Louisiana State 1951; M.S. 1953. Alternative parity formulas for cotton. *Iowa (Ames).*

WAYNE FULLER, B.S. Iowa (Ames) 1955; M.S. 1957. Alternative parity formulas for corn. *Iowa (Ames).*

WILLIAM E. GOBLE, B.S.A. Georgia 1949; M.S. Tennessee 1950. Variations in livestock receipts and shipments in the Sioux City terminal market. *Iowa (Ames).*

ABRAHAM GOLDBLATT, B.A. California 1947. Structure of rooming house markets in New York City. *Columbia.*

REGINALD K. HARLAN, B.S. Texas Tech. 1949; M.S. 1954. Possibilities and implications of vertical integration in corn production. *Ohio State.*

JOSEPH C. HEADLEY, B.S. Illinois 1952; M.S. 1955. The contribution of irrigation to regional differentials in technological change. *Purdue.*

JAMES F. HUDSON, B.S. Louisiana State 1942; M.S. 1945. Accuracy of pricing cottonseed for crushing purchases in Louisana. *Iowa (Ames).*

JACK D. JOHNSON, B.S.A. Georgia 1947. M.S.A. 1948. Cattle prices at auctions in the Appalachian area. *Iowa (Ames).*

PAUL R. JOHNSON, B.A. Oberlin 1950; M.S. North Carolina State 1953. Analysis of changes in corn yields. *Chicago.*

MONTE E. JUILLERAT, B.S. Purdue 1956. M.S. 1958. The pricing structure for soybeans at the county elevators and processors in Indiana. *Purdue.*

JOHN E. KADLEC, B.S. Purdue 1953; M.S. 1957. Alternative methods of estimating supply functions for milk in the Louisville milk shed. *Purdue.*

WILLIS G. KEARL, B.S. Utah State 1949; M.S. 1951. Economic use and integration of pasture for beef cattle production in California. *California (Berkeley).*

NORMAN LANDGREN, B.S. Nebraska 1951; M.A. 1954. Economics of watershed development. *Iowa (Ames).*

ROBERT E. LAUBIS, B.S. Ohio State 1949; M.S. 1956. Improving the financial management of agricultural marketing agencies in Ohio. *Ohio State.*

GREGOR LAZARCIK, M.Sc. Brno 1948; M.A., Q.D. Paris 1953. Production and productivity in Czechoslovak agriculture, 1946-1956. *Columbia.*

IRA S. LOWRY, B.A. Texas 1950; M.A. 1951. The spatial organization of cities. *California (Berkeley).*

RICHARD J. McCONNEN, B.S. Montana State 1952; M.S. 1954. Economic analysis of dry land range fertilization in California. *California (Berkeley).*

BERNARD MELTZER, B.C. City (New York) 1938; M.A. Pennsylvania 1948. Real estate prices in Philadelphia. *Pennsylvania.*

JAMES MILLER, B.S. Pittsburgh 1943; M.S. 1951. The economic effects of sewage treatment on residential construction. *Pittsburgh.*

A. MILTON MOORE, B.A. Queen's Univ. 1949. The pricing of Crown timber in Eastern Canada. *Chicago.*

CHARLES V. MOORE, B.S. Ohio State 1953; M.S. 1956. An analysis of farm accounting procedures in common usage and the development of a more effective system for the present-day commercial farm. *Ohio State.*

JOHN T. MOORE, B.A. Indiana 1949; M.A. 1956. Fuel and energy requirements for Arkansas industrial expansion. *Indiana.*

RAYMOND C. NICHOLSON, B.S. Manitoba 1950; M.S. 1957. Production adjustments (scale and supply response). *Purdue.*

JERRY H. PADGETT, B.S. Clemson 1954; M.S. 1954. Transportation patterns and marketing methods for Indiana milk. *Purdue.*

ROSS M. PARISH, B.S. Univ. of Sydney, 1951. Wheat production and the demand for land. *Chicago.*

GLEN R. PURNELL, B.S. Utah State 1952; M.S. Montana State 1953. Alternative parity formulas for dairy products. *Iowa (Ames).*

EMILIO U. QUINTANA, B.S. Univ of Philippines 1952; M.S. Cornell 1954. Land and labor utilization in selected areas of the Philippines. *Purdue.*

ROBERT H. REED, B.S. California 1949; M.S. 1953. Economic efficiency in multiple products freezing plants. *California (Berkeley).*

ALLEN B. RICHARDS, B.S.B.A. Northwestern 1951; M.S. Montana State 1955. Some effects of the federal grain programs on country elevators in Iowa. *Iowa (Ames).*

ROBERT B. SCHWART, B.S. Ohio State 1947; M.S. 1949. The relation of variations in education to the decision making of farmers. *Ohio State.*

EMILY A. SHIEDLER, B.S. California 1954; M.S. 1955. Application of programming methods in forest management decisions. *California (Berkeley)*.

RICHARD L. SIMMONS, B.S. Kansas State 1951; M.S. 1955. Optimum future adjustments in the milk processing industry. *California (Berkeley)*.

VIDYAPATI SINGH, B.Com. Calcutta 1947; M.A. 1950. Financing of road improvements for farm marketing. *Western Reserve*.

FRANCIS J. SMITH, JR., B.S. California Polytechnic 1948; M.S. Purdue 1952. The impact of technological changes on marketing of Salinas lettuce. *California (Berkeley)*.

NORBERT J. STEFANIAK, B.B.A. Wisconsin 1948; M.B.A. 1950. Locational characteristics of Milwaukee industrial plants and their relation to the land use patterns. *Wisconsin*

JOHN R. TEDFORD, B.S. Connecticut 1950; M.S. 1951. Long-run and short-run elasticities of demand for farm products. *Iowa (Ames)*.

JOSEPH VON AH, B.S. Swiss Federal Inst. Technology 1954; M.A. Nebraska 1957. The impact of industrialization and urbanization upon rural Wisconsin. *Wisconsin*.

LUTHER T. WALLACE, B.A. Harvard 1949; M.S. Oregon State 1955. Factors affecting industrial location in Southern Indiana. *Purdue*.

Labor Economics

Degrees Conferred

ROGER L. BOWLBY, Ph.D. Texas 1958. The statutory regulation of minimum wages in Great Britain.

ROBERT L. BUNTING, Ph.D. Chicago 1958. Employer concentration in local labor markets.

GWENDOLYN J. BYMERS, Ph.D. California (Los Angeles) 1958. A study of employment in women's and misses' outerwear manufacturing Los Angeles metropolitan area, 1946-1954.

JAMES F. CRAWFORD, Ph.D. Wisconsin 1957. Wage pattern following in the meat packing industry.

STANLEY K. CROOK, Ph.D. California (Berkeley) 1958. The right to work: an historical review.

JOSEPH R. DEMPSEY, S.J. Ph.D. Wisconsin 1958. The operation of the right-to-work laws.

BERNARD FEDER, Ph.D. New York 1957. The collective bargaining and the legislative policies of the United Mine Workers of America, 1933-1947.

SHELDON HABER, Ph.D. Johns Hopkins 1958. Trends in the share of females in the labor force.

W. LEE HANSEN, Ph.D. Johns Hopkins 1958. Life cycle earnings patterns and intra-occupational differences in earnings.

GEORGE W. HARDBECK, Ph.D. Illinois 1958. Union-management health and welfare programs since 1945.

JAMES L. LUNDY, Ph.D. Minnesota 1957. An analysis of work sampling in the study of management job activity.

IVORY L. LYONS, Ph.D. Harvard 1958. Competition between union and non-union mills in the hosiery industry.

STANLEY MILLER, Ph.D. Wisconsin 1957. The United Mine Workers of America: a study of how trade union policy relates to technological change.

JAMES R. MORRIS, Ph.D. Chicago 1957. Job rotation: a case study.

ERNEST J. MOSBAEK, Ph.D. Iowa (Ames) 1957. Fitting a static supply and demand function for labor.

IFTIKHAR AHMAD MUKHTAR, Ph.D. Columbia 1958. Industrial labor conditions and legislation in Pakistan.

PHILLIP J. NELSON, Ph.D. Columbia 1957. A study in the geographic mobility of labor.

ROBERT S. POLKINGHORN, Ph.D. Chicago 1958. Regional wage differentials.

QUENTIN D. PONDER, Ph.D. Columbia 1958. The supervisory practices of high and low rated foremen.

ELTON RAYACK, Ph.D. Chicago 1957. The measurement of the effect of the Amalgamated Clothing Workers of America on the wages paid in the men's clothing industry.

JACK W. SKEELS, Ph.D. Wisconsin 1957. The development of political stability within the Auto Workers Union.

HARRY F. STARK, Ph.D. Rutgers 1958. Trade union administration: theory and practice. A case study of the Amalgamated Food and Allied Workers Union, Local 56, AFL-CIO.

RAMNARIAN TARNEJA, Ph.D. Cornell 1957. Profit sharing and the problems of technological change—a case study of the workload change in a textile mill.

LEO TROY, Ph.D. Columbia 1958. The course of independent unionism.

JOHN VANDENBERG, Ph.D. Michigan 1958. Some economic aspects of guaranteed wages and supplemental unemployment benefit plans.

RICHARD J. WARD, Ph.D. Michigan 1958. The role of Association of Catholic Trade Unionists in the American labor movement.

ARNOLD R. WEBER, Ph.D. Mass. Inst. Technology 1958. The International Chemical Workers Union: a study in structural adjustment.

HERBERT E. WEINER, Ph.D. Columbia 1957. British trade unionism and nationalization, 1868 to 1945; the evolution of the nationalization policies of the British Trades Union Congress.

Theses in Preparation

WAYNE G. ANDERSON, B.A. Tulane 1944; M.A. Iowa 1948. A study of the Federal Mediation and Conciliation Service as an independent agency. *Iowa.*

SALVATORE BELLA, B.S. Boston Univ. 1947; M.A. 1948. A pattern and structure of collective bargaining in the electrical manufacturing industry. *Cornell.*

GENE S. BOOKER, B.S. Ball State 1952; M.A.T. Indiana 1955. Labor market experience of a sample of unemployment benefit exhaustees. *Indiana.*

WILLIAM G. BOWEN, B.A. Denison 1955. Wage determination and the price level: a theoretical analysis. *Princeton.*

JAMES D. BROWN, JR., B.A. Union 1955; M.A. Wisconsin 1957. Value-added productivity and interindustry wage shifts. *Wisconsin.*

JOHN H. G. CRISPO, B.Com. Toronto 1956. Collective bargaining in public enterprises: a comparative study of Ontario Hydro and TVA. *Mass. Inst. Technology.*

ELBERT T. EGGERS, B.S. Bowling Green 1948; M.S. Colorado 1949. Policies and practices of hiring and placing the physically handicapped in selected industrial firms in metropolitan Atlanta. *Indiana.*

LEO A. ELISON, B.A. Wisconsin 1948; M.A. Columbia 1951. Productivity of labor in the coal industry of the USSR. *Columbia.*

IBRAHIM EL-SHERBINI, B.S. Univ. of Ibrahim, Cairo 1932; M.S. Wisconsin 1955. Problems of staffing and professional development of faculty in technical institutes and community colleges. *Cornell.*

FRED HARTENSTEIN, B.A. Pittsburgh 1946; M.A. 1948. Federal policy with reference to secondary boycotts. *Pittsburgh.*

EMANUEL HILDES, M.A. New School for Social Research 1953. Purpose, structure, and organization of the Association of Catholic Trade Unionists. *New School for Social Research.*

JOHN D. HOLMES, B.A. West Virginia State 1950. Unions and technological change. *Illinois.*

PAUL F. HUDDLESTON, B.A. Indiana 1942; M.B.A. 1949. The influence of military government on the development of German trade unions. *Alabama.*

ETHEL B. JONES, B.A. Vassar 1952; M.A. Chicago 1954. Hours of work and the demand for leisure: 1900 to 1957. *Chicago.*

ROBERT R. KESELL, B.A. Indiana 1953. Role of the international union representative in industrial relations. *Mass. Inst. Technology.*

JOHN J. KORBEL, B.S., Harvard 1939; M.B.A. 1941; M.A. 1955. Determinants of labor force participation. *Harvard.*

ARANKA KOVACS, B.A. McMaster 1955; M.A. Toronto 1956. The evolution of wage theories in relation to British economic development in the 19th century. *Bryn Mawr.*

WILLIAM J. LEE, S.S., B.A. St. Mary's Seminary 1943; M.A. Catholic 1947. Right-to-work laws—economic and ethical aspects. *Catholic.*

HAZEL McCALLEY, B.A. Pennsylvania 1943; M.A. 1944. Collective bargaining problems of professional and technical employees in non-profit institutions. *Pennsylvania.*

HARRY A. McGUFF, B.S. Indiana Central 1952; M.B.A. Indiana 1954. Factors influencing success of graduates of liberal arts colleges who are employed in business. *Indiana.*

HIKMAT NABULSI, M.A. Missouri. The labor movement in Syria. *Georgetown.*

BARBARA W. NEWELL, B.A. Vassar 1951; M.A. Wisconsin 1954. A history of the Chicago labor movement in the nineteen-thirties. *Wisconsin.*

LIGUORI A. O'CONNELL, B.A. Notre Dame 1949; M.S. Loyola 1953. Migratory labor in the State of Wisconsin. *Wisconsin.*

J. ROY OTT, JR., B.S. Hendrix 1947; M.A. Vanderbilt 1950. Employment trends in the textile industry, 1947-1956. *Vanderbilt.*

THOMAS J. REYNOLDS, B.A. Swarthmore 1933; M.A. Columbia 1935. Growth and structural change of manufacturing employment in New Jersey. *Columbia.*

HENRY N. SANBORN, B.A. George Washington 1949; M.A. Chicago 1951. Earnings differences between men and women. *Chicago.*

CHARLES P. SAWAYA, B.A. Michigan State 1955; M.A. 1956. The employment effect of minimum wage regulation: the case of southern pine. *Indiana.*

JOSEPH SCHUSTER. Union management cooperation in job-skill training programs in the contract construction industry in the United States, 1932-57. *Georgetown.*

KENNETH T. STRAND, B.A. Washington 1953; M.S. Wisconsin 1956. Jurisdictional disputes in the construction industry. *Wisconsin.*

JOSEPH STRING, JR., B.A. California (Santa Barbara) 1950; M.A. California (Los Angeles) 1953. An analysis of the shortage of mathematics teachers in the public school system. *California (Los Angeles).*

S. HERBERT UNTERBERGER, B.S. Pennsylvania 1934; M.A. 1935. An assessment of wage incentive progress. *Pennsylvania.*

RHEA H. WEST, JR., B.S. Tennessee 1947. Jurisdictional labor disputes in the atomic energy industry; a case study of the Oak Ridge operations. *Alabama.*

RUDOLPH A. WHITE, B.S. Alabama 1951; M.S. 1953. A comparative analysis of manpower mobilization in England and the United States during World War II. *Alabama.*

J. EARL WILLIAMS, B.A. Carson-Newman 1949; M.A. Tennessee 1950. Labor relations in the telephone industry. *Wisconsin.*

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THE AMERICAN ECONOMIC REVIEW

| Volume XLVIII | DECEMBER 1958 | Number 5 |

ARTICLES

COMMUNICATIONS

BOOK REVIEWS

OTHER DEPARTMENTS

ACTIVITY ANALYSIS IN ONE LESSON

By WILLIAM J. BAUMOL*

Unfortunately it is not one easy lesson. Although this paper is addressed to the nonmathematical reader and the neophite mathematical economist, they will not find it light reading. The following survey of activity analysis is designed to offer to the reader an intuitive grasp of the meaning, the derivation, the implication for economics, and a sample of the applications of some of the most important theorems in this area of research.[1]

This article deals with three major areas which are frequently confused by nonmathematical economists: mathematical programming, input-output analysis and what may, for want of a better designation, be called activity analysis proper.[2]

I. Programming

Programming, both linear and nonlinear, is entirely a mathematical technique. Its economic content is therefore nil. This is no mere classificatory quibble. It means that programming per se can never tell us anything about the operation of any part of the economy. Like the calculus or any other branch of mathematics it can only help us to find the implications of the economic information which we already have or are willing to assume. To the extent then that economists were responsible

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[1] To the reader who wishes to go further into any of these materials I unhesitatingly recommend the superb Dorfman, Samuelson and Solow volume, *Linear Programming and Economic Analysis* [4]. I will give almost no other references because I believe readers cannot do better than to begin with this book. The volume also contains an annotated bibliography.

Another useful reference for the beginner is R. G. D. Allen's *Mathematical Economics* [1]. This volume which incidentally is *not* a new edition of Allen's *Mathematical Analysis for Economists*, is another fine example of the author's expository ability. However, it does not go as deeply into the basic theorems of activity analysis as do Dorfman, Samuelson and Solow; and several of the areas outlined in this article are not discussed by Allen.

[2] These areas though different, are not unrelated. Activity analysis, as we shall see, employs the techniques of mathematical programming, and even input-output can be interpreted as a very simple sort of linear program.

for the development of programming,[3] they may be said to have been productive in areas outside their own fields, as they have been in the past when they formulated the essentially technological law of diminishing returns, or when, by inventing the marginal analysis, they stumbled, a few centuries too late, on what is essentially a crude version of the differential calculus.

\Programming is, as we shall see, essentially suited to the analysis of rational behavior. It has, therefore, like the marginal analysis, been somewhat less successful in describing what is than in indicating what (given some preassigned goals) ought to be. Some of the most fertile applications of programming have involved welfare economics and advice to businessmen, both of which aim to tell the relevant persons how they can most efficiently go about working toward their objectives. Let us indicate briefly a few of the business problems to which programming is most frequently applied.[4]\

1. *Optimum product lines and production processes.* When operating at a high output level a firm is likely to run into a variety of capacity limitations. Its factory size, the amount of time available on different machines, its warehouse space and its skilled personnel may, any or all, constitute bottlenecks, some of which it is prohibitively expensive or even impossible to eliminate in the short run.

A crucial characteristic of such a situation is that the production of a relatively unprofitable item or the use of a production process which makes liberal use of the scarce facilities may take up valuable capacity that can better be used in more economical processes and in the manufacture of more lucrative commodities.

There is no simple solution, such as complete specialization in the one "most efficient process" for producing the one item which makes "most profitable" use of scarce facilities since, except by pure accident, there may be no process or no product which is economical in its use of all of the firm's limited facilities at once. One item may make good use of machine capacity and may therefore yield the highest profit per scarce machine-hour, whereas another may make more effective use of limited warehouse space. Production of only the former would find warehouses completely loaded before machine time was fully employed,

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[3] Several economists have made important contributions. Notable among these are T. C. Koopmans, R. Dorfman and W. W. Cooper. But if any one person is to be named as the father of programming we must undoubtedly award the honor to mathematician George Dantzig, inventor of the first successful (and still one of the most efficient) computational techniques, the simplex method. Important later contributions have been made by mathematicians such as H. W. Kuhn, A. W. Tucker, A. Charnes, etc. For a bibliography see [4, pp. 507-11].

[4] Much of the remainder of this section first appeared in a publication of Alderson Associates, Inc. Philadelphia, reproduced here with their permission [14, pp. 1-4].

while the latter product, since it is not bulky, might leave warehouses half empty even if the firm's machines were to turn out nothing else.

2. *Transportation routing*. In the selection of transportation routes, especially where a firm has many plants and its processes involve trans-shipment of items in various stages of production, substantial savings can be expected from careful planning of commodity movements. If the firm employs its own trucks or other transport facilities, the problem is to route them in a way that incurs as little cost as possible. Where the firm employs others to do its transporting, the computations may be further complicated by peculiarities in the transportation rate structure, for then the firm's objective is not to minimize ton-miles but to minimize payments to the carrier, and the two do not always correspond.

3. *Meeting product specifications*. Many contracts include a number of minimum specifications which must be met by the product, and sometimes the manufacturer will set up such standards for himself. Usually there is a variety of ways in which these specifications can be met. For example, an animal feed may require X units of protein per bag, Y of carbohydrates, Z of vitamin B, etc. Each of the grains combined in the animal feed contains some of the nutrients, and it is therefore possible to make a bag of feed meet these specifications in many different ways. A very inexpensive ingredient may contain much starch and very little else and to meet the standards it may be necessary to add some more expensive ingredients. But which ingredients should be added and in what proportions? Or will it prove cheaper to begin with somewhat more costly ingredients which supply a better balance of all the nutrients?

The least-cost combination of meeting specifications is basically a programming problem. This technique has, for example, been employed in the blending of gasolines. Programming techniques have been employed in many other business problems. They can help determine optimum inventory levels and have been used to solve production problems, like cutting of paper and cloth in a way which minimizes raw material waste, and in the job assignment of specialized personnel.

A. *The Analysis*

What is the common element in all of these situations which makes them amenable to programming analysis? It is clear that all of them require a search for "best" values of the variables. But there is something more involved which makes the usual tools of the calculus or marginal analysis inapplicable. In many problems of optimization there is a complication in that the outcome, to be acceptable, must meet certain conditions. For example, the problem of fencing in 20 square feet

at minimum cost involves the determination of that shape of plot which will save on fencing most effectively. But any saving which is achieved by fencing only 19 or 21 square feet will be unacceptable. This then is essentially a problem of finding the best way of meeting a very precise specification which the mathematician calls "a side-condition." So long as the specifications are so precise (the area must be 20 square feet, no more or less; or the starch content of a 100-pound bag of feed must be exactly so many calories, etc.) the optimization problem can usually[5] still be dealt with by calculus (marginal) techniques.

However, it is characteristic of many business problems that specifications are not precise but provide only minimum requirements that must be met. Or the specification, rather than stating the precise extent to which a facility will be used, may indicate only the maximum capacity which is available. Any output which overshoots the quality standards or does not fully utilize some part of capacity is not necessarily ruled out. Here the side-conditions are inequalities rather than equations. That is, they do not state that X must equal 500 but only that X must be no less than 500.[6]

[5] Even here there is an important exception. The mathematical form of the precise specification (side-condition) is an equation. If the graph of the equation is discontinuous or kinked, calculus methods cannot be depended on to work. This is because these techniques find an optimum by computing the slopes of the relevant graphs to investigate whether it is possible to go "uphill" (toward higher profits). Where the graph of a function is discontinuous or kinked, its slope is, for obvious reasons, not even defined.

[6] The reason marginal techniques break down in the presence of inequality side-conditions is easily illustrated with the aid of a simple graph. Marginal analysis finds, e.g., the point of maximum profits by locating the point at which marginal profit (the slope of the total profit curve) equals zero (output OQ in the figure). That is the meaning of the standard marginal-cost-equals-marginal-revenue condition. But suppose output is limited by the inequality that production cannot exceed OC. Then our problem is to find out whether the point of maximum attainable profit is OC or some point to its left (which is no easy problem in the N dimensional N variable case). But for this the marginal analysis is useless, for at the optimum point in the diagram, OC, the marginal criterion, marginal profit $=$ the slope of the total profit curve $=$ zero, is obviously invalid.
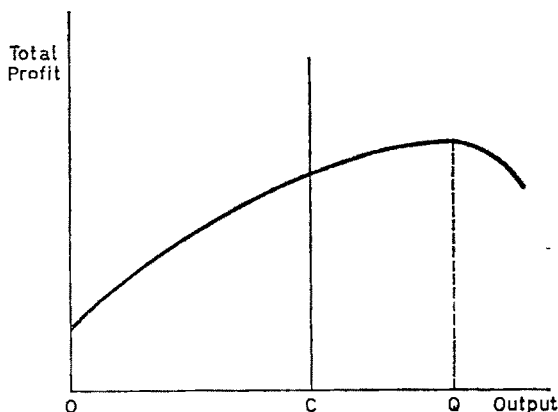
This sort of side-condition characterizes each of the business problems which has been described. In the optimum product line and production process problem there are maximum capacities to be dealt with. In meeting specifications at minimum cost, each specification is such an inequality. In the transportation routing and plant location problems the presence of such restrictions on the businessman's decisions is less obvious, but they are nevertheless there and play a fundamental role in the computation. There can be limitations on the size and cargo-carrying capacity of the trucks, trains, or ships to be routed. But the more relevant capacity limitation is a peculiar one which states that in no case is it possible to ship negative amounts from one place to another! This rather silly-sounding restriction is important partly because things like this are never obvious to an electronic computer and, unless it is specifically forbidden to do so, the computer will assign negative shipments from some supply sources to some destinations. For the machine will reason that if it is profitable to reduce some shipments to zero, it may be still more profitable to reduce these shipments even further!

For the economic theorist, such nonnegativity requirements are important for a far more fundamental reason. Like an electronic computer, marginal analysis is, by itself, incapable of taking account of them. To return to the more familiar optimum output problem, for the competitive firm we note that the rule of the marginal analysis is that the output of any item should be at a level at which marginal cost is equal to price. But for an unprofitable item marginal cost may only be equal to price at an impossible negative output level. That is, in a case of increasing costs, even at zero output cost need not have fallen back to the level of price. Of course, no moderately sane economist making a graphic analysis will ever recommend a negative output. But where a large number of interdependent decisions have to be made, the calculations may all have to be done with the help of mathematical reasoning. And a mathematical analysis, based on marginal equalities like marginal cost equals price, must in such a case yield nonsense results unless we impose on the calculation the explicit requirement that the variables be given no negative values. We will come across this point again later.

\ Programming, then, is the mathematical method for the analysis and computation of optimum decisions which do not violate the limitations imposed by inequality side-conditions./ In almost all cases the method of computation is a so-called "iterative procedure." Just as the term "ragout" disguises the fact that it is only stew, though presumably an elegant one, this fancy term is used to dignify a systematic trial-and-error procedure. The answer to a programming problem will ordinarily not be arrived at directly. Instead the solution is found by

groping toward it. But the trial-and-error procedure is not pure guess-work. It is systematic in that it usually involves at least the first two of the following features:

(1) There is a mechanical rule which determines, after each step, exactly what the next step is to be on the basis of the results of the trial just completed. One purpose of this feature of the method of solution is that it makes electronic computation possible. These mechanical brains unfortunately possess no judgment of their own and so they must be told what to do in every contingency. This is like teaching a human the rules of algebra before giving him an algebraic problem to solve. In any event, a mechanical rule stating what must be done at each succeeding trial in the trial-and-error procedure is useful because in a problem complicated by a great number of variables and inter-relationships, human judgment can go badly wrong and this can result in an inefficient, even a totally ineffective, search for the answer.

(2) A second feature which usually characterizes the systematic trial-and-error procedure is a proof that the method has been constructed in a way which guarantees that each trial will yield values which are closer than the last to the correct answer. This very important feature assures the computer that he is always getting closer to his result and is not wasting his time by going off in a wrong direction. We shall see later, in our discussion of the simplex method, how this sort of guarantee can be built into a computation procedure. Of course, such a guarantee can only be provided where there is a mechanical rule which specifies step by step what will be done. Otherwise successive steps are unpredictable and it is then not possible to say in advance whether they will be closer to or farther from the correct answer.

(3) For a large class of problems there are available trial-and-error procedure rules which are guaranteed to yield precisely the correct result after a finite number of steps. In other cases where this is not possible, one can hope to calculate a maximum error, and to be able to say, for example, that the result of the most recent trial is at most one-tenth of one per cent from the correct answer.

Where the problem is one involving *linear* programming there are several computational methods which yield a precise answer after a finite number of steps. The simplex method, the method of fictitious play and the complete description method are all linear programming computational techniques. In the next section we will see what is involved in the linearity of a programming problem.

B. *Algebra and Geometry*

First, let us set out the equations of a typical linear programming problem postponing comments on nonlinear programming to the geometric section which follows. Consider a firm which can produce any

of the four products w, x, y and z whose outputs are W, X, Y and Z and whose profits per unit of output are, respectively 5, 3, 2 and 7. Then the total profits of the firm are given by $5W + 3X + 2Y + 7Z$. (Here, e.g., W and X may represent outputs of the same product manufactured by different "processes," i.e., with the use of different input proportions.) Suppose, moreover, the firm has available only 50,000 square feet of warehouse space and 32,000 machine hours. If the manufacture of one unit of w requires 0.5 hours of machine time, that of x requires 2 hours of machine time, etc. we have an inequality relationship (which is called a "constraint" or "side-condition") such as:

$$0.5W + 2X + 1.9Y + 3.1Z \leq 32,000$$

which states that no more of these outputs can be manufactured than the available machine time permits. There is a similar warehouse-space constraint. We can see now that the programming problem can be written:

Maximize profits: $5W + 3X + 2Y + 7Z$,

Subject to the constraints:

$0.5W + 2X + 1.9Y + 3.1Z \leq 32,000$ (available machine time) and

$10W + 1.2X + 7Y + 4Z \leq 50,000$ (warehouse capacity) where $W \geq 0$, $X \geq 0$, $Y \geq 0$, $Z \geq 0$.

This is the standard form for a linear programming problem. It is called linear because the expression to be maximized and the inequalities only involve the variable multiplied by constants and added together (as in an equation for a straight line such as $y = 5x + 3$). There are no X^2's or $5 \sin Y$'s or $\log Z$'s or more complex expressions. We may note that this model employs an assumption of competitively or otherwise fixed input and output prices and constant returns to scale in production. This enters in two ways—from the information that profit per unit of w is 5 we can only compute the profit from producing 10 units of w at $5W = 50$ because the assumption of constant returns to scale and constant input prices implies that costs, revenues and profits will all rise precisely in proportion with the level of output. (We note again that w is *defined* in such a way that it must always be produced with the same input proportions—in this analysis a change in the amount of scarce machine time used per unit of output is described as a shift from the manufacture of w to that of x.) The linearity of the inequalities also rests on the assumption of constant returns to scale— the amount of warehouse space occupied by product y is assumed strictly proportionate to Y, the level of output of that item.[7]

[7] Where the facts of the situation do not warrant these assumptions even as an approximation, we may be forced to employ techniques of nonlinear programming. Usually these are, at best, more complicated. For many types of nonlinear program no method of solution is known.

Let us now look at some programming geometry. First, we must see how an inequality is represented graphically. Consider the inequality $2X + Y \leq 5$. (This may be interpreted, e.g., as a warehouse capacity limitation.) In Figure 1 any point such as P on the line LL' which represents the equation $2X + Y = 5$ (and represents full use of capacity) clearly satisfies the inequality. But, in addition, any point such as Q, R, or S which lies below and to the left of LL' also satisfies the
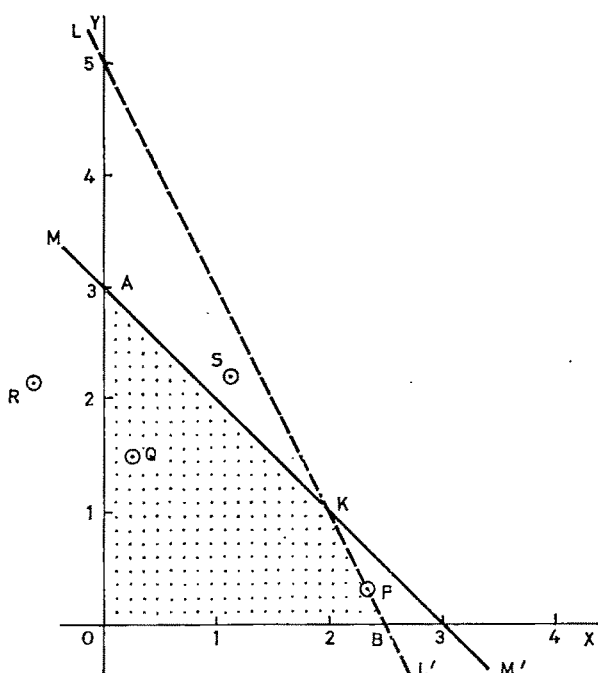
FIGURE 1

inequality because it involves values of X and Y smaller than those which completely use up the capacity. We note then that while a two-variable equation is represented by a line, a two-variable inequality is represented by a region. Indeed a linear two-variable inequality is represented by drawing a straight line which divides the plane into two regions called "half-spaces," one of which contains all points satisfying the inequality.

Let us see what happens if, in addition, the variables must satisfy a second inequality, say $Y + X \leq 3$ represented by the half-space to the left of line MM'. All of the points which satisfy both inequalities must lie below both lines MM' and LL', so that we are left with the

region bounded by the broken line MKL'.[8] Further inequalities may bound the region from all sides—e.g., the addition of the inequalities $X \geq 0$, $Y \geq 0$ leaves us with the shaded region in the diagram.[9]

We can now represent the entire programming problem (linear and nonlinear) by adding to the figure a set of isoprofit (indifference) curves (Figure 2).[10] Our objective is to pick the most profitable point among those representing technologically feasible output combinations, i.e., among all the points in region $OTKU$. This will be the point in the region which lies on whichever happens to be the most valuable
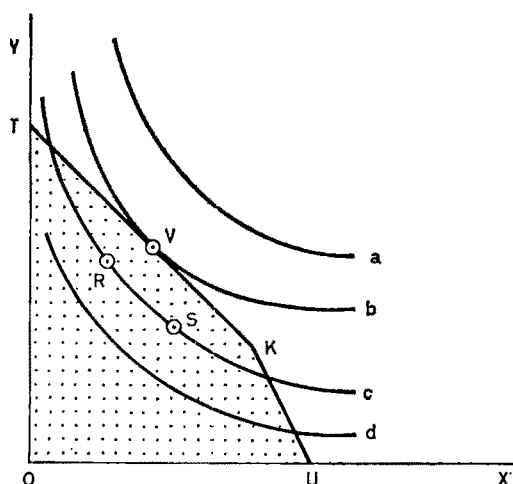
FIGURE 2

indifference curve. It may be the point of tangency V in Figure 2 but it need not be. Actually, because of increasing costs or diminishing revenues, indifference curve b may represent lower profits than does curve c even though b is higher and hence represents higher levels of physical output than does c. The points like R and S inside the feasible region $OTKU$ may be optimal.

But in *linear* programming such a situation can never arise. The

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[8] Note that while two equations in two variables will normally leave us with only one possible point (the intersection of the two curves), two or more inequalities will often still leave us an unlimited number of points to choose from. Thus, there is nothing necessarily wrong with, say, a system of 5 inequalities in 3 unknowns even though we usually prefer to have no more equations than unknowns.

[9] Nonlinear inequalities are represented similarly. Only instead of straight lines like LL' the graph will involve curves.

[10] It has been convenient here, for expository purposes, to begin with the nonlinear case since nonlinear indifference curves can assume the more familiar curved shape and can behave in the familiar manner (one-point tangents like point V).

profit equation in a two-variable linear program is of the form:
$$\text{Profit} = 3X + 2Y.$$
Thus the curve representing a $50,000 profit level, whose equation is:
$$50,000 = 3X + 2Y$$
will be a straight line whose slope is $-3/2$. Similarly the line representing a $70,000 profit level will be higher than and of the same slope as the $50,000 line. The indifference curves of a linear program will therefore be a series of parallel straight lines. Further, moving to higher and higher curves will either increase or always decrease profits.[11]

It follows that the optimal point of a *linear* program will always lie on the boundary of the feasible region. The logic is simple. Any commodity whose production is profitable will continue to be lucrative as its output expands because there will be neither diminishing returns to scale nor unfavorable effects on input and output prices. It will, therefore, always pay to expand production until some capacity limit is reached, i.e., until the boundary of the feasible region is attained.[12] In fact in a linear program, since the profit indifference curve is a straight line, an optimal point will always occur at one of the corners, O, T, K or U of the feasible region.[13]

This leads to a fundamental theorem of linear programming: Suppose there are, say, 5 inequalities (capacity limitations) or equations in a linear program (here we do not count inequalities like $X \geq 0$); then an optimal solution can be found which involves no more than 5 non-zero values of the variables, i.e., it will pay to produce no more than 5 products (or use no more than 5 production processes).[14] Moreover if this optimal solution, say, involves just three products, exactly three facilities will be used to capacity and the other two will have unused

[11] The latter will happen if both outputs are unprofitable. The reader can easily form in his mind a three-dimensional picture of the difference between a linear and a nonlinear programming problem. Visualize a graph with the values of two variables X and Y on the two bottom axes and with total profit recorded on the vertical axis. Then the programming problem is to find the point on the floor of the diagram which lies below the highest *feasible* point on the surface representing profit as a function of X and Y. The constraints of the problem limit the values of the variables to the feasible region (the shaded area in Figure 1). In nonlinear programming the profit surface can be curved or, indeed, of any shape. In the simplest case it will be a hill with no dents and with a single peak. But in a linear programming problem the profit surface *must be a plane* and to find the optimum point we go uphill along the plane until the constraints permit us to go no further.

[12] This also shows why an interior point like R or S (Figure 2) may be optimal in a nonlinear program—diminishing returns to increased specialization in any one output may make it unprofitable to use facilities fully.

[13] If the straight line indifference curves are parallel to one of the segments of the feasible region's boundary, say to TK, (Figure 2), then the entire segment *including corners T and K* will be optimal.

[14] Sometimes there will be other optimal solutions but they will offer no advantage over the so-called "basic" (5-product) solution. See the preceding footnote.

capacity, i.e., in an optimal arrangement there will be exactly as many facilities used to capacity as there are commodities produced.

This is really a rather surprising result. It states that a firm which has, say, 12 fixed factors and is producing 150 products, can maximize its profits by cutting its product line down to no more than 12 items! The result should properly be viewed with suspicion and it must be recognized to be a direct consequence of the linearity assumption. In practice a businessman must be very careful to make sure the assumption is valid before accepting this sort of radical advice!

We can get some idea of the rationale of the theorem from Figure 3
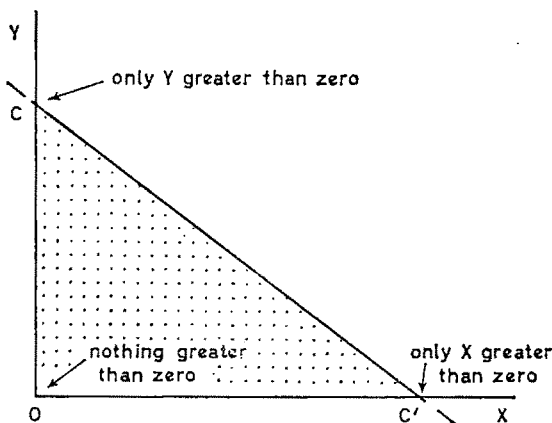
FIGURE 3

where we assume there is only one capacity restriction represented by line CC' and the region below and to the left of that line. At any point on CC' (including points C and C') our single limited facility (the warehouse?) is used right up to capacity. With the requirements $X \geq 0$, $Y \geq 0$ the feasible region is reduced to the triangle $OC'C$. We have just seen that an optimal point must occur at one of the corners O, C or C'. Consider each of these in turn. At either C or C' there is just one output (Y or X respectively) and one facility used to capacity, while at O there is no output and nothing used to capacity. Thus the theorem holds in this case. Only where there are two facilities of limited capacity can a corner optimal solution occur which involves nonzero outputs of both commodities.

For example, at point K in Figure 1 there are positive outputs of both X and Y (and both facilities are used to capacity—K lies on both LL' and MM').[15]

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[15] A glance at Figure 2 shows that the theorem does not hold in nonlinear programming. Point V which is not a corner of the feasible region may be the only optimal point. Here

But why should this be so—why should the optimal number of products equal the number of fully used facilities? Roughly the answer is that if only machine time is limited it will pay to produce the one item which yields the highest profit per machine hour. But if warehouse space too is really limited (if it is used to capacity) it will also be profitable to introduce a second item which makes good use of warehouse space, and so on.

The observation that some corner of the feasible region will always be optimal lies behind the simplex method[16] of computation, for this at once reduces the possibilities to a finite set. The method, whose conceptual simplicity is one of those fine achievements of the human intellect, can be roughly described as follows:

1. Find any corner of the feasible region (this is not as easy as it sounds—if the origin is a corner of the feasible region as in our diagrams it can save a lot of trouble to start there). Suppose then we start at O (Figure 1).

2. Compute the profits at point O and at the adjacent corners A and B.[17]

3. If one or both of the latter yield higher profits than does O, move to the corner which offers the greater profit per unit of output, say to B.

4. Now repeat steps 2 and 3, only substituting point B for point O. Thus compute profits at K and see if they are higher than those at B, if so move to K; otherwise stay at B. In this way, by successive elimination we must eventually find the optimal point.[18] Of course, this sounds much easier than it is in practice. Most of the work arises in just locating the (adjacent) corners of the feasible region.

More specifically, the simplex method (which, as was said before, is a trial-and-error iterative method) proceeds as follows: Suppose we begin with a trial solution involving outputs of, e.g., only items 2, 3 and 6. That is, we wish to test whether the firm can maximize its profits by specializing in the production of only these three commodi-

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both commodities are produced, but only the *one* facility whose capacity is shown by line TK is used to capacity. If an interior point like R is optimal because indifference curve c is more profitable than is b or any other such curve in the figure, then we have an optimal point with two commodities produced and *no* facility used to capacity.

[16] The name was not chosen to imply that the procedure is simple. Roughly, a simplex may be described as the N-dimensional analogue of a triangle. The outline of the computations, below, indicates that the method consists in successive investigation of adjacent corners of such a figure.

[17] This is not quite accurate. In practice, we only compute the change in profits resulting from a small move in the direction of A or B. A theorem states that *in a linear problem* if such a small move is profitable, so is the move all the way to A or B.

[18] We do not have to try every point for it can be shown that if no move to an *adjacent* corner increases profits we are at an optimum. If both O and K are less profitable than B it is not necessary to try A.

ties. We start with outputs of these items which are as large as the resource limitation inequalities permit.[19] To test whether we have chosen the best combination of commodities we must see whether it would be more profitable also to produce some of, say, commodity 4. Let the respective per unit profits of these commodities be P_2, P_3, P_4 and P_6. Then the introduction of one unit of item 4 will bring in P_4 dollars. But it will also involve an opportunity cost in that, because of resource limitations, in order to produce that unit of 4 we may have to reduce the outputs of the other commodities. Our inequalities permit a straightforward computation of the required reduction in the other outputs. In the simplex method we make this computation. Let $-\Delta X_2$, $-\Delta X_3$ and $-\Delta X_6$ represent the reductions in outputs 2, 3 and 6 necessitated by the introduction of a unit of output 4. Then the net gain, G_4 will be equal to P_4 minus the opportunity cost of the introduction of the unit of 4: $G_4 = P_4 - P_2\Delta X_2 - P_3\Delta X_3 - P_6\Delta X_6$. If G_4 is positive it will pay to introduce output 4.

But while the introduction of item 4 has been shown to be profitable, the introduction of some other item, e.g., item 1, may be even more remunerative. To test for this possibility we must compute G_1, the figure showing the net gain which would result from the introduction of a unit of item 1. Similarily we compute G_5 and every other possible G, the net gain figure corresponding to the introduction of any other relevant output. If, say, G_5 turns out to be the largest of these figures, the introduction of a unit of item 5 will make the maximum per unit addition to profit. And so the solution must be changed—the output of item 5 increased, and the outputs of 2, 3 and 6 reduced as required by resource limitations, until one of the latter (say output 3) falls to zero.[20] Resource limitations permit no further expansion of 5 since no negative outputs are possible. We now have another trial solution to our program, this one involving only outputs 2, 5 and 6, and we begin all over again on the computations just described.

We may note that each trial solution found in this way is necessarily more profitable than the last so that we must always be coming closer

[19] This can always be done by including some fictitious types of output whose sole function is to use up excess capacities. It is perhaps easiest to think of such an output as garbage removal—the disposal of excess quantities of resources. If, for example, in our numerical program above, we insert numerical values of W, X, Y and Z which do not necessarily use up all available machine time so that $0.5W + 2X + 1.9Y + 3.1Z \leq 32,000$ we can insert the fictitious variable $M \geq 0$ which represents the quantity of machine time "thrown away," and obtain $0.5W + 2X + 1.9Y + 3.1Z + M = 32,000$. M is called a slack variable.

[20] This is justified only in a linear programming computation where there are no diminishing returns. In such a case, if it is profitable to substitute *any* of output 5 for some of output 3, it will be profitable to make as large a substitution of this sort as is possible. With nonlinearity it would pay to expand 5 and contract 3, but only up to a point where diminishing returns make further substitution unprofitable.

to our optimal solution. Moreover, since there exists only a finite number of output combinations to be examined in this way (and since we know that the increasing profitability of successive trial solutions guarantees that we will never accidentally try the same solution twice) we must, after a finite number of steps, exhaust all the possibilities and arrive at the true optimum. We will know when we have arrived because at that step the introduction of no other item will add to profits, i.e., none of the G's will be positive.

Note incidentally the crucial role played by this net gain figure, G. At each step we know that it pays to go on if and only if at least one of the G's is positive. This is called the *simplex criterion*.

C. *The Dual Program*

To every linear program there is a closely related, sometimes somewhat artificial, minimization problem whose mathematical significance is very great. Before examining its formal structure let us see what economic interpretation has been given this second "dual" problem.

Let our original problem be that of chosing the most profitable combination of outputs under the limitations imposed by several scarce factors, and suppose an Austrian economist enters the management of this competitive firm and decides to see how the profits can be imputed to the scarce factors which constitute the bottlenecks to output expansion. That is to say he wishes to decide what proportion of its profits the firm owes to each such scarce factor. To do this he will set up accounting prices for these resources which are just high enough to give to these factors a value equal to the total profit of the firm.[21] That is, *the firm's profits after paying their imputed values to its scarce factors must be zero*. Moreover a similar condition must hold for each commodity produced by the firm—the value of the scarce resources tied up in its manufacture must not fall short of the product's profit yield. In programming terms, the Austrian economist wants to find prices for the scarce resources which minimize the total accounting cost of these resources to the firm, and yet involve a scarce-factor cost of producing a unit of each commodity which is no less than its unit profit yield.[22] The problem of finding such prices is the dual program to the profit maximization program.

[21] As economists we would expect the accounting prices of these resources to reflect their marginal value products—i.e., the increase in profits which would accrue to the firm if it could somehow obtain another unit of such a factor. In fact this turns out to be the case— the prices determined by the dual program do represent the opportunity values of the scarce factors.

[22] Some commodities which the firm can but does not wish to produce will, as we shall see, yield negative profits. That is, their gross profit must be less than their opportunity cost in terms of scarce factors tied up in their production.

It must be emphasized that the zero profit condition in this problem is not related directly to the zero profit requirement for long-run equilibrium under perfect competition. In imputation zero profit is an accounting requirement. If accounting prices are set up which do not completely exhaust profits, these prices do not impute profits completely to the scarce factors which were used to obtain the outputs. The accounting prices must then be increased to eliminate these unimputed profits.

The term "duality" refers to a very remarkable symmetry in the mathematical statement of the two problems. One involves maximization, the other minimization. The former involves outputs *no greater than* resources permit, the other cost *no lower than* the amounts necessary to allocate all profits. Suppose, moreover, the firm has two scarce resources and three possible outputs. The maximization problem has three variables, X_1, X_2 and X_3, the magnitudes of these outputs. The dual problem has the same number (three) of inequalities stating that the profits, P_1, P_2 and P_3 of each of these outputs must be imputed fully to the firm's scarce resources. Similarly, the minimization problem has two variables, Y_1 and Y_2, the accounting prices of the scarce resources, and the maximization problem correspondingly has two inequalities stating that resource capacities, C_1 and C_2 must not be exceeded.

But the symmetry of the two problems is even more remarkable, as the algebraic statement of the two problems will show:

Primal Problem	*Dual Problem*
Maximize profit,	Minimize imputed cost,
$P = P_1X_1 + P_2X_2 + P_3X_3$	$C = C_1Y_1 + C_2Y_2$
subject to resource limitations:	subject to the requirement that all
$a_{11}X_1 + a_{12}X_2 + a_{13}X_3 \leq C_1,$	profits are imputed:
$a_{21}X_1 + a_{22}X_2 + a_{23}X_3 \leq C_2$	$a_{11}Y_1 + a_{21}Y_2 \geq P_1,$
and the requirement that no out-	$a_{12}Y_1 + a_{22}Y_2 \geq P_2,$
puts be negative:	$a_{13}Y_1 + a_{23}Y_2 \geq P_3,$
$X_1 \geq 0, X_2 \geq 0, X_3 \geq 0.$	and the requirement that no accounting prices be negative:
	$Y_1 \geq 0, Y_2 \geq 0.$

Here, e.g., a_{23} represents the amount of scarce resource 2 that is required to produce a unit of output 3. Thus, the first inequality on the left states that the amount of resource 1 used on output 1 plus the amount used on output 2 plus the amount used on output 3 must not exceed C_1, the amount available of that resource. Similarly the first inequality on the right states that the value of the amount of resource 1 (valued at its accounting price, Y_1) used in a unit of output 1 plus

the value of resource 2 used in a unit of that output must be at least as great as P_1, the net profit of a unit of this output.

Notice the position of the a's in the two sets of inequalities. E.g., a_{13} appears on the left in the third term of the first inequality, while on the right it is the first term of the third inequality. Here is another duality symmetry.

. It is now easy to prove the following very useful theorem: Given any value P^1 of the variable P which is to be maximized and any value C^1 of the dual variable which is to be minimized, P^1 will never exceed C^1. In the present illustrative case the reason is obvious. P is total profit and C is total imputed cost and we have constructed imputed cost so that it will never fail to eat up profit.[23]

Two more remarkable theorems can also be proved:

Duality Theorem I. The maximum value of P will equal the minimum value of C, that is, total profit from the optimal output combination will just equal the total "optimal" imputed cost.

Duality Theorem II. If in an optimal solution, say, the *second* inequality in one problem involves less than full use of capacity (i.e., strict inequality: that is the sign is $<$ not $\leq$) then the optimal value of the corresponding (second) variable of the dual problem will be zero.

What does this mean economically? In our illustrative problem theorem II has two interesting interpretations. It states that if resource 2, say warehouse space, is optimally *not* used to capacity, then Y_2, the optimal accounting price of resource 2, is zero. That is, warehouse space will (as seems reasonable) be considered a free good. Actually this result merely suggests that the programming price imputation is not unreasonable. The second (dual) application of theorem II states that if, e.g., commodity 3 yields a unit profit less than the imputed cost of the resources used to produce it, then the optimal value of the corresponding variable, X_3, in the dual problem will be zero, i.e., commodity 3 will not be produced. This result states essentially, and plausibly, that scarce resources will be used only on the production of commodities whose profits are no less than their opportunity cost.[24]

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[23] More generally, to prove the theorem, consider first the problem on the left. Multiply both sides of the first inequality by Y_1, and those of the second inequality by Y_2, and add the corresponding sides of both inequalities. We obtain: .

$$Y_1 a_{11} X_1 + Y_1 a_{12} X_2 + Y_1 a_{13} X_3 + Y_2 a_{21} X_1 + Y_2 a_{22} X_2 + Y_2 a_{23} X_3 \leq C_1 Y_1 + C_2 Y_2 \equiv C$$

In the mess on the left, e.g., the term $Y_1 a_{12} X_2$ represents the value of resource 1 used in producing X_2 units of output 2, so that the entire expression (call it $\Sigma\ YaX$) is the accounting value of all resources used in producing all ouputs.

Now multiply the first inequality in the dual program by X_1, the second by X_2, the third by X_3 and add. We get $\Sigma\ YaX \geq P_1 X_1 + P_2 X_2 + P_3 X_3 \equiv P$. Comparing the results of the two additions our theorem follows at once: $P \leq C$. .

[24] One of the most impressive applications of the duality theorems occurs in the theory of games where the profit or utility maximization calculations of the two players in a "zero-sum two-person game" can be formulated as a pair of dual programs. In these pro-

From pure mathematics with some economic interpretations we turn now to another field involving some economic with a mathematical structure.

II. *Input-Output Analysis*

Input-output analysis, for which we are indebted to Leontief [7], is the name given to the attempt to take account of *general equilibrium* phenomena in the *empirical* analysis of *production*. The three italicized elements in this statement are crucial and merit further discussion. Reversing their order, I re-emphasize, first, that the analysis deals almost exclusively with production. Demand theory plays no role in the hard core of input-output analysis.[25] The problem is essentially technological. The investigation seeks to determine what can be produced, and the quantity of each intermediate product which must be used up in the production process, given the quantities of available resources and the state of technology.

The second distinctive feature of input-output analysis is its devotion to empirical investigation. This is primarily what distinguishes it from the work of Walras and later general-equilibrium theorists. A consequence of this no doubt long-overdue concern with the facts is that compromises have been forced on the investigator. Input-output employs a model which is more severely simplified and also more narrow in the sense that it seeks to encompass fewer phenomena than does the usual general equilibrium theory. Its narrowness lies in its exclusive emphasis on the production side of the economy. Its oversimplifications I shall discuss presently.

The third distinctive feature is its emphasis on general-equilibrium phenomena.[26] Input-output seeks to take account of the interdepend-

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grams each player determines the best odds he can use to "load" a random device which chooses among the strategy alternatives open to him. Duality theorem II then tells us that the programming calculation will attach probability zero to any strategy which promises to be advantageous to his opponent. Duality theorem I leads directly to what has been called the fundamental theorem of the zero-sum two-person game. This theorem states the following. Suppose one player picks a strategy pattern which sets the highest possible floor under his earnings, i.e., it gives him maximum protection by guaranteeing that he will earn at least some sum L. Suppose moreover that his opponent adopts a similar counterstrategy which, by symmetry, sets the lowest possible ceiling C on the first player's earnings. Then the fundamental theorem states that the first player will in fact turn out to earn L, that his opponent will be left with $-C$ *and that* C *will always equal* L.

[25] This is strictly true only of the open model which is described here. In this model the final demand sector is in effect taken to be outside the production economy and final products are exported to consumers. There is, however, a closed model in which labor is treated as a produced commodity and consumption as the raw materials used up in the production of labor. Here at least some rudimentary demand analysis must enter to show how the levels of consumption demands are related to the levels of labor inputs supplied.

[26] This term equilibrium is misleading here. The outputs found by this method need not satisfy market equilibrium conditions. The analysis qualifies for the title general equilibrium in that it takes account of the interdependence of the various sectors of the economy. Perhaps we can say, more properly, that it is general but not equilibrium.

ence of the production plans and activities of the many industries which constitute an economy. This interdependence arises out of the fact that each industry employs the outputs of other industries as its raw materials. Its output, in turn, is often used by other producers as a productive factor, sometimes by those very industries from which it obtained its ingredients. Steel is used to make railroad cars and railroad cars in turn are used to transport steel and the coal and pig iron which are used in its manufacture.

The basic problem, then, is to see what can be left over for final consumption (consumer, military, etc.) and how much of each output will be used up in the course of the productive activities which must be undertaken to obtain these net outputs. It should be clear that a successful attack upon these problems can result in an abundance of applications. It can be used in predicting future production requirements if usable demand estimates can somehow be obtained. Particularly, it can be used for economic planning including problems of mobilization. A more modest purpose which it has already succussfully begun to serve is the provision of a very illuminating detailed structure for national income accounting.

As stated earlier, the intransigence of the empirical materials and the computational problems have forced on input-output analysis a number of simplifying assumptions even more extreme than those usually employed in our theoretical models. Particularly noteworthy are two assumptions, each of which has to some extent been relaxed in practice. One assumption, which I will not discuss, is that no two commodities are produced jointly. Each industry produces only one homogeneous output. But this restriction can be somewhat relaxed by making this to be a composite commodity which is made up of several items produced in fixed proportions.

Perhaps more serious is a second assumption, that in any productive process all inputs are employed in rigidly fixed proportions and that the use of these inputs expands in proportion with output. This is much stronger than the assumption of constant returns to scale which is perfectly consistent with the substitution of one factor for another. A linear homogeneous production function can, for example, permit both labor-intensive and capital-intensive processes and states only that a tripling of the scale of either type of operation will triple output. Not so the Leontief system, which requires that a manufacturing process which is labor-intensive offer no option of a capital-intensive alternative. Whether this assumption is relatively innocuous or does considerable violence to the input-output results is still under dispute. But it must be emphasized that the premise is certainly never true, even in those cases where chemistry and engineering dictate fixed proportions

between some ingredient and output. As Jacob Viner has pointed out in conversation, there is even room for variation in the amount of gold *used up* in producing a fixed amount of gold leaf of given specifications. For a rise in the price of gold can lead to more careful recovery processes being employed in the search for dust and scraps in the workroom (a substitution of labor for gold). Or a rise in the price of gold may lead to the use of less economic mining opportunities which at least affects the labor intensity of the output.

Basically the input-output analysis consists in nothing more complicated than the solution of a set of N simultaneous linear equations in N variables. To illustrate this let us consider a three-industry economy which produces coal, steel and the service of railroad transportation. Each of these is measured in dollar terms. Each of these industries employs the products of the others in its manufacture, say in proportions shown by the following table:

| | | User of Output | | |
		Steel	Coal	RR
	Steel	0.2	0.2	0.1
	Coal	0.4	0.1	0.3
Producer of Input	RR	0.2	0.5	0.1
	Labor	0.2	0.2	0.5

For example, the first column of the table states that every dollar's worth of steel uses in its manufacture twenty cents in steel, forty cents in coal, twenty cents in railroad transportation and twenty cents in labor.

Suppose now that somehow there have been set consumer output targets of $100 million in steel, $20 million in coal and $40 million in railroad transportation. The question is, how much of each of these goods will have to be manufactured for both consumer and industrial use to meet the final output goals. Let S, C, and R represent the dollar value of this total output of steel, coal and railroad transportation, respectively. Let us first examine the demands on the steel industry: in addition to the 100 demanded by final consumers, there will be the demand for its product for internal use which (the table tells us) amounts to 2/10 of the total steel output or $0.2S$. Similarly the railroad industry will require 1/10 of a dollar of steel for every dollar of its service, so that the total railroading demand for steel will be $0.1R$, etc. Thus we have the following equation which states that after all

these industrial demands have been subtracted from the total steel output, S, there must be 100 units left for consumer use:

$$S - 0.2S - 0.2C - 0.1R = 100$$

or

$$0.8S - 0.2C - 0.1R = 100.$$

Similarly we have the following two equations giving the amounts of coal and rail transportation available for final consumption:

$$-0.4S + (1 - 0.1)C - 0.3R = 20$$

and

$$-0.2S - 0.5C + (1 - 0.1)R = 40.$$

These are three simultaneous linear equations in the three unknowns S, C and R. If we solve the equations for the values of these variables we find what we started out to seek—the total outputs of the three commodities needed to meet the stated consumer targets. Only one more step is required. We note from the input-output table that 0.2 dollars of labor time are consumed in the manufacture of one dollar of steel, so that $0.2S$ dollars of labor will be needed to produce the required S dollars of steel production. Continuing in this way we see that $0.2S + 0.2C + 0.5R$ dollars worth of labor will be needed to produce the outputs of the three commodities required by our program. Taking the price of labor to be fixed, this involves a specific requirement of labor man-hours. If this computed number does not exceed the available supply, all is well—the targets are feasible. Otherwise more modest targets must be substituted. That is the core of the theory of input-output.

   We can see now why it is so convenient to work with fixed coefficients of production. With variable input proportions single numbers will not suffice in the input-output table. Instead we would have to deduce, from the available statistics and engineering information, functional relationships between the level of output of each industry and the quantity of each input which would be required to produce it. The enormous statistical problems should be obvious enough. It is equally clear that the relevant equations would be complicated enormously. Even with the huge economy effected by the fixed-coefficients premise the statistical and computational difficulties are tremendous. We can see that the three first rows of our table contain nine figures, the three inputs required by each of the three industries. Similarly a four-industry model would require more than 16 figures and so on. The number of required pieces of statistical information increases as the square of the number of industries considered, although in practice the work is reduced by the fact that many of the entries in the input-output table are zeroes be-

cause some industry, A, does not use as an input any of the products of some other industry, B. It can also be shown that the number of computational steps involved in solving the equations increases approximately as the cube of the number of industries. Thus the labor involved in an input-output analysis rapidly becomes astronomical as the breakdown of industrial classifications becomes finer. A table has been constructed involving some 400-industries[27] but most computation has involved considerably fewer industries. Certainly even 400 industries is too coarse a breakdown for most detailed planning purposes in an economy where the number of items produced can be considered to go well into the millions.[28]

III. *Activity Analysis*

A. *The Existence and Uniqueness Problems*

I take the term activity analysis to refer to the applications of linear programming methods to general equilibrium *theory*. The last few years have seen a new burst of effort devoted to this area which, at least until the '30's, had remained pretty much as it was left by Walras. The three outstanding developments relate to the solvability of the Walrasian equations, the development of general-equilibrium growth models and the application of general-equilibrium theory to welfare economics. In all three cases, two of which are discussed below, the main advance has consisted in the development of powerful methods rather than in the discovery of surprising new theorems. For this reason much of the discussion which follows is devoted to the description of mathematical arguments and analytic techniques, and its economic content may on first reading leave the neophite rather disappointed.

Walras was much concerned with the solvability of his equation system. That is, he wanted to be sure that the system of equations he had set up sufficed to determine the values of his variables—the prices and quantities of the economy's outputs and inputs. Some writers approached this by counting the number of his equations and unknowns. They found he had the same number of equations as unknowns and assumed that the problem was solved.

Unfortunately the matter is much more complicated. We usually expect a supply and a demand equation (curve) to determine a single

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[27] The fullest published report on this computation is by W. D. Evans and M. Hoffenberg[5].

[28] Further theoretical work on input-output analysis has taken a number of directions—attempts to weaken the artificial assumptions and to establish criteria for the feasibility of the solution. And since the work of numerical computation in the solution of a set of simultaneous equations grows so complex as the number of equations increases, there have been attempts to find labor-saving methods. For one example see [13].

equilibrium price-quantity combination but this is certainly not true of either of the following pairs:

Demand: $Q = 1000 - 5P$	$Q = 1000 - 5P$
Supply: $Q = 900 - 5P$	$2Q = 2000 - 10P,$

where P is price and Q is the quantity sold. The pair of equations on the left has no solution (no solution *exists*)—any price-quantity combination which satisfies the one cannot possibly satisfy the other because $Q + 5P$ cannot be both 1000 and 900 at the same time. The trouble here is that the supply and demand curves are parallel straight lines and never intersect. In this case we say that the equations are "inconsistent" and the system is "overdetermined."[29] By contrast the other set of equations offers us an embarrassment of riches. It is compatible with an infinite number of price-quantity combinations. (The solution is not "unique.") In fact since negative prices and quantities have not been excluded, every price can be an equilibrium price. In this case the difficulty is that the supply and demand curves coincide so that at every point of this single curve demand will equal supply. Here we say that the equations are not independent and the system is underdetermined.[30]

If there are scarce resources the Walrasian system may get into trouble in yet another way—the solution to the equations simply may not be feasible because the available resources do not suffice or are available in the wrong proportions to produce it. It may rightly be suspected that this is where programming enters in, for we are almost[31] back at the production-with-limited-capacity problem.

It should be made clear that an existence theorem (a theorem which states that some equation or set of equations possesses at least one solution) does not tell us anything about the operation of the economy—rather it tells us something about the operation of the Walrasian model. As Dorfman, Solow and Samuelson point out [4, pp. 350-51], we know by observation that the market somehow determines unique prices and quantities. Thus the market's "solution" always exists. An existence analysis can serve only as a test for a general equilibrium model, in that, if it turns out that the model possesses no solution we will perhaps want to reject it on the grounds that it may therefore neither be very helpful analytically nor very realistic.

[29] Just as it might be if we had three well-behaved equations in two unknowns.

[30] As would ordinarily be the case where a system consists in one equation in two unknowns.

[31] But not quite—because we have not found anything to maximize. It should be recalled that this capacity problem also occurred in the input-output analysis where the labor requirements of any output target had to be checked against the available labor supply.

An existence theorem is a rather esoteric idea. It assures us that a problem can be solved but it may tell us nothing about how to go about solving it. Nevertheless, it is more important even to an economist than it may at first appear to be. We know that a system which has passed the test of an existence theorem can contain no contradictory elements since, clearly, any contradictions within the system would make a solution impossible. This may even have some direct economic implications. For example, an existence theorem for a system which postulates both full employment and an "ideal" allocation of resources proves that these two desiderata are not incompatible goals. In other words, such a theorem can tell us whether we are pursuing aims which involve having our cake and eating it.

An existence theorem or a uniqueness theorem (a theorem which states that the system has no more than one solution) can have further economic relevance in another way. Often it will turn out that we can prove an existence theorem or a uniqueness theorem for a system only if it satisfies some special requirements. For example, we shall see later how such a restriction on the nature of consumer demand is used to prove uniqueness in a general equilibrium model. Now these requirements can be highly suggestive in indicating conditions which may be necessary for such an equilibrium to occur in the real world. This will become clearer in our discussion of the uniqueness problem below.

1. *Solution of the existence problem.* Existence theorems are closely tied in with the so-called fixed-point theorems. First let us see what is meant by "a fixed point." Suppose we have some functional relationship $Y = f(X)$ which associates different values of Y with X. Then a fixed point is a specific value of X, say $X = X^*$ (some number), for which $Y^* = f(X^*) = X^*$, i.e., for which the value of Y is equal to the value of X. The reason such a value of X is called a fixed point can be made clear with the aid of the following illustrative table which gives Y as a function of X:

X	1	2	5	6
Y	9	7	5	11

These data may be given the following geometric interpretation: We have four markers (e.g., paper clips) on a rule, one at each X figure, i.e., one at the 1-inch mark, one at the 2-inch mark, etc. The function gives us directions for moving these markers. It tells us to move the paper clip at the 1-inch mark to the 9-inch mark, the one at the 2-inch mark to the 7-inch mark, etc. Note, however, that the instructions tell

us not to move the paper clip from the 5-inch mark. That is $X = 5 = Y$
is a fixed point for this function. As another example, we note that
$X = 1$ is a fixed point for the equation $Y = 3 - 2X$ because for $X = 1$,
$Y = 3 - 2 = 1$.

But how are fixed-point theorems involved in existence proofs? The
answer is that for a wide class of problems they are practically one and
the same thing. Suppose, for example, we want to prove that there exists
a root for the equation $f(X) - 5 = 0$. This is the same as finding a
fixed point for the equation $Y = f(X) - 5 + X$. For if $X = X^*$
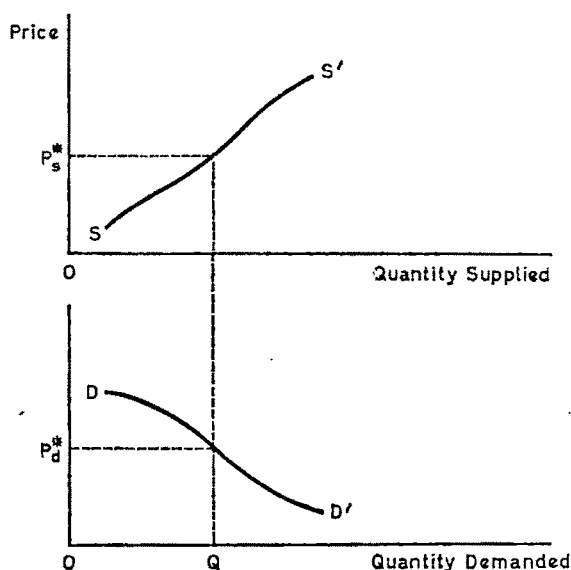
FIGURE 4

(where X^* is some number) is such a fixed point we have $Y = X^*$ so
that the equation becomes $X^* = f(X^*) - 5 + X^*$. Subtracting X^*
from both sides, we see that $0 = f(X^*) - 5$; i.e., X^* must be a root of
our original equation. More generally, we see that X^* is a root of the
equation $0 = G(X)$ if and only if it is a fixed point for the related func-
tion $Y = G(X) + X$.

There is another way in which we can see the relation between a
fixed-point theorem and an existence proof, this time for the solution
of a pair of simultaneous equations. Suppose we are, for example, try-
ing to find a solution to the standard supply-demand problem. A clumsy
way to go about it is to draw the demand and supply curves one above
the other as in Figure 4. We then pick some price OP_d on the Y axis
of the demand diagram, see what quantity, OQ, buyers are willing
to buy at this price, and then by moving vertically to the supply curve,

we find the price, OP_s, at which sellers are willing to supply that quantity. In this way we obtain a relationship which gives supply price as a function of demand price, $OP_s = f(OP_d)$. If it turns out for some particular demand price $OP_d{}^*$ and its associated supply price, $OP_s{}^* = f(OP_d{}^*)$, that we have $OP_d{}^* = OP_s{}^*$, it is clear that price $OP_d{}^*$ is a fixed point for this function. But it is also obvious that $OP_s{}^*$ and $OP_d{}^*$ are then the equilibrium supply and demand prices. We see then that if the function which relates supply price to demand price has a fixed point there must exist a solution to the supply-demand equations. This,
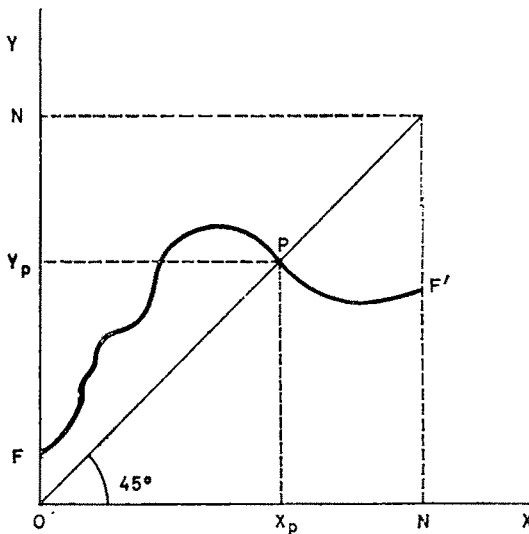
FIGURE 5

as we shall see presently, is in essence an outline of the McKenzie proof of the existence of equilibrium in the general equilibrium system [9].

The proofs for fixed-point theorems are generally very deep and complex. However, there is one very simple case which is usually used as an illustration. This simple theorem states that in *any* two-variable equation, $Y = f(X)$, if $f(X)$ is continuous (roughly, if there are no breaks in its graph—kinks, though, are permitted) and if Y is never negative and never larger than some (any prespecified) number N, the function will possess a fixed point. This is shown in Figure 5 which plots the function $f(X)$ as FF' between $X = 0$ and $X = N$. The graph also contains a 45° line through the origin, and it is clear that any point such as P where FF' intersects the 45° line is a fixed point, for there we will have $Y = X$. There are two possibilities: if $Y = 0$ at $X = 0$ this is our fixed point. On the other hand, if $Y \neq 0$ at $X = 0$

so that FF' starts out above our 45° line, it can "try to avoid" the 45° line by staying above it. But since, by assumption, F can never be greater than N, FF' will be kept from rising above the upper dotted line and the 45° line must catch up to it and corner it at $X = N$ if not sooner.

The first proof of the existence of a solution to the Walrasian system was published by A. Wald in 1933 [12]. His proof was exceedingly difficult. Like the authors of the more recent proofs, Wald had to impose several further assumptions on the Walrasian model. Several of Wald's assumptions have been considered excessively restrictive and economically unjustified. Since then two alternative proofs have been offered—one by Arrow and Debreu [2] and another by Lionel McKenzie [9]. Both of these involve much weaker and more plausible assumptions. The former (Arrow and Debreu) is more complex but it covers somewhat more territory than does the latter, which, like Wald's, deals with the more aggregative general equilibrium model of Cassel.

Without going into details we can now very briefly outline the McKenzie proof. His model contains two sets of inequalities and one set of demand equations. The first set of inequalities states that production takes place under constant returns to scale, and uses up no more than the physical resources which are available.[32] The second set of inequalities states that since we are dealing with a case of perfect competition, profits will be zero. All products sell at a price which is no higher than the cost of production. (Processes which involve a cost greater than the price of the commodity will, of course, not be used). The problem is then to show that there exists a set of factor prices, commodity prices and outputs which are consistent with those resource limitations, profit limitations and the market demand relationships.

We now proceed exactly as in the discussion of Figure 4 (p. 860), only here we must deal with many prices and quantities at once. Pick some arbitrary set of prices and find the quantities which the demand functions tell us consumers will buy at these prices. Suppose that these quantities are producible within the given resource limitations.[33] Then use our second set of inequalities to see what supply prices are just compatible with these output plans and the no-profits perfect-competition requirement. In this way we deduce a set of supply prices from any assumed set of demand prices exactly as we did in Figure 4. As in the discussion of that diagram, to show that an equilibrium price-quan-

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[32] These are the same as the inequalities in our illustrative linear programming model. Moreover, the next set of inequalities are those of the dual problem. Dorfman, Samuelson and Solow employ this remarkable piece of luck to simplify the McKenzie proof by use of duality theorem I above [4, pp. 368-74].

[33] It can be shown that at the prices which finally emerge the quantities demanded will indeed be feasible. Their production will require no more than the available resources.

tity combination exists, it is necessary to prove that the assumed demand prices and the deduced supply prices can coincide. It is here that we must appeal to a fixed-point theorem. It can be shown that there is such a theorem which applies to our problem and proves that there is one set of demand prices which is the same as the set of supply prices deduced from it. At these prices and quantities, then, the demand conditions, the production conditions (resource limitations) and the profit conditions are all satisfied. This, in outline, is the McKenzie proof of the existence theorem for a general equilibrium model.

2. *Solution of the uniqueness problem.* Uniqueness is mathematically somewhat simpler to prove, though perhaps it is less plausible economically. There is really no good reason to believe that there will be no multiple intersections of supply and demand curves and hence a multiplicity of equilibrium points à la Marshall [8, p. 806n].

Further, the demand assumption which is used to prove uniqueness, while plausible enough for an individual consumer, is, at best, questionable when applied to the economy. This premise, which is employed in the uniqueness proof, turns out to be the basic assumption in Samuelson's revealed preference analysis. Suppose that an individual buys a collection of commodities *A* rather than some other collection *B* which is also available on the market. Presumably he will have made this choice either because he likes *A* better than *B* or because *A* is cheaper than *B*.[34] If in fact *A* is more expensive than *B* when the consumer buys *A* then the second possibility is ruled out—our consumer must have bought *A* because he actually prefers it to *B*. We therefore say, when a consumer buys the more expensive of these two collections, that *A* has been *revealed* preferred to *B*. Suppose that a different set of prices could have led the consumer to change his mind and buy *B*. If his tastes do not change so that he still prefers *A* to *B* he presumably will not buy *B* when it is more expensive than *A*. At least this will be the case if his tastes are consistent, for otherwise his buying of *B* rather than *A* when *B* is the more expensive collection reveals that he also prefers *B* to *A!* That is the basic revealed preference premise. In sum, it states that consumer tastes are consistent in the sense that if one set of prices reveals *A* to be preferred to *B*, then there exists no other set of prices which can reveal *B* to be preferred to *A*, i.e., which makes *B* more expensive than *A* and yet leads the consumer to buy *B*.

Now while this is a plausible requirement for consistent consumer behavior it has much less intuitive appeal when applied to market demand. It may be perfectly consistent for *the community* to buy *A* rather than *B* when *A* is more expensive and yet buy *B* rather than *A*

[34] Note that this rules out the possibility that we will want something just because it is expensive, e.g., cases of snob appeal or where quality is judged by price.

when prices change so that B is the more costly. This is because the price change redistributes real income among consumers whose expenditures patterns differ. Thus *different* consumer groups may foot the bulk of the bill in the two cases.[35] Despite these reservations, I shall proceed to indicate the line of reasoning which shows that the revealed preference premise for the market is violated if we have a multiplicity of equilibria. But before I can complete the argument one more preliminary theorem must be explained. This preliminary result will also be needed in the discussion of a later section.

At any *fixed level of input and output prices,* competitive outputs will always tend to maximize total net profits; e.g., if there is any opportunity to increase profit by increasing some output at the expense of another, individual businessmen will make this switch until the opportunity disappears.[36] But I shall argue now that this also means that the value of the final products (at these fixed prices) will be maximized.

To make the argument clearer consider a simplified production arrangement in which the economy's fixed resources are used only by the producers of raw materials who sell their entire product to the makers of finished goods. The total profits in the economy are given by:

The profits of finished-goods producers
plus
= { 1. The value of finished products
2. Minus the cost of produced raw materials

The profits of raw-materials producers
= { 3. The value of produced raw materials
4. Minus the cost of the economy's fixed resources

But the cost to finished-goods producers of their raw materials is exactly the same as the value of the product (revenue) of the raw materials producers. Hence, in adding items 1 through 4 on the right above, to obtain a figure for the total profit of the economy, items 2 and 3 must cancel out. We see then that the total profit earned in the economy will equal the value of the output of finished products minus the cost of the economy's fixed resources.

With a given set of prices for all products and resources, what can businessmen do to increase the economy's total profit? Since, by definition, the quantities of the various fixed resources are fixed, then with the prices of these items given, nothing which businessmen do will affect

[35] For a fuller discussion see J. R. Hicks [6, pp. 54-58]. Recently Samuelson has argued that the revealed preference axiom for the market and the uniqueness of the equilibrium which follows can plausibly be assumed of a competitive economy when income is always redistributed by lump-sum transfers in such a way that the marginal social significance of a dollar to all individuals is the same, e.g., if their marginal utilities of income are equal and all persons are weighed equally in some social utility function [11, pp. 19-20].

[36] Of course, the assertion breaks down in the presence of external economies and diseconomies.

the cost of society's scarce resources. Hence, anything which can be done to add to the total value of final output will add an equal amount to total profits (equals the value of final output minus the cost of fixed resources). It follows that businessmen will have maximized the total profits of the economy if, and only if, they have maximized the total value of finished products. And, as we saw at the beginning of the discussion, with any set of given prices the maximization of total profit may be expected in competitive equilibrium. Therefore, *competitive equilibrium will involve maximization of the total value of all finished commodities produced in the economy.*

We are now only one step from the end of the uniqueness argument. Suppose, on the contrary, that equilibrium is not unique—that there are two alternative equilibrium output combinations A and B, each with its own equilibrium prices. We have just seen that, at the prices which lead to the manufacture of A, the value of final outputs will be maximized, i.e., A will be at least as expensive as B. Similarly, at the prices at which B is produced, B will be at least as expensive as A. But if A and B are competitive equilibria, demand must match supply, that is, A must be demanded in the first situation (when it is most expensive) and B must be demanded in the second (when it is the most costly). A is then revealed preferred to B and vice versa. This clearly violates the revealed preference assumption for the market, so that if that assumption is to hold, there cannot be two equilibrium outputs, A and B. The equilibrium output must be unique.

B. *Activity Analysis and Welfare Economics*

General equilibrium theory has been useful to economists not primarily as a source of theorems, but rather in providing them with a frame of reference, one which is indispensable if many pitfalls of partial analysis are to be avoided. A notable exception is to be found in welfare economics in which the fundamental problem of the allocation of society's resources among the different outputs is by definition a problem in general equilibrium analysis. Doubtless the best known theorem of elementary welfare analysis asserts that a long-run perfectly competitive equilibrium will yield an optimal allocation of resources. Not only is the theorem elementary and well known—it is also, strictly speaking, untrue, or rather true only under some rather restrictive assumptions.[37]

In recent years there has been much work devoted to the development of an alternative activity-analysis proof of this theorem. It may well be asked why this has been thought to be necessary. For one thing

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[37] This is not the place to go into the details which have so often been discussed. See e.g. my *Welfare Economics and the Theory of the State* [3, ch. 6].

activity analysis has made no attempt to dispute the restricted validity
of the result. In fact no way has even been found to apply the methods
of activity analysis to external economies and the related problems
which are incompatible with the optimality of perfect competitive
equilibrium. Rather, the new approach has been helpful in another way.

The standard welfare economics deals only with commodities which
are actually bought in the market and not with those which are free
goods or for which no customers can be found at a profitable price. For
old-fashioned welfare theory leans heavily on the marginal conditions of
equilibrium, e.g., the condition of equality of price ratios to the marginal
rates of substitution. But these conditions need not hold for free or un-
salable goods. In old-fashioned terms, if each consumer chooses not to
buy a commodity the marginal utility of that item may well be *less* than
its price (note the inequality again). Moreover the cost of production of
such an unsalable good must be greater than its price. For free goods
the ratio of prices is not even defined. It follows that the standard
theorem of welfare economics must be restated to read that (where the
theorem is valid) a competitive economy will allocate resources opti-
mally among commodities which are salable without loss and which are
not free. But which commodities will these be? We cannot assume we
know the answer in advance, for the answer is an economic question of
costs of production and demand patterns. Moreover, though our intui-
tion may tell us that this is so, we must prove rigorously that there can
be no preferable allocation of resources to free or unsalable goods.[38]

Old-fashioned welfare theory, by taking marginal utility to equal
price, may end up requiring negative consumption of an unwanted com-
modity since even with zero consumption its marginal utility may turn
out to be less than its price. Similarly, it cannot preclude the economic
absurdity of negative prices for "free goods." But since activity analy-
sis can cope with inequalities, it can specify that (a) prices and quanti-
ties exchanged must all be greater than or equal to zero, (b) the aver-
age cost of production must be equal to price for all items which are
produced, and greater than the price at which any item that no one
considers worth producing can be sold, and (c) production must not ex-
ceed the levels made possible by the available resources of society. Sub-
ject to these and the limitations of competition, businessmen and con-
sumers are then taken to do the best they can for themselves. Marginal
equalities and inequalities do not even make an explicit appearance.

Before outlining the proof of the theorem, let us first recall two inter-

[38] Pigou long ago pointed out that the production of some competitively unprofitable
items can conceivably yield a considerable addition to consumer's surplus. See [10, pp. 283,
810-11]. However, Pigou's case requires decreasing costs and these cannot be handled by
the activity analysis approach as developed to date.

related concepts. A productive arrangement is called "efficient" if any alternative productive arrangement which increases the output of some commodity must also involve a decrease in the output of some other commodity. The motivation of the definition is obvious. Any productive arrangement which is not efficient in this sense requires that the economy forgo the opportunity to get something for nothing—the opportunity to increase the output of some item, X, without giving anything up in exchange. Related is the concept of "Pareto optimality." A situation is said to be Pareto-optimal when it is impossible to effect a change which benefits some individual without any deleterious effects on someone else. Efficiency is then a purely technological concept while Pareto optimality is the corresponding concept for individuals as consumers and in their other economic roles.
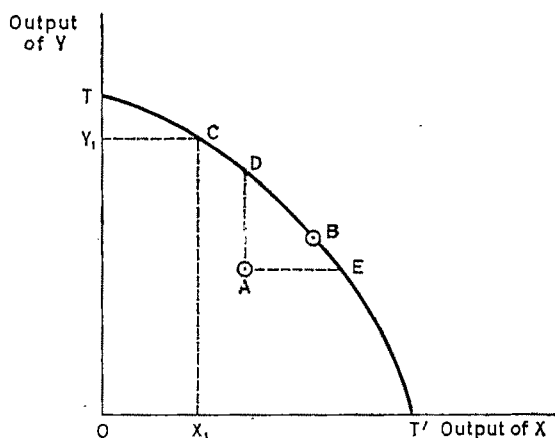
FIGURE 6

I shall now indicate how activity analysis can be used to prove that every competitive equilibrium is technologically efficient and that every efficient output combination is a competitive equilibrium, *i.e.*, for each efficient point there can be found a set of prices which would under perfect competition produce the efficient output combination in question. This part of the theorem amounts essentially to the standard result that a competitive output can occur at any point on the production-possibility locus (transformation surface) and can occur only at such a point. For the production-possibility locus is the locus of all efficient points. In Figure 6 we see that a point like A which lies inside the transformation locus TT' represents an inefficient combination of output X and Y because it is possible to move to an efficient point like B which lies on TT' northeast of A, so that B involves greater outputs of both com-

modities. C is also efficient,[39] but note that activity analysis has not settled whether it is or is not more desirable socially than is A.

How do we know that a competitive output is technologically efficient? The answer is simple. With fixed prices, an inefficient output cannot involve a maximum money value of output. For we can increase the output of, say, X without decreasing that of any other commodity and end up with an output combination whose money value is obviously increased. Thus, with fixed prices, the money value of E in Figure 6 is clearly higher than that of A. But we have seen earlier that a com-

FIGURE 7

petitive equilibrium necessarily involves a maximum value of output when valued at the equilibrium prices. Hence, no competitive equilibrium can be inefficient.

More difficult is the proof of the converse, which states that every efficient output combination is a competitive output. However, the method of proof is interesting in and of itself. This employs an "intuitively obvious" mathematical theorem whose proof is, in fact, rather difficult. The theorem states that, given an (N-dimensional) convex geometric figure (convex set) and any point R on the boundary of or outside this figure, it is always possible to draw at least one line (N-

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[39] We see then that there will usually be many efficient output combinations, which are represented by the points that make up the transformation locus. Moreover, the locus can be found with the aid of programming techniques. The trick is to choose any output of X, say OX_1 and find the *maximum* output of Y permitted by the resources left over from the production of that quantity of X. This is clearly a programming computation, and it will show that OY_1 is the maximum amount of Y then producible. In this way point C on the transformation curve will have been located, and by starting with other values of X other points on TT' can be found in the same way.

dimensional hyperplane) through R in such a way that the convex fig-
ure lies entirely on one side of this line. Thus in Figure 7 the shaded
region is convex. Through points A, B and C on its boundary, and point
D outside the figure, lines have been drawn which have the required
property. Such a line through a boundary point like A, B or C is called
a "supporting line" (plane) of the convex set. We saw above that a line
divides the plane into two "half-planes" (half-spaces) and in each
case the shaded figure lies entirely within one of the half-spaces pro-
duced by the lines in the figure. Note that supporting lines represent a
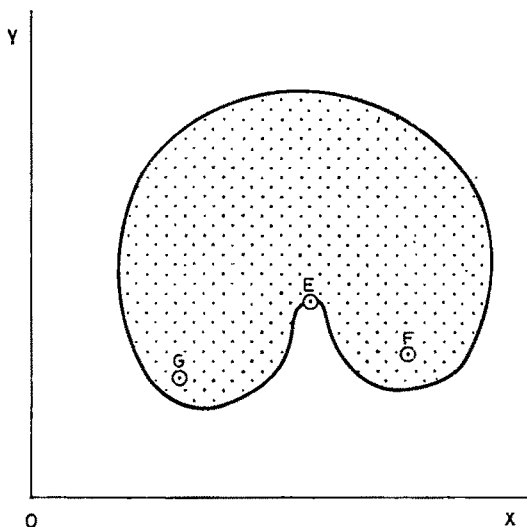
FIGURE 8

generalization of the concept of tangency to cover the case where
the boundary curve has a kink (e.g., at point B—note that there are
many supporting hyperplanes at such points). By contrast, in Figure
8 the shaded region is not entirely convex and we see that any line
through point E must intersect the shaded figure.

Consider now one of the supporting lines of the convex set, e.g., line
LL' through point A in Figure 7. Since the convex set lies entirely on one
side of such a line, then (if the line is not vertical) either no portion of
the convex set will lie above this line or no part of the set will lie below
this line. Suppose the convex set does not lie above the supporting line
in question (as is the case with LL'). Then any point of the set (e.g.,
point P) must lie on or below LL', and any line parallel to LL' which
goes through a point in the convex set (e.g., line MM' through point P)
must either coincide with LL' or lie entirely below it. In other words,
*such a supporting line must be the highest of all the lines which are
parallel to it and which meet the convex set at any point.* We shall em-

ploy this result by interpreting the family of lines parallel to our supporting line as a set of price lines which, as usual, involve price ratios that are given by the (constant) slope of these lines. Then the italicized result may be translated to read as follows: If only output combinations which are represented by points within the convex region are attainable, the highest attainable price line goes through A, the point of tangency with the convex region. Of course, this final statement sounds at least vaguely familiar.

Now to get back to our theorem that every technologically efficient point is a competitive equilibrium. We note first that the set of points representing all the feasible outputs in a linear program must form a convex geometric figure.[40] Suppose now that some output is efficient. We have seen that the point P which represents this output combination must lie on the upper boundary of the feasible region (Figure 6). Moreover, we know that through every such point there passes (at least) one supporting line. This line can be interpreted as a price line giving the value of the output through P and its slope can be taken to represent the ratio of the prices of X and Y. In other words for any efficient point there will always exist relative prices at which the efficient point maximizes the value of output.[41]

With these prices, moreover, businessmen under pure competition will (in the absence of external economies and diseconomies) be motivated to produce the technologically efficient output in question; for, as we have argued, it will pay them to maximize the value of output. Thus any such efficient output combination is a competitive output

[40] Mathematically convexity is defined as follows: A straight line connecting any two points in a convex region must lie entirely in that region. Thus the straight line connecting points A and C in Figure 7 will lie entirely within the shaded region. But the line connecting points G and F in Figure 8 must leave the shaded region which is therefore not convex. To prove that the set of feasible outputs is convex, suppose that A and C are feasible outputs. Convexity then follows if it can be shown that any point, H, on the straight line connecting A and C is also feasible. But H can be attained by a suitable scaling down of the activities at A and C, e.g., if H is the midpoint of line AC it represents half the outputs and inputs at A plus half the outputs and inputs at C. Since the production function involves constant returns to scale this must involve $\frac{1}{2}$ the resources used at A plus $\frac{1}{2}$ the resources used at C. Since both of these are feasible outputs they must each use no more than the available amount, R_i, of any resource i. It follows that H will use no more than $\frac{1}{2} R_i + \frac{1}{2} R_i$, that is, production of H will require no more than R_i, the amount of resource i available, so that by definition, H will also be feasible.

[41] The economic interpretation requires that the price line have a negative slope. But we can show that at an efficient point the slope of the price line must be negative. For we know that if all prices are positive the slope of a price line will be negative. (Indeed this is why we want the price line to have a negative slope.) But suppose the contrary, that one of the "prices" deduced from the supporting price line is negative. We can then increase the value of output by reducing the production of the item whose price is negative, i.e., we can increase the value of output by leaving the efficient point, contrary to what has just been shown. i.e. that the value of the efficient output with this price line will be a maximum.

combination: at some set of prices competitive businessmen will produce it.

This, then proves the theorem about the efficiency of competitive equilibrium. There is some similarity between this proof and the proof of the Pareto optimality theorem which will not be described in detail. Clearly the latter theorem must take into consideration consumer demands and this involves a number of complications. In effect, what we want to prove is that every competitive equilibrium involves tangency between the transformation locus and some sort of consumer's community indifference curve, and that any such point of tangency is a competitive equilibrium. For then society will have attained the highest state
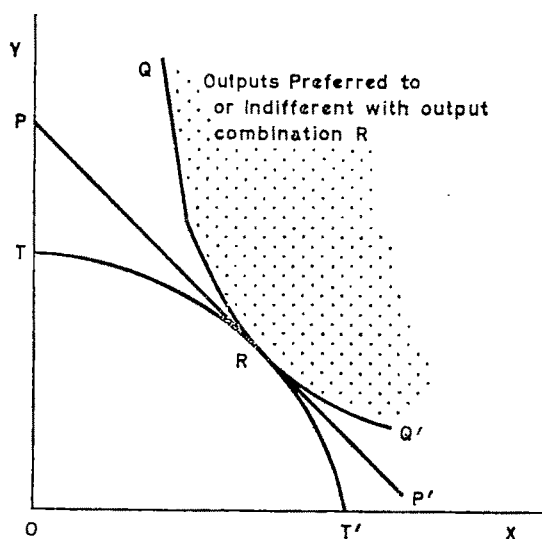
FIGURE 9

of welfare compatible with the available production possibilities. The tangency can be assured by the tangency of both the transformation curve and the community indifference curve with the same price line.[42]

Here too we have a theorem on convex sets which comes to our assistance. The theorem states that if two convex sets meet only at boundary points there will be at least one line such that one convex set lies in one of the half-spaces generated by the line and the other convex set lies in the line's other half-space.

Consider all output combinations above and to the right of some indifference curve, i.e., all the points which involve outputs that are preferred to or indifferent with the outputs at some point, R, on the indifference curve QQ' (Figure 9). Such a preferred point is defined in the Pareto sense as one that involves an output combination which can

[42] See Baumol [3, pp. 26-34] for details of the standard form of the argument.

make some consumers better off than they were at R, without hurting anyone. It can be argued that the set of points preferred to or indifferent with R (shaded region in Figure 9) is convex. The set of feasible output points (the points in region OTT' which lie on or beneath the transformation curve, TT') also constitutes a convex set, as we have seen. There will thus be a line, PP' which separates these two regions. If, in addition, point R lies on the transformation locus, PP' will be a supporting line for both sets and R will be a feasible efficient point which lies on the highest possible indifference curve.

IV. Concluding Remarks

What then are we to make of all this? The reader who has struggled through these many pages may well feel that he has really learned remarkably little about the operation of an economy. Programming is mathematics, not economics. Input-output is a noteworthy exception —it has given the profession a number of very interesting insights, but these arise primarily out of its empirical findings, not out of its theory, and so they are not reported here. The most heroic accomplishments discussed in this article—the existence theorems—are mainly reassuring. It is nice to know that someone somewhere has shown that our model does indeed have a solution. But again such theorems tell us little about the behavior of firms, consumers, national income and terms of trade, which are after all what we are in business to discover. The becoming modesty of some of the writings on existence and uniqueness is commendable but not comforting to the economist reader who wonders whether his time is well spent.

The welfare economics results are only a little different in this respect. The theorems are no doubt of very considerable interest and importance to the economist. But, we had these theorems before. Essentially, activity analysis has only assured us that one potential source of difficulty really does not cause trouble. But it has plugged this loophole only at great expense—by ruling out increasing returns to scale, external economies and a host of related and important matters. Again, the reader may feel that, though it is well to have had someone go through the excercise of checking up on this point, for himself it is a matter of business as usual during minor alterations.

Yet such doubts as these can lead the reader to miss the very real importance of what has been accomplished by activity analysis. It is truly a revolution and a very profound revolution at that. But the upheaval is a matter of methodology rather than content. I myself believe that theory should consist primarily in a body of generalized methodology plus a set of special models each constructed to deal with special problems as the need arises. The theory as presented is low in economic content only because the specific problems have not been pre-

sented. So long as theorists primarily seek quasi-eternal verities and more general generalizations it is not only activity analysis whose content will continue to be low.

The economist who turns to the opposite extreme of (applied) problem-solving must be impressed at the contribution of activity analysis. In my own consulting work, in case after case, these methods take one to the heart of the problem. In deciding how many products a firm should sell, which warehouses it should use, or which markets do not merit cultivation, a crucial part of the answer is, after all, which activities shall not be used at all. Here, as we have seen, the marginal equalities break down and must be replaced by inequalities. I can easily go on adding illustrations from this sort of "horribly applied" work, as one of my friends once facetiously called it. Suffice it to say that while activity analysis may have contributed little to our understanding of the economy "in general," it is no different from some other bodies of economic theory in this respect. And in what, to me, is the crucial test, the ability to help find the answer to concrete, well defined, specific (but not necessarily applied) questions, the approach has not been found wanting.

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CONSUMER INVESTMENT EXPENDITURES

By JAMES N. MORGAN*

Data from ten annual Surveys of Consumer Finances are utilized in an investigation of consumer expenditures on cars, durable goods, and additions and repairs to houses, here called "consumer investment expenditures." A brief summary of findings follows:

1. Unlike any other known country or period, postwar America shows a relatively constant proportion of income after taxes spent on consumer investment items at all income levels, except for the very lowest (under $1,000) and the very highest (over $7,500). If the category also included purchases of homes, fur coats, motor boats, and summer cottages, the proportion probably would not decline even at high incomes. This makes consumer investments a curious type of expenditure, neither necessity nor luxury, though perhaps closer to the former.

2. Demographic factors prove important in explaining cross-section differences, so much so that they may even be useful in middle-range predictions. Dates of birth and marriage seem particularly important.

3. The proportion of income spent on consumer investments varies over time, probably in response to major events and to changes in consumer attitudes.

4. With respect to changes in income, consumer investment expenditures appear to be sticky downward and flexible upward. Resistance to a decrease in such expenditures is particularly strong among those recently married, as is willingness to expend an even larger proportion of an increased income.

5. There was some evidence of a positive effect of beginning-of-the-year liquid asset holdings on consumer investment expenditures for the years 1947-1950, but some evidence that the effect has since become smaller and perhaps even reversed. If consumers alternated between liquid saving and investing in durables from year to year, there would be a correlation between liquid assets at the beginning of the year and

consumer investment expenditures during the year, even though the liquid assets did not cause the spending. At any point in time those in a sample who had substantial liquid assets would be those who were at the point in their individual saving-purchase cycle where they were ready to purchase again.

6. After the effect of the major demographic variables on consumer investment expenditures have been eliminated, some attitudes expressed at the beginning of a year appear to be related to subsequent levels of expenditure during the year.

7. Willingness to use credit does not seem to be associated with the level of consumer investment expenditures.

I. *The Present State of Research and Theory*

In the growing body of theory and empirical research on consumer saving, the treatment of consumer durables has been a major source of difficulty. Raymond Goldsmith and Milton Friedman, for somewhat different reasons, both prefer to include investments in durables in saving, and their depreciation in consumption [9] [10]. Earlier investigations by Lawrence Klein excluded expenditures on durables from saving [19], and those by the author included them. A joint article indicated some of the differences in results depending on the definitions used [21]. Empirical work by Modigliani-Brumberg, Milton Friedman, Malcolm Fisher, and others directed at the permanent income hypothesis has sometimes treated durables one way, sometimes another, depending on the preferences of the author or the availability of data [8] [9] [29]. Most other empirical work on saving or the consumption function has excluded investments in durables from saving and put them in consumption, and most of the available data on cross-section or aggregate saving follow the same course.

Yet, examination of both cross-section and aggregate time-series data will reveal that consumer investments in durables behave neither like consumption nor like saving. Indeed, they are an extremely important and volatile kind of expenditure worthy of separate study [27]. The well-known fact, documented again by our findings, that expenditures on durables are highly (negatively) correlated with age, means that in cross-section studies where age is used as an important variable for other theoretical reasons, the inclusion or exclusion of durables expenditure in "saving" will dramatically change the results [21].

In the recession of 1957-58 there was abundant evidence of the fact that the decline in consumer spending had been primarily in the durables. George Katona and Ruth Mack had pointed out long before that durables are frequently purchased as soon as income increases, in expectation of a new higher level of living [13] [15] [26]. Finally, rein-

terview studies indicated a high degree of substitutability (competition) between expenditures on durables and liquid saving [19, p. 77]. Indeed, recent studies of consumer attitudes and plans have generally used some measure of expenditure on durables as the test indicator of the level of discretionary spending [16] [17] [12].

We are led, then, to consider the consumer's use of his income as involving more than the two categories, consumption and saving. If it were possible, we might like to separate both spending and saving into fixed and discretionary components, but lacking evidence on how to make such a classification, we can at least consider separately consumption, saving (in liquid assets or monetary investments), and investments in physical assets. This has the double advantage of dealing separately with the third component which is neither saving nor consumption from the consumer's point of view. If these consumer investment expenditures are the prime competitor for what funds remain after the down-the-drain living expenses are met, and if most direct saving is contractual and semicompulsory (annuities, life insurance, mortgage payments), then it may well be the fluctuations in consumer investments that primarily determine short-run changes in consumer saving. The decisions of consumers, in other words, may be about these investment expenditures rather than about either regular consumption expenditures, or about liquid saving which is then a "residual." A subsidiary reason for analyzing these items of consumer expenditure is that they represent large items, important and interesting to consumers, and hence easy to remember and probably reported with more accuracy than most expenditures. The survey data on them are probably considerably more accurate than those on saving.

The study reported here is one of consumer investment expenditures defined as expenditures on household appliances, additions and repairs to homes, and purchases of automobiles (net of trade-ins). Data from seven Surveys of Consumer Finances on expenditures during the years 1947-1953 were used in the main analysis [22] [36]. Even after the exclusion of farmers and some other unusual groups this left some 16,000 cases. Later data were added to some tables for 1954, 1955 and 1956 as they became available.[1]

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[1] There have been other studies of consumer expenditure on durables or on some particular durable. The aggregate studies have of course been limited to the use of a limited number of explanatory variables and will not be cited here. A few studies have combined time series and cross-section data in ingenious ways [6] [25] [28]. A number of analyses have been made of cross-section survey data where more flexibility of analysis is possible, and one uses state data in a cross-section analysis [35] [7] [19] [20] [28]. Little or nothing is said in most consumer economics texts about these investment items, and in many consumer expenditure studies they are divided and mixed with other expenditures in categories such as "transportation," "housing," and "household equipment."

II. *Definition of Consumer Investment*

Most previous studies have been devoted to expenditures on cars, or on cars and durables, or on household equipment alone. We chose to combine all these plus additions and repairs to houses. The reason was that they all represent consumer expenditures which are postponable, suitable for financing on the instalment plan, widely variable as to amounts that can be spent, perhaps status-involved, and certainly subject to rapid style-change. If they are highly substitutable in the consumer's mind, then it is better to analyze them together. They also represent commitments to a future way of life, to a level of consumption through depreciation (25 per cent per year on the value of the car), hence should respond to changes in one's long-range outlook. Of course, we are using the term "investment" in a gross sense, not net of depreciation, and in a somewhat loose way. Some small fraction of home repairs will last less than a year and should not be considered investment at all. However much one might want to distinguish between durables and additions it is difficult in practice. A window fan is a durable, an attic fan an addition. A dishwasher is a durable if it is movable.

An additional (statistical) reason for using this definition of investment was that our data deal with expenditures over a one-year period. In any one year, less than a fourth of all spending units buy a car, less than a tenth buy a new car, two-fifths buy some durable as defined in the Surveys of Consumer Finances (furniture or large household appliances). Three-fifths of urban home owners and one-fifth of nonowners spend money on additions or repairs. It is difficult to analyze data composed of some expenditures and a large number of zeros—the means are unstable, the distributions bimodal. One could, by using reinterviews or by relying more on respondents' memory, use expenditures over longer periods, but no large representative samples of either sort are available.

We did not include the purchase of a home for a similar practical reason: only 4 to 5 per cent of spending units make such expenditures in any one year, and these expenditures are so large that they would have made means or relations unstable. Also, we did not have the data necessary to deduct proceeds from the sale of a previous home to get net expenditures on homes. While payments on mortgage principal increase equity in a home, they do not involve an active decision and should not be included here.

While there were some minor differences in the successive surveys as to whether trade-ins on appliances were deducted, or net outlays on second or third cars included where people purchased and kept more than one car during the year, the methods of collecting and handling the

data were remarkably uniform over this ten-year period. The most important change came in 1953 when a revised income schedule increased efficiency in securing information about secondary earners, and hence increased estimates of income of some families. This results, of course, in what appears to be a slight drop in the proportion spent on consumer investment items. There were some changes in the list of actual appliances mentioned in the question, and this affected the reporting of purchases, and in one year borrowed funds used for additions and repairs were excluded.

III. *Dependent Variable and Procedure*

The dependent variable for the analysis was the proportion of disposable income (money income after estimated federal income taxes) spent on consumer investment items as defined above.

Why not a several-stage analysis first to "predict" whether or not one or several types of expenditures would be made, and then to predict how much would be spent? This has been done recently by Lansing, Kreinin and Maynes in analyzing debt [23]. The two-stage analysis has some advantages, particularly where some people consistently remain uninvolved (some people never borrow). We felt that the added complexity was not worth while here, and that whether or not a person made consumer investment expenditures in any one year was primarily a matter of the particular year (random), i.e., that those who spent nothing would be a curious mixture of a few people who never buy anything with those who merely happened to buy nothing that year. Most of the analysis is concerned with subgroup means. This is not merely a matter of convenience. We want to analyze peoples' normal expenditure levels, not those of a single year.

If there is a negative serial correlation over time for an individual's expenditures on these large lasting items of expenditure, i.e., years of spending alternating with years of repaying debts or rebuilding asset balances, then a homogeneous subgroup of people reporting one-year expenditures can be considered as made up of people at various stages in this time sequence, and the group means would be more stable than could be accounted for by the mere averaging process. Since clustering in the sample produces some opposite effects, it is difficult to demonstrate this effect. It would mean, however, that estimates of error variance based on individual cases would exaggerate the true errors of group means, and this might explain some of the low correlations one obtains using individual cases.

If we want to analyze consumer investment, and particularly to see what dynamic forces would tend to change it, we must recognize that in any sample of people, many of the differences in expenditures are

the simple result of differences in their situations (age, home owner-ship, etc.) and these constitute "nuisance variables" from the point of view of our final goal of investigating more dynamic factors. They are not uninteresting in themselves, however, and some of them change over time for the population as a whole so that they are useful in mak-ing longer-run predictions. In order to develop methods of eliminating these situational variables, we needed a large body of data, hence we took our data from seven Surveys of Consumer Finances. A subsidiary reason for taking seven years was the possibility of analyzing detailed changes over time for subgroups in the sample, after taking account of changes in the situational variables.

Why not expenditures rather than the expenditure-income ratio? After all, it is the aggregate dollar expenditure that is crucial in na-tional income analysis. There were several reasons:

1. There were some price and income changes during the period, and using a ratio removes the effect of these scale shifts, in so far as prices and incomes move together.

2. Consumer investment expenditures are remarkably linear with re-spect to income, and almost homogeneous (through the origin) varying from 8 to 10 per cent of income. This was not true in the United States before the war, nor is it true in England where durables are still a lux-ury; but in America these expenditures are now apparently a sort of "expansible necessity." They have Engel curves somewhere between necessities like food which show declining proportions, and luxuries like recreation, travel and education which take increasing proportions of income. This means that most of the income effect can be taken care of by simply using the ratio. The importance of income is obvious, but our goal was to see what *else* was important in explaining this type of expenditure. If income could do it all, even though it were partly a proxy variable for other things like age or education, we should be less concerned with these other effects.

3. In so far as heterogeneity of variance should be a statistical prob-lem (high-income people having more variability in their expenditures), using the ratio would tend to reduce the heterogeneity. Of course neither the amounts nor the ratios are normally distributed, since there are always a cluster of people with no expenditure (zeros) and a few with large amounts and large proportions of income. However, means of subgroups rapidly approach normality as the number of cases in each group increases.

Exclusions. We eliminated other sources of instability or heterogene-ity as follows: (1) Those with incomes over $100,000, expenditures over $10,000, or who spent more than 200 per cent of their income on consumer investments (1-3 per cent of the sample each year).

TABLE 1.—AVERAGE RATIO OF CONSUMER INVESTMENT EXPENDITURE TO DISPOSABLE INCOME BY HOME OWNER-SHIP AND FAMILY STATUS, 1947-56[1]
(Per Cent)

	1947	1948	1949	1950	1951[2]	1952	1953[3]	1954[4]	1955[4]	1956[4]
All Nonfarm Primaries[5]	12.1	13.6	15.7	16.2	12.6	13.7	14.5	14.1	16.0	15.0
Nonfarm Primary Renters—All	9.2	8.7	10.6	12.0	9.8	11.2	11.0	9.7	11.4	11.6
Single person, 18–44 years old	8.2	7.1	11.2	4.9	9.0	10.8	8.8	7.9	17.6	7.2
Single person, 45 or older but not unemployed or retired if over 55	2.9	2.0	4.4	3.8	2.5	6.1	3.5	6.0	9.1	6.6
Two or more people, 18–34, no children	17.8	15.2	14.0	18.7	19.9	17.9	15.7	16.6	17.4	25.5
Two or more people, 35–44, no children	11.0	10.2	9.6	17.3	11.2	15.5	7.4	7.7	19.0	14.4
Two or more people, 45–54, no children	6.5	5.7	9.0	13.5	9.9	9.1	7.8	5.7	9.8 }	
Two or more people, 55 or older but not unemployed or retired, no children	6.6	7.3	6.1	8.2	4.3	6.6	6.5	6.2	4.3 }	7.6
Two or more people, 13–34, children	11.6	13.1	13.9	14.0	12.2	13.9	14.7	15.9	14.7	14.7
Two or more people, 35–44, children	7.7	7.6	13.7	12.7	8.5	9.8	14.8	11.9	10.9	13.2
Two or more people, 45–54, children	5.3	4.5	5.8	13.0	10.2	6.1	9.8	8.0	10.1	7.3
Two or more people, 55 or over, but head not unemployed or retired, children	0.9	5.4	9.4	8.0	11.6	14.3	11.9	6.3	9.1	2.5
Head 55 or over and unemployed or retired (includes a few NA's on number of adults or of children)	2.2	3.4	0.7	2.7	2.3	3.5	6.3	2.5	6.9	2.8
Not ascertained on age, number of adults or number of children	19.0	5.8[6]	6.9	7.0	8.9	17.2	17.6[6]	10.4	8.2	9.0
Nonfarm Primary Home Owners—All	14.9	17.7	19.8	19.3	14.5	15.6	16.8	16.7	18.8	16.9
Single person, 18–44	13.2	12.1	11.1	33.4	18.0	16.0	18.3	20.1	17.3	9.3
Single, 45 or over but not unemployed or retired if over 55	13.5	22.8	21.7	20.1	13.9	15.8	18.5	17.0	18.3	14.0

Two or more people, 18–34, no children	30.0	22.9	25.5	19.1	25.7	18.5	22.1	18.2	24.9	16.9
Two or more people, 35–44, no children	16.8	17.1	20.5	20.4	8.9	18.5	20.0	22.2	17.1	17.0
Two or more people, 45–54, no children	12.4	15.9	20.2	22.1	17.5	17.5	19.7	15.0	19.0	
Two or more people, 55 or older but not unemployed or retired, no children	11.8	13.9	17.6	17.5	13.0	14.8	16.9	15.2	16.2	18.0
Two or more people, 18–34, children	17.6	20.9	21.6	19.4	13.5	16.8	18.1	18.0	19.3	16.4
Two or more people, 35–44, children	13.9	19.2	18.6	18.2	15.3	16.6	16.1	17.1	18.6	15.0
Two or more people, 45–54, children	14.8	20.2	19.1	19.7	14.7	12.5	12.6	13.8	16.9	14.3
Two or more people, 55 or over but not unemployed or retired, children	12.1	17.1	32.5	9.9	13.7	15.7	18.4	15.0	25.2	19.3
Head 55 or over and unemployed or retired (includes a few NA's on number of adults or children)	16.2	12.2	17.0	21.9	13.7	11.0	13.5	12.4	21.7	22.0
Not ascertained on age, number of adults, or number of children	17.3	11.8[6]	16.6	2.7[6]	12.3	22.2	6.8[6]	18.4	16.9	17.0
Total number of interviews used	2290	2355	2356	2417	2097	2319	2166	2381	2348	2466
Expected percentage for all nonfarm primaries[7]	14.3	14.3	14.5	14.5	14.5	14.5	14.5	14.6	14.6	14.8

[1] Each figure is an average of percentages, not the percentage of average expenditure to average income.
[2] Excludes borrowed funds used for additions or repairs.
[3] Better income schedule makes ratios for 1953 and later years look lower.
[4] Added after most of the analysis was done.
[5] Primary spending units are the main spending unit in each dwelling unit (house, apartment). Secondary units, unrelated to primary or related but with separate finances, are excluded.
[6] Based on fewer than 10 cases.
[7] Average percentages of Table 2 weighted by the proportion of sample in that group for that year.
Source: Surveys of Consumer Finances.

(2) Those for whom some component of expenditure was not ascertained (1-5½ per cent of spending units). (3) Related secondary spending units and a few primary units who neither owned a home nor rented, e.g., domestic servants, etc. (12.4-17.3 per cent of sample). The secondaries are mostly children living with parents or older parents living with their children, yet with enough income to maintain some autonomy. They use the primary's household equipment, and in editing questionnaires, aren't "allowed" to make additions and repairs. (4) Farm operators not already excluded (7-13 per cent of units), because of their entrepreneurial nature (a farmer's car is often used as a truck) and the fact that additions and repairs are often to farm buildings, particularly in early surveys when they were included in order to estimate saving. (5) Unrelated secondary spending units (roomers and boarders) (3 per cent). This leaves two-thirds of the population of spending units, who accounted for 70-80 per cent of aggregate consumer investment expenditures.

Income Linearity

Preliminary investigations seemed to show that the linearity in income was not quite perfect, that there was some humping in the middle-income ranges. One might think this was the result of upwardly mobile people trying to acquire status goods, i.e., a genuine nonlinear income effect. It turned out, however, that the humping was caused by people in the early stages of family life (young married people). To preserve the mobility hypothesis, one would have to assume that all young married people, and only they, are upwardly mobile. Once we took account of differences between families at various stages in the life cycle, the relationship of income to consumer investment expenditures became even more nearly linear and homogeneous.

Hence, we used proportions of disposable income spent on consumer investments for 16,000 spending units for the years 1947-1953. Our major purpose was to look at the effects of such dynamic variables as income changes, and attitudes and expectations. In order to do this, however, we had to take account of some situational variables as well as income levels. There were good reasons to expect that home ownership and stage in the family life cycle would be crucial. Renters do not pay for very many of the additions and repairs to their homes, and not for all the durables in them. They also tend to have less space and perhaps to be less interested in their homes than owners. Hence they may well spend less even on the furniture and appliances which they furnish themselves.

The stage of the family life cycle has an obvious importance for consumer investment expenditures. Each family that is formed has to accu-

mulate a car and a set of household goods, and if possible they tend to buy new ones rather than accept hand-me-downs. Previous data had already indicated the importance of this variable [24] [30]. Further-more, since the possible accumulations of durables by renters are limited, there was reason to believe that there would be different life-cycle pat-terns for home owners and renters.

IV. *Consumer Investment Related to Family Status, Home Ownership and Income*

Table 1 shows for each of ten years the proportions of income spent on consumer investment items by people with different situations as to housing, labor-force participation, and stage of the family life cycle.

TABLE 2.—AVERAGE PERCENTAGE OF DISPOSABLE INCOME SPENT ON CONSUMER INVESTMENTS BY HOME OWNERSHIP AND FAMILY STATUS, 1947–1953

Home Ownership	Marital Status	18–34	35–44	45–54	55 or Over	55 or Over & Unem-ployed or Retired	NA on Age or Family Status
Home Owners	Single	17		18		15	16
	Married, no children	23	18	18	16		
	Married, children	18	17	16	16		
Renters	Single	9		4		3	13
	Married, no children	17	12	9	7		
	Married, children	16	11	8	8		

Source: Table 1, 1947–53 data, 16,000 cases.

The differences are wide and persistent in spite of some year-to-year fluctuations. The differences hardly need a test of significance, and tests would be inappropriate, since this table represents the result of a search for the most important demographic variables related to con-sumer investment expenditures. Clearly, the effects of these variables must be removed or somehow taken into account in looking for the ef-fects of other more dynamic variables, if one is to avoid finding spur-ious correlations, or failing to find real ones.

Apart from our purposes, a number of other useful things can be done with these tables. One could use them to project the impact of demographic changes in the age or home-ownership patterns of the population. If one is interested in consumer credit or consumer budgets, one notes that the substantial proportion of income that goes to lumpy

TABLE 3.—AVERAGE DEVIATION FROM "NORMAL" EXPENDITURE-INCOME RATIOS, BY INCOME AND YEAR AND BY HOME OWNERSHIP[1]
(Per Cent of Income)

Income After Taxes	1947	1948	1949	1950	1951	1952	1953	1954[2]	1955[2]	1956[2]
Less than $1000	-2.2	+0.8	+3.5	-1.3	-2.2	-1.3	+1.4	+0.7	+4.2	-1.8
$1000–1999	-4.2	-2.1	+1.3	+2.1	-3.0	-1.3	+1.4	+1.8	+4.1	+5.4
$2000–2999	-2.3	-1.1	+2.0	+2.9	-2.7	+0.8	+1.8	+2.3	+4.0	+0.9
$3000–3999	-1.3	-0.6	+0.4	+2.1	-2.2	-0.3	+0.2	-0.8	+2.0	+2.4
$4000–4999	-1.6	+1.4	+0.2	+1.2	-2.0	-1.2	-0.6	-1.7	+0.3	-1.2
$5000–7499	+0.1	+0.4	+1.9	+3.0	+0.9	-1.4	-1.2	-0.4	+0.1	-0.6
$7500 and over	-3.5	-5.0	-2.3	+2.5	-2.1	-3.4	-2.5	-4.3	-3.8	-3.0
All Incomes	-2.2	-0.7	+1.2	+1.7	-1.9	-0.8	0.0	-0.5	+1.4	+0.2
All nonfarm primary owners	-2.2	+0.6	+2.8	+2.3	-2.5	-1.5	-0.2	-0.7	+1.9	0.0
All nonfarm primary renters	-2.2	-2.3	-0.7	+0.9	-1.2	0.0	+0.2	-0.2	+0.5	+0.4

[1] Nonfarm primary units only, renters and owners combined. For "normal" see Table 2.
[2] Added later.

and difficult-to-plan items helps explain the use of instalment credit, and the failure of most people to keep personal budgets.

In order to get a set of "norms" that were stable and not too much influenced by random fluctuations, we combined adjoining groups that were small in number and similar in their behavior. By combining the years 1947-1953 as well, we ended up with Table 2 which became the "normal." We then computed how much each individual spending unit each year spent above or below what one would have expected from this table. This left year-to-year variation in these residuals, as one can see in Table 3, where the over-all average residual from normal varied particularly during the Korean War and the boom of 1955 and more violently for owners than renters. Table 3 also indicates that the income linearity we had assumed in using ratios was a good working assumption except perhaps for those with incomes over $7,500. Had the investment category included homes, summer cottages, boats, and luxury-type durables, even this decline might have disappeared. The interesting differentials between home owners and renters suggest a number of explanations, such as the shortages of materials for additions and repairs in 1951, resistance to the high prices and the fact that home owners are older and more likely to have a stock of furniture and appliances.

The over-all average deviations for each year, combined with the over-all percentage actually spent, imply an "expected average percentage" for each year, which is given at the bottom of Table 1. It can also be computed directly by weighting the averages of Table 2 by the sample proportions in each year. Almost none of the year-to-year fluctuation in consumer investment spending appears to result from changes in the composition of the population, though there does appear to be a long-term trend toward more people in the groups which spend larger proportions of their incomes on consumer investment items.

One could think of the analysis so far as first taking account of income in a simple way that happens to be feasible, i.e., taking expenditure as a per cent of income after taxes. Then we take account of a complex pattern of variables: home ownership, family status, and labor force status, by computing residuals from the average proportions of income spent by these groups over a seven-year period. We have not computed any estimate of the proportionate reduction in unexplained variance in expenditures by using the ratio to income, though this could be done. The additional reduction in the unexplained variance of the ratios by taking deviations from the 24 group averages of Table 2 has been estimated roughly, using unweighted data for the single year 1953, as follows: If one asks what proportionate reduction in the error in estimating the expenditure-income ratio of an in-

dividual results from the use of Table 1 for 1953, the answer is 4 per cent. This figure can also be thought of as the squared correlation co-efficient which would result if one used a "dummy variable" (one or zero) for each of the 24 groups except one.[2]

One can interpret this partly to mean that income has removed most of the variation, partly that there are many other situational or personality variables that need to be taken into account before one really explains individual behavior, and partly that the use of expenditures for a single year makes the data for individuals a poor measure even of individual behavior.

One must remember, also, that individual data always provide lower correlation than grouped data, and even the .04 above involves a highly significant F ratio of 4.5 with 23 and 2142 degrees of freedom. On the other hand, none of these statistics is strictly valid, since no account is taken of clustering in the sample [5] [11] [18]. It is for this reason that we have concentrated in this article on estimates rather than on significance tests.

V. Consumer Investment Related to Other Variables

Having "taken account" of income and some of the other major variables of a situational or long-run nature, we now look at the residual deviations to see how they are related to more interesting dynamic factors such as changes in past or expected income, attitudes of consumers, and other things that might be expected to change even in the aggregate. We can also look for the effects of factors that pertain only to small subgroups of the population, e.g., membership in a minority race, or those who have had no education.[3] We use the deviations from the "norms" of Table 2, i.e., pooled residuals from what that table predicts, regrouped using the same general procedure as in Table 3.

Again it should be remembered that because of the complexities of the sample, no precise estimates of the sampling errors of these deviations has been computed. We pooled the variance within the home-ownership family-status subgroups for the one year 1953, and by adding 50 per cent to the variance (roughly 25 per cent to the sampling errors) to allow for losses through clustering, can estimate that for mean residuals a difference between two such means, each based on 100 cases, would have to be at least 6 per cent (300 cases each, 4 per cent) to be significant (at the 5 per cent level).

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[2] This 4 per cent reduction in variance is roughly equivalent to the multiple correlations of .24 and .19 reported by Klein between the proportion of income spent on durables and cars, on the one hand, and age and initial liquid asset-income ratio on the other [19].

[3] Space prohibits presenting here any, but the most interesting tables and a summary account of some analyses of variance which were used to assess significance and check for interaction effects.

Using the years 1947-1950, for which we had estimates of liquid assets at the beginning of the year, we ran a number of analyses of variance of the residuals, starting with one using 2 income levels (under and over $4,000), 2 levels of liquid assets (under and over $200), 4 income-change groups, 3 groups on expected income, and 4 years, a

TABLE 4.—MEAN RESIDUALS BY ASSETS, PAST INCOME-CHANGE, AND EXPECTED INCOME-CHANGE (1947–50)
(Per Cent of Income)

Expects Income to:	Past Income-Change[1]					All Past Income Changes (incl. NA's)
	Went Up a Lot	Went Up a Little	No Change	Went Down a Little	Went Down a Lot	
	Less than $200 in liquid assets, beginning of year					
Go up	+0.9	−2.8	−4.0	−4.0	+0.1	−2.3
Stay same	−0.8	−2.8	−3.2	−0.5	0.0	−2.3
Go down	+1.7	−3.6	−1.6	−0.1	−3.4[2]	−1.4
Don't know	−2.3	−1.8	−6.5	−4.6	+1.0	−3.3
	−0.1	−2.7	−3.6	−2.2	−0.4	−2.3 All low liquid assets
	$200 or more in liquid assets					
Go up	+3.0	0.0	+4.0	+5.6	+1.4	+2.3
Stay same	+2.4	+1.2	+0.3[3]	+1.2	+0.8	+0.9
Go down	+3.4	+1.9	0.0	+2.5	+1.7	+1.6
Don't know	+3.9	−2.1	+2.2	−1.5	+7.0	+1.1
	+2.9	+0.5	+1.2	+2.3	+2.4	+1.5 All high liquid assets
	Both liquid asset groups					
Go up	+2.3	−1.0	+1.2	+1.4	+1.0	+0.6
Stay same	+1.1	−0.2	−0.9	0.0	+0.6	−0.2
Go down	+2.7	+0.3	−0.5	+1.6	−0.3	+0.6
Don't know	+1.0	−2.0	−1.6	−2.9	+4.3	−0.9
	+1.8	−0.7	−0.5	+0.5	+1.3	+0.06 All[4]

[1] For the majority who gave both years' incomes in dollars, "a lot" is defined as 25 per cent or more and "a little" as 5–24 per cent. Changes of less than 5 per cent are "no change."
[2] Smallest number of interviews=36.
[3] Largest number of interviews=1128.
[4] Excludes those whose past income change was not ascertained.

total of 192 subgroups. Liquid assets, income-change, and year were significant at the 1 per cent level with F-ratios of 26, 4, and 5 but nothing else was.[4] In order to reduce the disparate frequencies within cells, we eliminated the high-income low-asset group and the same

[4] See Appendix on Statistical Procedures and Problems.

three variables appeared significant. Tables 4 and 5 show the nature of the effects, with more detail on income-change than we used in the variance analysis (the small group with substantial declines is kept separate in the table). The most interesting and important thing about these findings is the impact of income-change (Table 4). What the data indicate is that when people's incomes increase, they increase their expenditures on consumer investment items more than propor-

TABLE 5.—MEAN RESIDUALS BY ASSETS, INCOME AND YEAR
(Per Cent of Income)

		Liquid Assets, Beginning of Year		Both Asset Groups
		Under $200	$200 or More	
1947	Income under $4,000	−6.3	0.0	−2.5
	Income $4,000 or more	−1.8	−1.3	−1.4
	Both Income Groups	−6.0	−0.4	−2.2
1948	Income under $4,000	−3.6	+1.0	−1.0
	Income $4,000 or more	−2.0	+0.3	0.0
	Both Income Groups	−3.4	+0.8	−0.7
1949	Income under $4,000	−0.5	+3.6	+1.5
	Income $4,000 or more	+3.2	+1.4	+0.4
	Both Income Groups	+1.0	+2.8	+1.2
1950	Income under $4,000	−0.6	+4.4	+1.9
	Income $4,000 or more	+0.6	+1.4	+1.4
	Both Income Groups	−0.5	+3.2	+1.7

tionately and that when their incomes decline, they decrease their expenditures on these items *less than proportionately*.

To use total consumer expenditures (including consumer investments), as a measure of "permanent income," as some writers suggest, rather than income would clearly be unwise where this sort of asymmetry exists. It may be true that one could define a variable called "permanent consumption" which excluded such variable items, and argue that it is a better measure of economic status, but it might be difficult to specify and measure such a concept. Returning to the categorization of consumers' uses of their income suggested at the beginning of this article, we are suggesting that liquid saving and certain forms of

consumer expenditures are both variable over time, but that this varia-
bility is of a curious sort. There is first the negative serial correlation
and the competition between them, and second, variations that result
from changed income and changed expectations about future income.
What the data seem to show is that when income falls, there is some
tendency to carry on with plans to buy certain durables, though not
completely, perhaps because the drop is considered temporary. When
income rises, there appears to be a tendency to hasten the purchase of
these consumer investment items, perhaps because the increase is con-
sidered permanent. It is also possible, however, and consistent with the
data, to conclude that even those who have a temporary increase in in-
come use it for these consumer investment items as a method of getting
something lasting out of the extra money rather than let it go down the
drain and set new consumption standards.

This does not mean that uncertainty per se would increase consumer
spending on these items, since optimism and confidence about the
future also affect it, but it does mean that changes in income may stim-
ulate expenditures for the fortunate more than such changes depress
expenditures for the unfortunate.

It is not surprising that expected income for the next year had little
relation to expenditures during the past year. When we come to a
reinterview sample where we have income expectations at the begin-
ning of the year for which we analyze spending, a relationship does
appear (Table 9, below).

The asset effect—people with more liquid assets at the beginning of
the year tending to spend more—tends to be stronger for low-income
people, as one might expect (Table 5). There is no good way to test
the possibility that this may have been a temporary postwar phenome-
non which has since disappeared. Using end-of-year liquid assets for
1953 however we found a *negative* relation between assets and ex-
penditures that continued up the asset scale clearly beyond the impact
that one year's expenditures could have had on the assets remaining
at the end of the year. If there exists a short-run oscillation between
expenditures on consumer investment items and liquid savings, such a
correlation could exist without proving that the assets themselves affect
expenditures. As more and more consumers use instalment credit rather
than liquid asset accumulations, even this spurious correlation would
tend to disappear.

Even though we are dealing with residuals from a pattern of ex-
penditures over the family cycle, household formation is so important
in consumer investment expenditures that we decided to see whether
recency of marriage would still have some additional effect. Table 6
shows that it does. On the hypothesis that these expenditures are less

TABLE 6.—AVERAGE DEVIATIONS BY NUMBER OF YEARS MARRIED[1]
(Per Cent of Income)

Number of Years Married	1949	1950	1951	1952	1953	All Five Years	Per Cent of Ana-lyzed Population
One or less	+2.5 (72)	+3.8 (47)	+4.5 (58)	+5.2 (70)	+4.4 (57)	+4.0	2.7
2	+3.7 (78)	+2.6 (73)	−2.3 (54)	+4.9 (77)	+4.1 (64)	+2.8	3.1
3	+2.4 (90)	+1.9 (76)	−2.6 (53)	−1.9 (55)	+7.3 (55)	+1.6	2.9
4	0.0 (98)	+0.7 (77)	−2.4 (73)	−1.6 (71)	−0.4 (51)	−0.7	3.5
5–9	−0.4 (297)	+0.5 (332)	−1.4 (284)	−3.0 (352)	−0.5 (338)	−1.0	14.3
10–20	+1.1 (549)	+2.9 (581)	−3.8 (530)	+0.2 (559)	−1.2 (571)	−0.2	23.9
20 or more	+2.4 (731)	+2.5 (752)	−0.4 (638)	−1.9 (677)	+0.4 (614)	+0.5	29.0
Not married	0.0 (429)	+0.2 (450)	−2.8 (387)	−0.5 (429)	0.0 (411)	−0.8	19.7
Not ascertained	−4.4 (12)	−10.0 (19)	−4.3 (20)	−1.6 (29)	+2.0 (5)	−4.0	0.9
							100.0
All	+1.2	+1.7	−1.9	−0.8	0.0	0.0	

[1] Note that these are deviations from norms which already take account of home owner-ship and stage in the family life cycle, including a separation of young married people by age of head and whether or not they have children.
( ) = Number of cases.

discretionary for people who are outfitting a household for the first time, it is interesting to note that those married one year or less did not seem to cut their spending at all during the resistance to high prices and shortages of 1951. Furthermore, those married only two years re-entered the market in 1952, those married only three years in 1953, while the rest appeared to be resisting the high prices through 1953.

Table 7 indicates, though the frequencies are very small, that educa-

TABLE 7.—AVERAGE DEVIATIONS, 1953 ONLY, BY EDUCATION AND RACE[1]
(Per Cent of Income)

Education of Head of Spending Unit	Race		All[2]
	White	Negro	
None	+8.9 (24)	+7.5 (10)	+8.5 (34)
1–8 years	−0.2 (608)	+5.2 (71)	+0.5 (690)
9–12 years	−1.0 (853)	+4.5 (35)	0.0 (899)
College (no degree)	−0.8 (252)	+2.2 (4)	−0.8 (259)
College graduate	−1.6 (259)	+24.2 (3)	−1.3 (267)
All[3]	−0.6 (2010)	+5.6 (126)	0.0 (2166)

[1] The over-all average for 1953 was 0.0 so no adjustment is required for the particular year. Number of interviews is small, and is given in parentheses beneath each average.
[2] Includes a few "other."
[3] Includes a few not ascertained as to education.

TABLE 8.—1952 RESIDUALS BY MARITAL STATUS AND INCOME CHANGE
(Per Cent of Income)

Marital Status	Income Change					
	Up 25 Per Cent or More	Up 5–24 Per Cent	Changed Less Than 5 Per Cent	Down 5–24 Per Cent	Down 25 Per Cent or More	All
Not Married	−0.8 (50)	+1.2 (147)	−1.0 (145)	−2.7 (43)	+0.4 (26)	−0.3
Married 4 Years or More	+0.2 (276)	−1.4 (577)	−2.3 (494)	−1.9 (207)	−1.0 (93)	−1.4
Married 1–3 Years	+7.4 (55)	−0.1 (66)	−2.3 (47)	+9.4 (18)	+5.9 (11)	+2.8
All	+1.0	−0.7	−2.0	−1.1	−0.1	−0.8

( ) = Number of cases.

tion and race have some effect. The negative residuals for the college graduates, however, may well only be a reflection of their very high incomes and the departure of the spending-income relation from linearity at these incomes.

If these consumer investment items are actually "status goods," socio-economic status should affect expenditure on them. Such status is generally indicated by mixtures of income, education and occupation, often with emphasis on the last of these. For the single year 1953, for which we looked at residuals by occupation, the lowest-status occupations had the largest positive residuals. Again the imperfect removal of the income effect may have given the professional people and the managers their negative residuals, but it could hardly explain why the residuals were 5.1 per cent for unskilled and service workers, —0.7 for skilled and semiskilled workers, and —1.3 for clerical and sales people.

Further evidence of the urgency of these expenditures for recently married people is given by Table 8, where resistance to a reduction of expenditures when income falls appears to be stronger for those married three years or less.

VI. *Reinterview Results*

Part of the 1953 Survey involved reinterviews with people interviewed in early 1952. This allows us to make use of attitudes and expectations expressed before the expenditure year. It would of course be better to have three interviews so that one could relate changes in attitudes and expectations to changes in subsequent spending behavior, but we can look at the available data remembering that personality and situational differences will create a good deal of "noise" in our data. It is also a troublesome fact that people who moved were not reinterviewed, so that the reinterview "sample" includes fewer young people and no movers and hence fewer of the large spenders on consumer investments.

However, looking at differences *within* the reinterview group is still revealing. Table 9 shows that feeling better off financially than a year ago, and expecting to be making more a year hence are both positively associated with spending on consumer investments. The very low expenditures of those who did not know or say whether they were better or worse off than before suggests (though the frequencies are very small) that these people may have been so badly off that they wouldn't admit it, perhaps even to themselves. On the other hand, those who wouldn't commit themselves as to a year hence behaved more like the optimists. With the paucity of cases, however, these results are not statistically significant. However, those with no changes clearly spent the least.

Table 10, finally, combines the reported attitude at the beginning

TABLE 9.—MEAN RESIDUALS BY 1952 REINTERVIEWS, BY TWO
ATTITUDES AT BEGINNING OF YEAR[1]
(Per Cent of Income)

Expected in Early 1952 to be Making a "Year from Now":	Better or Worse Off in Early 1952				
	Better Off	Same	Worse Off	Didn't Know or Say	All
More	+0.5 (104)	−2.2 (79)	−2.1 (78)	−9.2 (7)	−1.5
Same	−1.8 (74)	−5.4)92)	−3.4 (83)	−11.1 (7)	−3.9
Less	−6.5 (15)	−6.4 (25)	−5.6 (31)	−14.5 (2)	−6.4
Didn't know or say	+1.2 (49)	−4.0 (71)	+0.4 (57)	−8.9 (11)	−1.4
All	−0.4	−4.1	−2.3	−9.9	−2.6

[1] Attitudes were measured in early 1952; expenditures, incomes, and residuals were computed for the year 1952 on the basis of reinterview reports in early 1953. Numbers in parentheses are numbers of interviews.

TABLE 10.—MEAN RESIDUALS, 1952 REINTERVIEWS, BY EARLY 1952 "BETTER OR
WORSE OFF" AND SUBSEQUENT CHANGE IN INCOME FROM 1951–1952
(Per Cent of Income)

Change in Income During 1952	Better or Worse Off—Early 1952				
	Better Off	Same	Worse Off	Didn't Know or Say	All
Up 25% or more	−2.1 (54)	−8.1 (24)	−5.5 (24)	−8.1 (8)	−4.7
Up 5–24%	+1.2 (90)	−5.0 (98)	+2.5 (74)	−9.9 (11)	−1.1
Changed less than 5%	+1.7 (59)	−5.1 (97)	−2.5 (91)	−12.2 (5)	−2.8
Down 5–24%	−5.9 (24)	−4.3 (31)	−5.0 (38)	−11.1 (2)	−5.3
Down 25% or more	−6.1 (11)	−10.3 (10)	−8.2 (12)	−9.0 (1)	−1.8
All (including 22 NA's)	−0.4	−4.1	−2.5	−9.9	−2.6

Numbers in parentheses are numbers of interviews.[*]

of 1952 and the later reported income change during 1952. For this sample, which excludes some of the young people, the effect of income changes is not so marked, and indeed is different for large increases and small decreases.

VII. *Special Analyses of 1954 and 1955 Data*

We add here Table 11, using data from the 1955 Survey for 1954. This was run later as a check on our earlier results, and also to make use of a simpler method than pooled residuals for dealing with family-status and home-ownership differences. The resistance to contracting expenditures under the impact of income declines seems to be greatest for young home owners who expect their incomes to go up. However, the lack of symmetry in the effect of income changes is again marked.

In some unpublished studies by Jacob Cohen and the author, the population was divided according to replies to a question: "How do you handle your finances when you make a large purchase for your home?" Answers were easily classified in the following groups: (1) I pay cash. (2) I use installment credit, or borrow. (3) It depends; cash or credit depending on whether we have the money. (4) We never buy any large items. (5) Not ascertained. We found no association with change in liquid assets, but some association with variability in liquid assets, e.g., with the proportion whose liquid assets went up or down. The clear interpretation was that people who use credit don't accumulate liquid assets and use them to buy large items, but pay out of income.

This left unanswered the question whether those who use credit might not spend a larger proportion of their income on large lumpy items, because they had discovered a convenient way to do it, or because they were tempted by credit to spend more on them. It was important to isolate any effects of willingness to use credit from income, home ownership, stage in the family life cycle and other "nuisance" variables. For this purpose, the 1954 average residuals were tabulated for those who did or did not use credit, together with the average proportions of income actually spent (Table 12). Clearly, the use of credit is not associated with a significantly larger proportion of income being spent on consumer investment items.

One of the reasons for not using multiple regression in the first place had been the problem of scaling qualitative variables and dealing with interactions. The pattern of proportions spent averaged over 7 years, however, provides a method of introducing as a single variable these "expected" proportions. Taking the data for 1954, we found the following:

$$E = -.01 +.99E' -.41H +.07M +.01N; \quad R^2 = .038$$
$$ (.16) \quad (.10) \quad (.13) \quad (.09) \quad (.09) \quad N = 2381$$

TABLE 11.—AVERAGE INCOME AND PROPORTION SPENT ON CONSUMER INVESTMENTS, 1954, BY FAMILY STATUS, INCOME CHANGE, AND EXPECTED INCOME

Expect Income To:	(Compared with previous year) Last Year, Income Was²	Married Renters Aged 18–44		Renters¹ 45 or Over or Single		Home Owners 18–44 Years Old		Home Owners¹ 45 or Older	
		Per Cent of Income Spent	Average Disposable Income	Per Cent of Income Spent	Average Disposable Income	Per Cent of Income Spent	Average Disposable Income	Per Cent of Income Spent	Average Disposable Income
Go Up	Much higher	13.1	$4975	9.4	$3224	18.2	$6584	9.6	$8874
	Higher	12.8	4745	7.3	4230	18.3	6017	12.4	5978
	Same	9.5	4424	5.4	3459	14.8	5158	20.7	4507
	Lower	13.0	4584	4.6	3738	23.1	4682	16.5	4462
	Much lower	15.9	2734	7.0	2602	28.2	3768	19.1	2045
Stay Same	Much higher	14.7	4899	4.1	4272	16.9	5130	31.8	3859
	Higher	12.5	4321	4.1	3053	19.0	5398	13.0	4286
	Same	7.5	3935	4.5	2638	18.6	4947	15.1	3819
	Lower	7.4	4305	8.0	3408	24.1	4778	15.0	4088
	Much lower	19.7³	3840	3.4³	1783	15.7³	3261	13.2	3067
Go Down, or Don't Know	Much higher	13.5	3796	14.7	4105	14.3	7634	23.7	2973
	Higher	20.5	4601	3.4	4239	15.1	5624	16.2	4288
	Same	17.3	3959	6.3	2159	10.2	4623	15.4	2746
	Lower	8.2	3846	2.3	3557	17.6	4187	19.1	3424
	Much lower	22.8	2985	9.0	1542	24.3	2906	8.9	2582

¹ Includes unemployed or retired but not N.A. on age or family status.
² Excludes not ascertained on income change.
³ Fewer than 10 cases.

where E is proportion of income spent on consumer investments.

   E' is proportion expected on the basis of age, home ownership, family status, etc., from Table 2.

   H is a "dummy variable" equal to 1 for those with incomes of $7,500 or over and zero for everyone else.

   M is a "dummy variable" equal to 1 for those who have moved (lived in their present house for less than two years).

   N is a "dummy variable" equal to 1 for units with two or more income receivers.

   Numbers in parenthesis are the result of computing the standard errors of the regression coefficients, in spite of the fact that these errors do not apply to a clustered sample like this one.

Partly because of the mixture of those who did and those who did not spend anything last year, but mostly because of the basic variability of

TABLE 12.—EXPECTED AND ACTUAL PROPORTIONS OF INCOME AFTER FEDERAL INCOME TAXES SPENT ON CONSUMER INVESTMENT ITEMS, BY WHETHER SPENDING UNIT USES CREDIT

	Expected	Actual	Deviation (Mean Residual)
Cash	14.8%	14.1%	−0.7%
Credit	14.6	14.3	−0.3
Depends	15.7	17.0	1.3
Never buy	9.9	7.0	−2.9
Not ascertained	13.6	14.4	0.8

consumer behavior and the use of ratios which remove most of the income effect, the correlation is low. Of the 3.8 per cent of the variance of E explained by this regression, the expected E' accounts for 3.6 per cent. In some other regression analysis using subgroups who had not had and did not expect a change in income, we found slightly higher correlations, but removing those who spent nothing at all from the sample did not improve the correlation.

   Why should the differences between the home-ownership family-status groups be so large and so persistent over time and yet explain so little of the individual variance in a single year? It may be that there is nothing here but the stability of averages, but there may also be a negative intraclass correlation resulting from the use of one-year expenditure data and a propensity to alternate between spending a lot one year and little or nothing the next. A difficulty in investigating this problem of variability versus repetitiousness of behavior is that there are strong positive serial correlations in many of the explanatory variables (income, age, whether owns home, marital status) so that a gross measure of autocorrelation would not be sufficient. Only an auto-

TABLE 13.—EXPECTED AND ACTUAL PROPORTION OF DISPOSABLE INCOME SPENT ON CONSUMER INVESTMENT ITEMS, BY INCOME AND HOME OWNERSHIP, 1955

(Per Cent of Income)

Income After Federal Income Taxes	Home Owners			Renters			All Primaries			Average Difference 1947–1953
	Actual	Expected	Difference	Actual	Expected	Difference	Actual	Expected	Difference	
Less than $1,000	23.4	16.7	6.7	7.4	6.9	0.5	16.9	12.7	4.2	-0.2
$1,000–1,999	23.2	16.1	7.1	8.9	7.6	1.3	15.8	11.7	4.1	-0.9
2,000–2,999	21.9	17.6	4.3	13.0	9.3	3.7	18.0	14.0	4.0	0.1
3,000–3,999	19.2	16.5	2.7	12.5	11.3	1.2	16.2	14.2	2.0	-0.2
4,000–4,999	18.3	18.7	-0.4	12.3	11.0	1.3	16.0	15.7	0.3	-0.4
5,000–7,499	18.5	18.5	0.0	13.8	9.8	4.0	15.8	15.7	0.1	0.3
7,500 and over	15.5	19.8	-4.3	8.0	8.8	-0.8	13.9	17.7	-3.8	-3.0

correlation study of residuals would be revealing and that has not yet been done.

The data for 1955 became available after much of the above was written, but since 1955 was a boom year for consumer expenditures, particularly on cars and durables, it is interesting to look at the actual and expected proportions of income spent for home owners and renters at different income levels (Table 13). Clearly it was the low-income home owners and the middle-income renters who spent more in 1955 than one would have expected from the 1947-1953 pattern. The negative residual for the top-income group was only slightly larger than the 1947-53 average.

This table illustrates a use of norms such as developed for this article in the assessment of dynamic changes. It is hoped to use them further, particularly in reinterview studies, in order to assess the impact of changes in individual financial conditions and attitudes on consumer investment expenditures.

APPENDIX ON STATISTICAL PROCEDURE AND PROBLEMS

It is customary in statistical work to set up a stochastic model and then devise estimating and testing procedures appropriate to it, e.g., by maximum likelihood methods. This is particularly important where one is reasonably sure of the theory and the specifications of the variables, and is concerned with unbiased estimates of the effects of specific variables in a system. Sometimes where it is difficult to set up a "just identified" system one derives from the equations a single predicting equation, arguing that the results are interesting and useful even if they cannot be translated back into the parameters of the original model.

In our investigations, however, there are a number of reasons why we took neither of these approaches: (1) We are not sure exactly which measures from the data correspond best to the factors of the theory, and there are often a number of things we want to try out. Even with modern machinery, it is something of a problem to compute very many solutions to multiple regressions in many variables. (2) Most of the explanatory variables are not quantitative, and although one can approximate any grouping by a series of dummy (1-0) variables, this expands the number of variables beyond the capacity of the machinery very rapidly. (3) There are reasons to believe that interaction effects exist and when we combine these with qualitative variables, the number of dummy variables required becomes prohibitive. (4) The clustering of cases at 0 (no expenditures) causes statistical problems, the solution to which is complex. Recently, James Tobin has provided methods for dealing with the problem of a truncated dependent variable in regression analysis [33].

Another possibility is to compute regressions for subgroups of the sample, and compare the coefficients, in a somewhat flexible version of covariance analysis. Tobin has done this in a recent study of debt [32]. If one puts all the measured variables that don't interact with one another into the regres-

sions, and looks for interactions and for effects of nonmeasurable character-
istics by examining differences between subgroups, this is a rather efficient
procedure. One must, however, make initial assumptions as to what interac-
tions are likely, and which characteristics are to be approximated by some
simple scale or set of 1-0 dummy variables.

The methods we have used minimize many of these problems. We need not
quantify explanatory categories, nor make so many assumptions about the
absence of interaction effects. Since we use averages of subgroups, there is
little truncation of these averages—in any one year a few people in almost
any subgroup will spend something on consumer investment items. Indeed,
there is probably a negative intraclass correlation within subgroups.

The sequential method we used, first taking some major explanatory
variables and then examining residuals, is less general than a multivariate
approach that considers all variables jointly. It is, however, a very flexible
procedure and allows one to try numerous potential explanatory variables in
an efficient and conservative way, i.e., the procedure underestimates the im-
portance of the secondary variables.

In an area of investigation where the direction of causation can be specified,
it is sometimes true that we may want to know the *gross* effect of a variable,
including its effects indirectly through influencing other variables. Suppose,
for instance, that age (A), liquid assets (L) and the proportion of income
spent on consumer investments (E) are related in the following manner
(arrows indicating direction of influence):
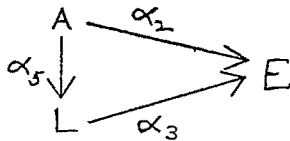
We could form a model of this set of relations as follows:

(1) $\qquad\qquad E = \alpha_1 + \alpha_2 A + \alpha_3 L + u_1$
(2) $\qquad\qquad L = \alpha_4 + \alpha_5 A + u_2$

where the u's are randomly distributed disturbances with zero means. Sub-
stituting, we have:

(3) $\qquad E = \alpha_1 + \alpha_2 A + \alpha_3 (\alpha_4 + \alpha_5 A + u_2) + u_1$
$\qquad\qquad = (\alpha_1 + \alpha_3\alpha_4) + (\alpha_2 + \alpha_3\alpha_5)A + \alpha_3 u_2 + u_1$

and the term $\alpha_2 + \alpha_3\alpha_5$ can be regarded as the gross age effect. If we use
equation (3) as a computing equation and relate the residuals thus developed
to liquid assets we have a regression coefficient: $\alpha_3(1 - r_{AL}^2)$ of liquid assets
on these residuals. This means that by such a procedure we would be under-
estimating the effect of liquid assets on expenditures in so far as there is a
positive correlation between age and liquid assets.

Measurement and definition problems in analyzing survey data are such
that lack of apparent relation is seldom conclusive anyway. Hence we are
seldom in danger of denying a relation which exists. As against the opposite

danger of positing a relation which does not really exist, the procedure of analyzing residuals appears to be conservative.

This is analogous to what we actually did in this study, except that the residuals were not from regressions but from a complex pattern of averages of subgroups which allowed us to take account of interaction effects and non-linearities in the effects of some factors as well as to avoid the problem of scaling qualitative explanatory factors. That is, our analysis is analogous to running a multiple correlation between the expenditure-income ratio and the following variables: home ownership, age, marital status, presence of children, whether head is in the labor force, and interactions of these variables, and then correlating the residuals with income change, expected income, etc. Multiple correlation would have forced a smoothing of some relations or a preliminary estimating of scales and would have eliminated the possibility of finding interaction effects, but both methods face other problems from the nature of the dependent variable, and from the fact that the data come from a multistage clustered sample.

This last point needs emphasis. Like any probability sample, the sample for the Survey of Consumer Finances is multistage, that is, a sample of primary sampling units (roughly counties) is selected, then a sample of cities and open country areas within these, then a sample of blocks within cities and dwellings within blocks, etc. In so far as people in the same county or city block or dwelling are more like one another than like other people, this clustering of the sample means that a sample of 3,000 is *not* 3,000 independent random observations. Using variability within the sample to estimate sampling errors tends to underestimate them, and by different amounts depending on the particular variable being estimated [18]. The sample is also stratified, which reduces sampling errors, and oversampled in the high-rent dwellings, which reduces some sampling errors and increases others. Using grouped data as we do and treating group means as single observations is a conservative procedure in this case. Work is in progress at Survey Research Center in estimating sampling errors of derived numbers such as the deviations we use, and preliminary results indicate that they do not differ much from sampling errors of the original data.

As for the variance analyses, using grouped data only partly avoids the clustering problem. The complex reason for this is that the small subgroups we used in computing interactions as estimates of the error variance have less losses of sample efficiency from clustering, hence less between-group variance than the larger groups used in estimating main effects. The F-ratios would be inflated by this, but probably never more than doubled. They are, on the other hand, underestimated if a high-order interaction actually exists. Another statistical problem remains: the literature on analysis of variance with dis-proportionate cell frequencies provides a number of alternative methods of analysis. They vary all the way from a complete least-squares solution (which runs back into the clustering problem) to analysis of cell means as though they were individual items (which runs into increased problems of hetero-geneity of variance because the means are based on different numbers of cases) [3] [4] [31] [34].

The basic problem is the "robustness" of the tests of significance with departures from the assumptions on which they were developed. Some recent writers have argued that the analysis of variance is quite robust in this sense, and that a simple analysis of unweighted cell means generally gives much the same results as more complex methods [1] [2]. It is this simple analysis which we used. It remains true, however, that heterogeneity of variance, which is intensified by using means based on varying n's, makes the F-tests somewhat less conservative so that higher significance levels should be required.

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DO WE HAVE A NEW INDUSTRIAL FEUDALISM?

By ARTHUR M. ROSS*

There are fashions in problems as well as hats; and when any given problem is solved, an equal and opposite problem generally emerges. The matter of labor mobility is a good case in point. A generation ago, "wasteful and destructive turnover" was seen as a primary evil. Today it is feared that workers have become badly immobilized. While "turnover" may still be an ugly word, "mobility" has taken on a favorable connotation, something like "alkalinity"; and while "stabilization" is still a virtue, "immobilization" is a vice.

It is said that seniority systems, health and welfare plans and negotiated pensions have chained the worker to his job; that the adaptability and flexibility of the labor force are being sacrificed; and that a new industrial feudalism is being built. The crux of the problem, it is held, is that the worker can no longer afford to quit his job.

Proponents of this view emphasize the recorded decline in quit rates since the second world war. For example, Joseph Shister has observed that "at no time since 1929 has voluntary mobility been as high as, say, in the 'twenties of this century." He concedes that the low turnover rates of the depression were understandable, but expresses surprise at the subsequent failure to regain the levels of the 'twenties. By way of explanation he states:

> When we turn . . . to voluntary horizontal mobility [i.e., quits], we find the role of trade-unionism shifting from a negligible to a paramount influence. Seniority in promotions and layoffs has reduced the amount of voluntary mobility for reasons which have been stressed again and again. . . . The negotiated pension plans in many units will serve as another obstacle to voluntary mobility by organized workers [22, p. 44].

Comments by Paul F. Brissenden, Ewan Clague and Gladys L. Palmer are essentially to the same effect, and are interlaced with concern over the supposed immobilization of the work force [5, pp. 767-68] [7, pp. vii-ix] [15, pp. 114, 116].[1]

[1] Compare Frank Tannenbaum's exultant conclusions in his highly romantic treatment of trade unionism as a syndicalist "counter-revolution": "The mobility of labor has been

If it were true that the worker can no longer afford to quit his job, grounds for concern should indeed be voiced. First, a serious issue is raised by the image of an employer who is too benevolent. The all-embracing institution which takes care of every need cannot be made to fit our traditions of self-reliance and free association. Voluntary servitude can be as degrading as the involuntary kind [13]. Second, if quitting one's job were so expensive as to be unthinkable, many desirable personal readjustments would be prevented. Some people *should* quit their jobs to find more congenial kinds of work, more agreeable supervisors and fellow workers, and better advancement opportunities. Third, a serious economic problem would indeed be created if manpower resources were to become inflexible and immobile. In our dynamic economy, a great deal of movement between occupations, between establishments in the same industry, between industries and between geographical areas is required. The question of why workers decide to move is the subject of much research and debate [14, ch. 3, 5] [18, ch. 4, 5] [20] [19, Pt. 1] and many other publications, but this much can be said: up to the present, the American labor force has been sufficiently fluid to fill the changing needs of employers. Recall, for example, the vast occupational, industrial and geographical movements of personnel during the second world war, when wage rates could not be used to attract manpower because of economic controls.

Therefore, it becomes important to examine the proposition that our labor force is being immobilized by the attractions of seniority and negotiated fringe benefits. The basic conclusion of this article is that the weight of evidence is strongly to the contrary. While some of the premises of the immobilization theory are valid, they do not add up to the stated conclusion. A long-term decline in the quit rate has occurred, but not for the stipulated reasons. Most employees who quit are too recently employed to have acquired substantial seniority or pension credits; employees with more than a few years of service have always had low quit rates; and the decline in voluntary turnover has been concentrated among junior rather than senior employees. Seniority systems have served to stabilize workers by bridging over their trial-and-error period of employment rather than by tying them to their jobs. However there are exceptions to any rule. Doubtless many individuals are influenced by pension rights, etc., when forced to make close decisions, but not so many as to alter the general picture of labor-market operation.

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possible because the worker had no investment in the industry in which he worked. . . . The industry will now assume responsibility for providing his family with medical care, maternity insurance, sickness, old-age and retirement pensions, vacations with pay, and innumerable other services. . . . What this really means is that the worker is changing a contract terminable at will to a contract terminable only at death" [24, pp. 181-82].

I. *Variations in the Quit Rates, 1910-56*

Movements of quit rates in American industry are shown in the last column of Table 1. (Other ratios in Table 1 will be used as measures of work opportunity in Charts 1 and 2 below. The sources and limitations of American labor turnover statistics are discussed in Appendix A.) A preliminary reconnaissance of the quit rates reveals two overriding influences which produce a great deal of the variation. One is the business cycle; the other is the characteristic dislocation and readjustment of war and postwar years. The first accounts for the sharp rise in 1913 and the decline in 1914-15; the sharp decline in 1921; the sustained low level of the 1930's, with a slight recovery in 1936-37; the drop in 1949 and again in 1954. The second explains the extraordinarily high quit rates of 1917-20 and 1942-47. Cyclical variations obviously reflect changes in the opportunity to shift from one employer to another. Wartime and postwar increases reflect both propensity and opportunity. The extra workers in the labor force are not well adjusted to employment and not strongly attached to any particular establishment, and of course in wartime disorganization of work groups, family situations and personal lives is prevalent. The resulting propensity to move creates more opportunity of itself, above and beyond that resulting from the tight labor market of wartime periods.[2]

It follows that significant conclusions about long-term movements in the quit rate cannot be drawn until cyclical variations in work opportunity have been discounted. It also follows that significant comparisons cannot be made except between periods which are relatively similar in their economic context. It proves little, for example, to show that quit rates in the 1950's have been higher than in the 1930's or lower than in the 1940's.

With these initial observations in mind, we can now examine the relationship between opportunity to move and quit rates somewhat more systematically. Alternative measures of work opportunity are incorporated in Charts 1 and 2. These are two approaches to the same problem, the second being more precise. Therefore it should not be thought that 70 per cent of the variation in quit rates is explained by Chart 1, and another 85 per cent by Chart 2.

In Chart 1 the average monthly quit rate is plotted against the rate of unemployment (the average number of unemployed as a percentage of the civilian labor force). Two measurements have been calculated. For 1910-56, the explained variance is 39.8 per cent. Turnover

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[2] Quit rates did not rise very much in 1951-53, during the Korean war. This is explained by the fact that the Korean war involved very little strain on the economy or the labor force. For the same reasons, the price level advanced only about 15 per cent despite the absence of effective price control.

TABLE 1.—UNEMPLOYMENT AS PER CENT OF CIVILIAN LABOR FORCE, MANUFACTURING EM-
PLOYMENT AS PER CENT OF NONAGRICULTURAL LABOR FORCE, AND
AVERAGE MONTHLY QUIT RATE IN MANUFACTURING, 1910–56

Year	Unemployment as Per Cent of Civilian Labor Force	Manufacturing Employment as Per Cent of Nonagricultural Labor Force	Average Monthly Quit Rate in Manufacturing
1910	3.3	39.8	5.1
11	4.0	37.0	5.3
12	4.7	35.8	5.8
13	2.8	36.6	8.8
14	6.7	34.3	3.3
15	8.8	33.8	2.8
16	4.2	37.8	n.a.
17	4.4	41.9	
18	1.4	42.9	12.0
19	2.3	37.9	5.8
1920	4.0	36.6	8.4
21	11.9	27.9	2.2
22	7.6	31.2	4.3
23	3.2	34.4	6.2
24	5.5	31.5	2.7
25	4.0	32.3	3.1
26	1.9	32.9	2.9
27	4.1	31.2	2.1
28	4.4	30.8	2.2
29	3.2	32.3	2.7
1930	8.8	28.1	1.6
31	15.9	23.3	0.9
32	23.6	19.2	0.7
33	24.9	20.0	0.9
34	21.7	22.5	0.9
35	20.1	23.8	0.9
36	17.0	25.5	1.1
37	14.3	27.6	1.3
38	19.0	23.5	0.6
39	17.2	25.3	0.8
1940	14.6	26.8	0.9
41	9.9	31.1	2.0
42	4.7	35.5	3.8
43	1.9	40.3	5.2
44	1.2	40.5	5.1
45	1.9	37.3	5.0
46	3.9	33.2	4.3
47	3.6	33.5	3.4
48	3.3	32.9	2.8
49	5.4	30.4	1.5
1950	4.9	31.3	1.9
51	2.9	32.7	2.4
52	2.6	32.7	2.3
53	2.5	33.6	2.3
54	5.0	31.0	1.1
55	4.0	31.4	1.6
56	3.8	31.1	1.6

Sources: See next page.
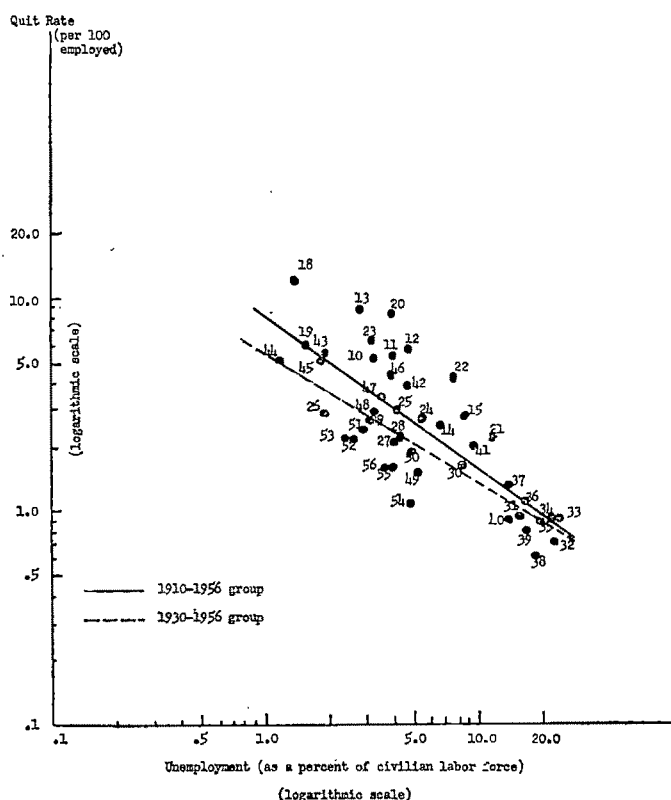
CHART 1. QUIT RATE COMPARED WITH UNEMPLOYMENT RATIO,
ANNUAL AVERAGES, 1910-1956

Sources:

1. *Civilian labor force.* 1910–1918: computed from S. Lebergott, "Annual Estimates of Unemployment in the United States, 1900–1954," *The Measurement and Behavior of Unemployment*, Nat. Bur. Econ. Research, New York 1957, pp. 215–15. 1919–1928: estimates of employment from J. P. Herring, *Labor Force. Employment and Unemployment*, Seattle 1951, p. 216, plus unemployment as estimated by Lebergott, *op. cit.* 1929–1949: U. S. Bur. Lab. Stat., *Handbook of Labor Statistics, 1950*, p. 35. 1950–1956: U. S. Dept. Commerce, Bur. Census, *Annual Report on the Labor Force*, annual issues, 1950 through 1956.

2. *Unemployment.* Lebergott, *op. cit.*

3. *Manufacturing employment.* 1910–1918: N.I.C.B. data from *Historical Statistics of the United States, 1789–1945*, Bur. Census, Washington 1949, p. 65. 1919–1956: U. S. Bur. Lab. Stat. data; from "Historical Data," *Employment and Earnings*, June 1957, *3*, 1.

4. *Nonagricultural labor force.* 1910–1918: computed from Lebergott's figures; Lebergott, *op. cit.* 1919–1956: U. S. Bur. Lab. Stat. Nonagricultural employment figures (from *Employment and Earnings, op. cit.*), plus Lebergott's unemployment estimates.

5. *Quit rates.* 1910–1918: computed from P. F. Brissenden and E. Frankel, "Mobility of Labor in American Industry," *Mo. Lab. Rev.*, June 1920, *10*, 48. 1919–1929: computed from *Mo. Lab. Rev.*, July 1929, *29*, 64–65, and Feb. 1931, *32*, 105. 1930–1948: computed from U. S. Bur. Lab. Stat., *Handbook of Labor Statistics, 1950*, p. 40. 1949–1956: *Mo. Lab. Rev.*, Apr. 1957, *80*, 517.

data since 1930 are more reliable than in the earlier years. The corresponding figures for the 1930-56 period is 70.0 per cent. In other words, 70 per cent of variations in the quit rate during this period can be said to be associated with variations in the unemployment rate.

The variance analysis measures deviations from a curvilinear regression line fitted to the original data. To facilitate computations, transformations to logarithms have been made, but all resulting relationships have been retransformed to original units.

While the results of the variance analysis are of interest, further analysis is necessary to identify the long-run changes. As one would expect, the lowest unemployment ratios were encountered in wartime and immediate postwar years, and the highest in depression years. There were 21 years, however, in which unemployment ratios were more than 3 but less than 6 per cent. We can discount the influence of work opportunity to a considerable extent by confining our analysis to these years.

(1) At the top of the heap we find five early years: 1910-12, 1920 and 1923. The average quit rate for these years was 6.2 per cent.

(2) Slightly lower are 1942, 1946, 1947 and 1948, with an average quit rate of 3.8 per cent. It is felt that quit rates in wartime and immediate postwar periods are affected by short-run changes in the propensity as well as the opportunity to move. Therefore these years should probably be excluded from the comparison.

(3) Now we encounter five years in the late 1920's: 1924, 1925, 1927, 1928 and 1929. The average quit rate was 2.3 per cent.

(4) Finally, five of the more recent years are found at the bottom: 1949, 1950, 1954, 1955 and 1956. For these years the quit rate averaged 1.5 per cent.

Thus, it is necessary to go back to the 1920's to find an interval which can rightly be compared with the period since 1948. Quit rates in this recent period (excluding the years of the Korean war) are indeed lower than in the middle and late 1920's. The decline amounts to about eight-tenths of a percentage point, or one-third of the total. Certainly the change is substantial enough to be noted. But this reduction, over a thirty-year span, is much smaller than the one which took place during the 1920's. Then the contraction was as much as 3.9 percentage points, or almost two-thirds.

Actually the measure of work opportunity used in Chart 1 is very rough. If we use a more refined alternative approach, about 85 per cent of the variations in the quit rate since 1930 can be explained.

The difficulty with the ratio of unemployment is that it refers to the labor force as a whole, whereas the available quit rate data refer to manufacturing workers. There are strong reasons to believe that the

manufacturing worker's opportunity to change his job is more closely related to employment conditions in manufacturing than to those in the total economy.

It makes a real difference if the measurement of work opportunity is limited to the manufacturing sector or extended throughout the entire economy. Movements of manufacturing employment and total
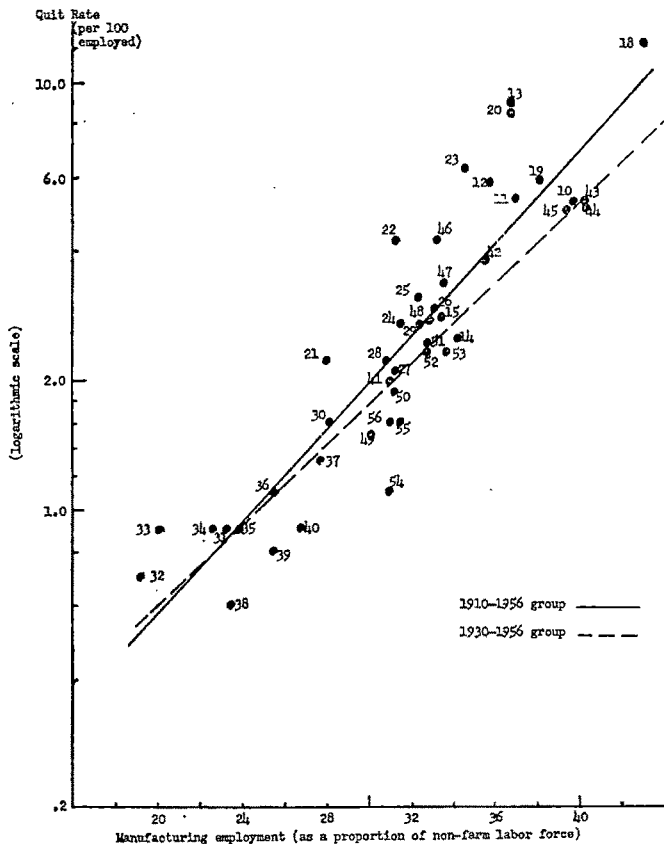
CHART 2. QUIT RATE COMPARED WITH MANUFACTURING EMPLOYMENT AS A PROPORTION OF NON-AGRICULTURAL LABOR FORCE.

employment have not been the same. During the 1950's, for example, manufacturing employment has been relatively constant whereas total employment has risen sharply. The same was true in the 1920's (See Table 1).

It is believed that the ratio between manufacturing employment and the nonagricultural labor force represents the most satisfactory measure of the factory worker's opportunity to change jobs. In Chart 2,

quit rates have been plotted against this ratio. A semilogarithmic representation produces a linear relationship, but the variance analysis was made on the basis of the original data, using the same method employed in connection with Chart 1. Here the explained variation is 71.3 per cent for 1910-56, and 84.1 per cent for 1930-56. Doubtless a more sophisticated form of statistical analysis would be desirable; but we have to do what we can with the available data. If it is conceded that quit rates are closely associated with opportunity to move, the purpose has been served.

Again the changes over time can be identified by reference to grouping the individual years. There are 12 years in which manufacturing employment equaled 35 per cent or more of the nonagricultural labor force. Quit rates were extremely high in all of these years, which included 1910-13, the two world wars, 1919-20, and 1923. Next there are 12 years in which manufacturing employment was less than 29 per cent of the nonagricultural labor force. Quit rates were generally low in these years, which included 1921 and 1930-40.

There remains a middle group of 21 years, with ratios of more than 29 per cent and less than 35 per cent. This is not quite the same middle group as appeared in Chart 1: for example, the years 1951-53 are now included, whereas 1910-12 are excluded. Within this group a very significant comparison can be made between 1924-29 and 1949-56. This comparison is significant for several reasons. The intervals are of substantial length and generally comparable in economic circumstances. Relative work opportunity, as defined in Chart 2, did not vary to any large extent. The more recent interval represents the period of negotiated economic security plans.[3] The average quit rate for 1924-29 was 2.6 per cent. For 1949-56 it was 1.8 per cent. Once more the difference is equal to eight-tenths of a percentage point, or about a third of the total. And once again the reduction is small as compared to that which took place during the 1920's.

II. *The Decline in the Quit Rate in the 1920's*

Our findings thus far can be summarized briefly. (1) A large part of the fluctuation in quit rates is associated with variations in the opportunity to move.[4] Therefore, it is clear that even if collective bargaining provisions did affect the willingness to move, they would operate

[3] 1949 was the year in which the pattern-setting health and welfare and pension plans were adopted in the steel industry, after a strike, pursuant to the recommendations of a presidential fact-finding board.

[4] Diminished opportunity to move is not the only reason why quit rates are low in depression years. Young workers with low seniority, who account for most of the voluntarily mobility (see Section III below), are likely to be involuntary unemployed during such years.

within a narrow compass. (2) A very substantial reduction in the quit rate occurred about the middle of the 1920's. (3) There has also been a smaller decline during more recent years. It remains to discuss the probable causes of these long-term changes.

Obviously the earlier phase cannot be explained by unionism, seniority or negotiated fringe benefits. While doubtless there were many contributing causes, the critical sequence of occurrences was probably somewhat as follows:

Until the 1920's, employers had made no particular attempts to conserve manpower. Under the liberal immigration policies of the day, conservation was unnecessary. During the first world war, however, employers had a foretaste of curtailment. Then the quota system began to take effect; and by the second half of the 1920's average annual immigration was less than one-third of what it had been two decades earlier [34, p. 92]. Meanwhile manpower needs were growing rapidly as industry expanded. The number of employees in manufacturing industries rose from 5,675,000 in 1904 to 7,514,000 in 1914 and 9,475,000 in 1923 [34, p. 783]. There were similar increases in other activities. Thus the demand for labor accelerated at the same time as an important source of supply was virtually eliminated. Furthermore, the occupational composition of the labor force was gradually shifting. The proportion of unskilled labor was declining; the proportion of skilled, semiskilled and white-collar workers was growing [8, p. 187]. So long as the unskilled hand was replaceable, the employer suffered no great loss when the employee quit. But when the trained employee resigned, a considerable investment had to be duplicated.

It was under these circumstances that personnel administration emerged and flourished as a specialized and systematic technique of industrial management. Surfeited as we are with "human relations" today, we find it difficult to comprehend the shock of original discovery which employers experienced in learning that workers were human beings. For the first time, attention was turned to the influences affecting discipline, efficiency, morale and worker adjustment in general. In other words, the employer decided to decasualize the whole employment relationship and to incorporate system into it.

Currently it is fashionable to assume that the central purpose of personnel administration in the 1920's was to keep the unions out. This is rewriting history to a large extent. It is true that some of the sources of the personnel administration movement are to be found in the strike wave following the first world war. For example, the Jersey Standard program initiated by the Rockefellers grew out of the strike at the Bayonne refinery; the McKenzie King program at Colorado Fuel and Iron resulted from the Ludlow massacre. But on the whole, in manu-

facturing industries at least, the unions were so weak in the 1920's that no special efforts were needed to keep them out. The really urgent objectives were to obtain more production from the worker, improve the condition of discipline, and especially, reduce the prodigious rate of labor turnover.[5] Exactly how this last objective was accomplished need not concern us here. Like most purposes tenaciously pursued, it had the reward of success; and if the available statistics are deserving of any belief at all, the success was considerable.

A further explanation is found in the stability of manufacturing employment during the late 1920's. Annual averages in Table 1 show a narrow fluctuation between a low of 9,523,000 and a high of 9,997,000 during the 1924-28 period. As indicated above, published turnover statistics refer to manufacturing industries. When manufacturing employment expands, young people, geographical migrants and unemployed workers are drawn in. These groups have high quit rates. When manufacturing employment levels off, the work force becomes stabilized. Average age and seniority increase. Older workers and senior workers (who tend to be the same individuals) are much less likely to quit.

III. *Reduction in the Quit Rate in Recent Years*

In addition to the signal reduction in the quit rate achieved during the 1920's, there seems to have been a smaller reduction during more recent years. The employee's desire to preserve his seniority status, retain his group insurance and protect his pension credits is frequently invoked as the cause. It has become so costly to quit, we are told, that the worker is bound to his job by chains of benevolence.

Actually it is not very likely that these negotiated benefits have had much effect on the quit rate. Most workers who quit their jobs are young in years and low in service. They do not have enough seniority to keep them from changing jobs; they have typically not reached an age where retirement is a real element in their thinking; and they have

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[5] The early textbooks and treatises in personnel management emphasized this goal. "The estimated economic loss due to turnover in the United States is placed in round numbers at one billion and a half dollars annually. . . . The cost of training of a new employee up to the point of efficiency varies from $25 per head to $1,000" [25, p. 61].

"The cost of labor turnover is far greater than one would at first imagine. . . . In practically every instance decreased turnover has been followed by increased production and lessened cost. One concern reduced its turnover from 150 per cent to 33.5 per cent, and its manufacturing cost was lowered by 10 per cent and its production increased by 42 per cent" [2, pp. 69, 71].

See also [3, pp. 12-13][12, pp. 197, 203, 205].

By 1929, however, it was possible to state: "The trend of labor turnover has been distinctly downwards during the last ten years, to such an extent, indeed, that the subject is no longer considered as of primary importance" [10, p. 300].

plenty of time to accumulate work credits after coming to rest. The older worker, on the other hand, is disinclined to change employment for a good many reasons even in the absence of seniority and fringe benefits—particularly the difficulty of securing another job, the probable loss of economic status, and his settled way of life generally.

The function of voluntary turnover has been admirably described by Lloyd G. Reynolds:

> Voluntary mobility is essentially a form of job shopping by workers. . . . Workers have great difficulty in judging the attractiveness of a job by talking it over in the company's employment office. The only way to judge it accurately is to work on it a while. After a few weeks or months of work, one can tell whether the job is worth keeping. This explains why quits are most frequent during the first few months of service and diminish rapidly after that point [19, p. 22].[6]

Innumerable labor mobility studies confirm this analysis. "So universally has mobility been found to decline with advancing age that this relationship may be regarded as conclusively established" [18, p. 102]. The inverse correlation between age and mobility has been demonstrated in surveys of various Philadelphia work groups in the 1920's and 1930's [16, p. 49] [11, p. 116] [17, p. 20], sample studies of employees with OASI wage credits in the 1940's [1, pp. 70-71] [4, p. 33], and most recently in Eldridge and Wolkstein's careful analysis of a sample of the entire labor force [9, p. 103].

These studies certainly throw doubt on the proposition that the moderate decline in the quit rate during recent years can be explained by seniority clauses and negotiated fringe benefits. They are not conclusive on the point, however, for the rate of turnover among older workers, although relatively low, is still considerable. The matter becomes much clearer when turnover rates and quit rates are correlated with length of service. Here the inverse relationship is much more striking. Thus, in his study of New Haven manual workers, Reynolds found that 71 per cent of "voluntary changers" had less than 3 years' service in the establishment; 80 per cent had less than 5 years; 94 per cent had less than 10 years. (This was a predominantly nonunion group, incidentally.) Reynolds concluded that "the propensity to move declines sharply with increasing length of service; it is slight after three years and negligible after ten years of work in the same plant" [19, p. 21].

This condition has prevailed as far back as labor turnover statistics have been available. As early as 1913-14, Brissenden and Frankel

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[6] The present writer has shown that the discharge is another phase of the trial-and-error process during the initial period of employment. Most discharged employees have low seniority status. See [21, pp. 29, 49].

found that 81.3 per cent of all "separations" had been on the payroll
less than a year, and 89.4 per cent less then two years. For 1917-18,
the corresponding figures were 83.9 per cent and 91 per cent [6, p. 51]
[23, pp. 54-57] [26, pp. 39-41]. Thus even the enormous labor turn-
over prior to 1920 was concentrated almost entirely among junior em-
ployees. The most recent demonstration is found in a series of labor
market studies issued in 1956. These were conducted in seven metro-
politan areas, by state agencies or local universities, and were coordi-
nated by the U. S. Bureau of Employment Security. To a considerable
extent, a common format and standardized survey and statistical
methods were used. The data for Minneapolis-St. Paul, Detroit, Phil-
adelphia and Worcester, are most comparable.[7] Although quit rates are
not tabulated by age and seniority, the total separation rates are most
instructive. The average monthly separation rate for all workers dur-
ing the sampling period varied between 1.6 per cent in Philadelphia and
3.9 per cent in Minneapolis-St. Paul. Among junior employees with less
than 1 year's seniority, the rate varied between 7 per cent in Philadel-
phia and 11.8 per cent in Worcester. For those with 1 to 4 years, it
lay between 2 and 3 per cent in all cases. For employees with 5 to 9
years of service, the rate is less than 1 per cent per month in each
area; and after 10 years, it becomes very low. Keeping in mind that
these are total separation rates, we can be sure that quit rates are negli-
gible indeed for workers with any substantial seniority—[28] [29]
[30] [31]—Appendix or suppl. Table 4(c) in each.

It is true that firms with pension plans generally have lower separa-
tion and quit rates than firms without pension plans. One should be
careful not to jump at conclusions, however. Firms which are likely to
adopt pensions plans are also likely to have lower separation rates for
independent reasons. The recent Los Angeles labor market study, for
example, shows that pension plans are associated with low turnover.
But pension plans are also relatively more frequent in the larger firms
and in nonseasonal industries [27, p. 53]. It is well established that
large firms tend to have low turnover rates, probably because of abund-
ant opportunities for promotion and transfer; and the same is true of
nonseasonal industries, almost by definition. In the Minneapolis-St.
Paul study, the higher quit rates among firms not having pension plans
virtually disappear when size of firm is held constant. In the Philadel-
phia study, such firms do not have higher separation or quit rates.
They do in Detroit, however, even when size of firm is held constant—
[29] [30] [31] Appendix or Supplementary Table 7(a) in each. Cer-
tainly it cannot be claimed that these studies are conclusive on the

[7] The San Francisco report is generally on a par with the four named above, except
that an error in calculation seems to have crept into some of the tables.

point. But at the least, they are sufficient to show the danger of spuri-
ous correlations between pension plans and turnover rates.

The material analyzed thus far in this section shows that a very high
proportion of labor turnover is concentrated among youthful workers,
and that practically all is concentrated among short-service employees.
These facts render it most unlikely that the reduction in the quit rate
can be explained by the attractiveness of seniority protection, group
insurance and industrial pensions.

It is likely that several other causes are responsible for the moderate
decline in the quit rate during recent years:

1. *The spread of unionism.* In the pre-union period, the most com-
mon method of expressing dissatisfaction was to quit. Nonunion work-
men have been known to strike, and sabotage and slowdowns were
available expedients; but generally if the employee became unhappy
enough, he could move off elsewhere. Under unionism this individ-
ualistic method of exhibiting and relieving discontent has been replaced
by concerted action, which can be invoked without quitting jobs.
Furthermore, many of the most obnoxious features of factory life have
been removed; and although discontent is the normal condition of man-
kind, the American factory worker's grounds for discontent are more
relative and technical than they were a generation ago. The appalling
conditions which once drove men out of their jobs have largely disap-
peared.

2. *Aging of the labor force.* "Aging" means that fewer employees
are going through the experimental phase of their working lives. It
results from the relatively small number of young people reaching
working age, and reflects the low birth rates of the 1920's and 1930's.
In 1940, 14.5 per cent of the civilian labor force in the United States
were between 20 and 24 years of age. As countless surveys have shown,
this age group accounts for a very large part of all labor turnover. By
1956, only 8.8 per cent of the labor force were between 20 and 24.
Another relatively young age group (25 to 34 years) declined to a
somewhat smaller extent, from 25.7 per cent to 22.2 per cent of the
labor force. Over the same period, the proportion of "mature" workers
in the civilian labor force (a comfortable euphemism designating those
between 45 and 64) increased steadily. It stood at 27.2 per cent in
1940 and 33.0 per cent in 1956.[8]

It is interesting to note that the proportion of young people in the
labor force has in all probability about reached bottom. The large age
generations of the 1940's will soon be ready for work. In due course

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[8] Sources of civilian labor force statistics: [32, pp. 1-254] [33, p. 22]. Statistics on the age
of distribution of manufacturing employees would be the most pertinent, since turnover
data are limited to manufacturing. Unfortunately such statistics are not available.

of time they will be followed by the even larger generations of the 1950's. (There were 2,155,000 live births in the United States in 1935, 2,360,000 in 1940, 2,735,000 in 1945, 3,554,000 in 1950 and 4,168,000 in 1956 [34, p. 56].) These new workers will go through their own periods of trial and error, and eventually settle into their own grooves. If the present analysis is valid, it follows that quit rates in 1966 should be considerably higher than those prevalent today.

It may well be, however, that most of the new workers will not find their way into the manufacturing industries where quit rates are measured. (The reasons for this expectation are explained in the next paragraph.) Therefore it is quite possible that manufacturing industries will not participate fully in the "de-aging" of the labor force and the concurrent increase in the quite rate during the 1960's.

3. *Stability of manufacturing employment* can be listed as a third reason why the quit rate among factory workers has recently declined, as well as explaining why it is less likely to increase during the next decade. It will be remembered that manufacturing employment remained stable from 1924 through 1928; and this fact has been cited as one explanation for the declining quit rates in the second half of the 1920's. During the present decade, the number of production workers in manufacturing has not increased since 1951. In contrast, employment in various white-collar fields such as wholesale and retail trade, finance, insurance, government, etc., has moved up sharply [34, p. 202]. Since young persons entering the labor market gravitate toward expanding areas of activity, the new entrants have been attracted into nonmanufacturing industries to a disproportionate extent. Presumably labor turnover rates have increased there, but unfortunately we have no measures. At the same time they have been depressed in manufacturing.

Corroboration is found in the fact that the manufacturing labor force has "aged" considerably faster than the total labor force. This was already evident by 1950, as Table 2 shows. It is believed that the disparity is considerably greater at the present time. The proportion of employees in the mobile phase of working life has become even smaller than otherwise would have been the case.

4. *Effect of seniority rules.* Age and length of service are intimately related. While there are no statistics on the seniority status of employees in manufacturing, undoubtedly the average factory worker has a greater length of service today than he did ten or fifteen years ago. And the quit rate almost drops out of sight for workers with more than five years of employment in a given establishment. Wholly aside from any effect on motivation, seniority rules have facilitated the preservation of a stable work force in the manufacturing industries. Even the

short-service employee is protected against replacement by a younger worker from the outside as long as he survives his probationary period and is not discharged for cause. (This factor is particularly important in industries where seasonal layoffs are important, such as the manufacture of automobiles and ladies' garments.) Thus he is kept on the payroll to the point where he reaches a decision to "stay put," and his trial-and-error period can be said to have closed. Thereafter, the guarantee of being rehired after layoff and the protection against arbitrary

TABLE 2.—MEDIAN AGE OF EMPLOYED WORKERS, UNITED STATES, BY
INDUSTRY AND SEX, 1940 AND 1950

	Males			Females		
	1940	1950	Increase	1940	1950	Increase
All industries	38.3	39.7	1.4 yrs.	32.3	36.4	4.1 yrs.
Manufacturing industries	36.0	38.4	2.4 yrs.	29.8	34.5	4.7 yrs.

Sources: U. S. Dept. Commerce, Bur. Census, *Sixteenth Census of the United States: 1940: Population:* Vol. III, *The Labor Force;* Pt. I, *United States Summary,* Washington 1943, pp. 197, 199; and *Census of Population: 1950,* Vol. II, pp. 1–286, 287.

discharge permit him to carry his decision into effect.[9] Although seniority rules have thus contributed indirectly to the moderate reduction of the quit rates in manufacturing during recent years, they have done so not by virtue of their attractiveness and not by tying men to their jobs, but rather by tiding them over the trial-and-error period.

· Of these various causes of the moderate decline in the quit rate in recent years, the spread of unionism is probably not reversible. The "aging" of labor force on the other hand is reversible and is scheduled to be reversed. The stability of manufacturing employment as a "cause" does not have much social significance and would disappear if more extensive turnover data were available. The effect of seniority rules, finally, does not involve any diminished propensity to change jobs at a given level of seniority.

Probably a complex of sociological factors should also be included as causes. People marry early, have children quickly, buy a tract house and imbed themselves in the slough of instalment debt. Then there is the prevailing mood of togetherness and the fear of disrupting the little ones by changing their schools.

[9] "The propensity to change employers diminishes rapidly with increasing length of service. It is not entirely correct, however, to say that it is length of service which 'causes' a worker to remain with an employer. It is at least equally correct to regard the acquisition of years of service as the *result* of a prior decision by the employee that the company is a good place to work" [19, p. 39]. Similarly, "Greater length of service is not an *explanation of past* immobility, but a *description* or *measure* of it" [18, p. 107].

All in all, little evidence can be found for the proposition that labor resources have become immobilized and a new industrial feudalism has been created because men can no longer afford to quit their jobs.

APPENDIX A—LABOR TURNOVER STATISTICS

There are grave difficulties in using the available time series of turnover rates for comparative purposes. While the comparisons made in the foregoing article are considered valid, the reader is entitled to a description of the series. (Sources have already been cited at the bottom of Table 1).

For the period 1910-18, Brissenden and Frankel pieced together some large government surveys with more fragmentary data to make a continuous series. The samples are very small except for 1913-14 and 1917-18. Aside from these two years, the industrial composition is not known, except that it included not only manufacturing but also nonmanufacturing industries, as well as certain activities excluded from later studies as being too seasonal. The employment base used to calculate turnover rates did not consist of actual employees. "Equivalent full-year workers" of 3,000 man-hours were used for this purpose. The aggregated rates are arithmetic means weighted by "equivalent full year" employment.

Data for 1919-29 were gathered by the Metropolitan Life Insurance Company. Again the inadequacy of the sample is evident, especially for the 1919-26 period when fewer than 160 firms of uncertain industrial and geographical composition were included. This time the employment basis is the average number of workers on the payroll; and the aggregated rates are unweighted medians.

In 1930 the Bureau of Labor Statistics inaugurated the current series of turnover statistics. The sample of firms is much larger and undoubtedly more representative than those used in the earlier series. As in the original 1910-18 studies, aggregate rates are weighted averages. Actual employment on the survey date is used for the computation of rates.

The most critical comparison, as we have noted, is between the quit rates in the late 1920's and those in the 1950's. Unfortunately the Metropolitan Life Insurance rates and the B.L.S. rates are only roughly comparable at best. We have no information as to the industries and plants represented in the Metropolitan surveys. Use of average monthly employment to calculate percentages, rather than actual employment on the survey date, probably did not introduce any systematic bias. Median rates, used in the 1919-29 series, will certainly be different from the weighted averages used since 1930. If, as is likely, the larger firms have lower turnover rates, medians will run higher than weighted averages.

Despite these difficulties, it is believed that the statistics are good enough for the present purposes. There are several reasons for this belief. First, a primary objective of this article has been to cast doubts on certain affirmative propositions. These propositions have been made without careful study of the available statistics and certainly without access to any better ones. Second, precision is not of crucial importance. General tendencies and relationships are more significant than exact quantities. Third, although a good part of the

statistical showing can properly be questioned on grounds of sampling error, etc., there are so many indications pointing in the same direction as to suggest that the argument is probably valid.

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PRICING OBJECTIVES IN LARGE COMPANIES

By ROBERT F. LANZILLOTTI*

The recent sharpened interest of the "Kefauver Committee" in administered prices and inflation has focused attention once again on the inadequate state of knowledge of the price-making process.[1] In particular, more empirical information is needed with respect to (a) the motivational hypothesis of the firm, i.e., the specific objectives upon which business firms base pricing decisions, and (b) the mechanics of price formulation. This article is addressed to the first problem; it will present some data on pricing objectives of the firm which have been developed in the course of a general study of pricing policies and practices of large industrial corporations.

I. Scope of Present Study

The procedure followed involved the postprandial variety of research. Lengthy interviews were undertaken with officials of twenty companies over periods ranging up to about one week in most cases.[2] A second set of interviews was undertaken several years later to fill in gaps in the data and to ascertain if any changes had been made in price policy since the original interviews. Pricing obviously being a sensitive area, some officials did not care to discuss their policies except in general terms, but these persons paved the way to individuals who were more willing and, in some cases, more aware of the practices employed and reasons for them.

[1] See *Administered Prices*, Hearings Before the Subcommittee on Antitrust and Monopoly of the Committee on the Judiciary, United States Senate, 85th Cong., 1st Sess., Washington 1958.

[2] The companies were selected from among the largest corporations on the basis of the willingness of management to cooperate by permitting extensive interviews with top company officials: Aluminum Company of America, American Can, A & P Tea Company, du Pont, General Electric, General Foods, General Motors, Goodyear, Gulf, International Harvester, Johns-Manville, Kennecott Copper, Kroger, National Steel, Sears, Standard of Indiana, Standard Oil Company of New Jersey (ESSO), Swift, Union Carbide, and U. S. Steel.

The questions were designed to elicit information concerning: (1) whether any formal or informal commercial goals had been adopted by the corporation; (2) the procedures employed for implementing and evaluating the goal; (3) the techniques of price determination (i.e., the mechanics of pricing); and (4) the functions of pricing executives (individuals, committees, special divisions, etc.)—including extent of authority on price matters, kinds of materials utilized by them in setting prices, and relative weights given to various price-influencing factors. The portion of the information presented in this paper concerns, for each of the twenty companies, the principal and collateral objectives which are regarded as guiding pricing decisions.

The twenty corporations have one feature in common: each of them is among the 200 largest industrial corporations, and over one-half fall with in the 100 largest industrials, in terms of assets. But they differ in a wide variety of ways from each other. Some, like Johns-Manville, U. S. Steel, International Harvester, and Union Carbide, dominate a whole industry and are price leaders. At the other extreme, there are companies like Swift and A & P which face so many competitors of various sizes and abilities that in spite of their absolute size they are very far from being able to make decisions for the market, and do not think of competition in terms of actions of one or a few competitors. The other companies fall between these extremes.

II. Company Goals: Rationalizations of Pricing Methods

It is important to recognize at the outset that a company statement of policy is not necessarily an accurate representation of what that policy is.[3] Also, company rationalizations of pricing do not always represent the first step in planning price policy, and not all pricing of a given company is determined by the general company objective.

In a few cases officials insisted that there was little latitude in selecting a policy. However, for the most part, the prominence of each of the corporations in their respective industries makes most of them masters, to a significant degree, of their fates; hence, they are able to adjust pricing to the company's general goal.

Table 1 presents a summary of the principal and collateral pricing goals of the twenty companies as determined from interviews with their respective officials. The most typical pricing objectives cited were: (1) pricing to achieve a target return on investment; (2) stabilization of price and margin; (3) pricing to realize a target market share; and

[3] The following analysis is based upon the author's interpretations of views expressed orally by officials of the corporations concerned. Of course, neither the companies nor the author wish these views to be interpreted as necessarily the official views of the companies.

(4) pricing to meet or prevent competition. In most of the companies, one of the goals predominates, but as the listing of collateral objectives indicates, price-making by any one firm was not always ruled by a single policy objective.[4]

III. *Pricing to Achieve a Target Return on Investment*

Target return on investment was perhaps the most frequently mentioned of pricing goals.[5] About one-half of the companies explicitly indicated that their pricing policies were based mainly upon the objective of realizing a particular rate of return on investment, in a given year, over the long haul, or both; but in most cases the target was regarded as a long-run objective. The average of the targets mentioned was 14 per cent (after taxes); only one was below 10 per cent; and the highest was 20 per cent.

Under this pricing system both costs and profit goals are based not upon the volume level which is necessarily expected over a short period, but rather on standard volume; and the margins added to standard costs are designed to produce the target profit rate on investment, assuming standard volume to be the long-run average rate of plant utilization. In effect, the procedure is designed to prevent cyclical or shorter-run changes in volume or product-mix from unduly affecting price, with the expectation that the averaging of fluctuations in cost and demand over the business cycle will produce a particular rate of return on investment.

Firms that were conscious of shooting for a particular target return

[4] To illustrate, in U. S. Steel, out of a variety of divergent views mentioned, three rationales can be distinguished. (1) The first is the "ideal" price, i.e., pricing that is believed to be "just, fair, and economic," with reference to a general target of about 8 per cent after taxes on stockholders' investment plus long-term debt. This strand is colored by the management's concept of the corporation as the industry leader vested with the responsibilities and subject to the inhibitions of a public utility. In fact, one official said he was "unable to understand or properly describe the Corporation's pricing policy except as something like the approach of the public utilities." (2) The second rationale centers on the difference between the "ideal" system and what officials regard as the "practical exigencies of steel price-making," i.e., limitations imposed upon price policy "by followers who are disloyal and prices of competitive products that get out of hand." (3) A third policy objective is essentially a target market share and is embodied in the motto: "to obtain as a minimum that share of all markets for the products sold, product by product, and territory by territory, to which the corporation's capacity in relation to the industry as the whole entitles it, and to accomplish this participation ratio through the exercise of judgment so as to insure the maximum continuing return on investment to the Corporation."

[5] Target-return pricing is defined as the building up of a price structure designed to provide such a return on capital employed for specific products, product groups, and divisions, as to yield a predetermined corporate average return. In most cases managements referred to stockholders' equity (net worth) plus long-term debt. Usually a standard cost system is used as a means of allocating fixed cost to various product divisions, with the standards premised on an assumed rate of production, typically about 70 per cent to 80 per cent of capacity, and an assumed product-mix as "normal."

TABLE 1.—PRICING GOALS OF TWENTY LARGE INDUSTRIAL CORPORATIONS

Company	Principal Pricing Goal	Collateral Pricing Goals	Rate of Return on Investment (After Taxes) 1947–1955[a]		Average Market Share[b]
			Avg.	Range	
Alcoa	20% on investment (before taxes); higher on new products [about 10% effective rate after taxes]	(a) "Promotive" policy on new products (b) Price stabilization	13.8	7.8–18.7	Pig & ingot, 37%; sheet, 46%; other fabrications, 62%°
American Can	Maintenance of market share	(a) "Meeting" competition (using cost of substitute product to determine price) (b) Price stabilization	11.6	9.6–14.7	Approx. 55% of all types of cans[d]
A & P	Increasing market share	"General promotive" (low-margin policy)	13.0	9.7–18.8	n.a.
du Pont	Target return on investment—no specific figure given	(a) Charging what traffic will bear over long run (b) Maximum return for new products—"life cycle" pricing	25.9	19.6–34.1	n.a.
Esso (Standard Oil of N. J.)	"Fair-return" target—no specific figure given	(a) Maintaining market share (b) Price stabilization	16.0	12.0–18.9	n.a.

[a] Federal Trade Commission, *Rates of Return (After Taxes) for Identical Companies in Selected Manufacturing Industries, 1940, 1947–55*, Washington [1957], pp. 28–30, except for the following companies whose rates were computed by the author using the methods outlined in the Commission Report: A & P, General Foods, Gulf, International Harvester, Kroger, National Steel, Sears Roebuck, and Swift.

[b] As of 1955, unless otherwise indicated. Source of data is company mentioned unless noted otherwise.

° *U. S. v. Alcoa et al.*, "Stipulation Concerning Extension of Tables III–X," dated May 31, 1956, U. S. District Court for the Southern District of New York.

[d] As of 1939, U. S. Department of Justice, *Western Steel Plants and the Tin Plate Industry*, 79th Cong., 1st Sess., Doc. No. 95, p. L 1.

Company	Principal pricing goal	Collateral pricing goals			
General Electric	20% on investment (after taxes); 7% on sales (after taxes)	(a) Promotive policy on new products (b) Price stabilization on nationally advertised products	21.4	18.4–26.6	—ᵉ
General Foods	33⅓% gross margin: ("⅓ to make, ⅓ to sell, and ⅓ for profit"); expectation of realizing target only on new products	(a) Full line of food products and novelties (b) Maintaining market share	12.2	8.9–15.7	n.a.
General Motors	20% on investment (after taxes)	Maintaining market share	26.0	19.9–37.0	50% of passenger automobilesᶠ
Goodyear	"Meeting competitors"	(a) Maintain "position" (b) Price stabilization	13.3	9.2–16.1	n.a.
Gulf	Follow price of most important marketer in each area	(a) Maintain market share (b) Price stabilization	12.6	10.7–16.7	n.a.
International Harvester	10% on investment (after taxes)	Market share: ceiling of "less than a dominant share of any market"	8.9	4.9–11.9	Farm tractors, 28–30%; combines, cornpickers, tractor plows, cultivators, mowers, 20–30%; cotton pickers, 65%; light & light-heavy trucks, 5–18%; medium-heavy to heavy-heavy, 12–30%
Johns-Manville	Return on investment greater than last 15-year average (about 15% after taxes); higher target for new products	(a) Market share not greater than 20% (b) Stabilization of prices	14.9	10.7–19.6	n.a.
Kennecott	Stabilization of prices	Stabilization of prices	16.0	9.3–20.9	n.a.

ᵉ The company states that on the average it aims at not more than 22 to 25 per cent of any given market. Percentages for individual markets or products were not made available, but it is estimated that in some markets, e.g., electrical turbines, General Electric has 60 per cent of the total market. Cf. Standard and Poor's, Industry Surveys, "Electrical-Electronic-Basic Analysis," Aug. 9, 1956, p. E 21.

ᶠ Federal Trade Commission, Industrial Concentration and Product Diversification in the 1000 Largest Manufacturing Companies: 1950, Washington, Jan. 1957, p. 113.

TABLE 1—(*Continued*)

Company	Principal Pricing Goal	Collateral Pricing Goals	Rate of Return on Investment (After Taxes) 1947–1955[a]		Average Market Share[b]
			Avg.	Range	
Kroger	Maintaining market share	Target return of 20% on investment before taxes[g]	12.1	9.7–16.1	n.a.
National Steel	Matching the market—price follower	Increase market share	12.1	7.0–17.4	5%
Sears Roebuck	Increasing market share (8–10% regarded as satisfactory share)	(a) Realization of traditional return on investment of 10–15% (after taxes) (b) General promotive (low margin) policy	5.4	1.6–10.7	5–10% average (twice as large a share in hard goods v. soft goods)
Standard Oil (Indiana)	Maintain market share	(a) Stabilize prices (b) Target-return on investment (none specified)	10.4	7.9–14.4	n.a.
Swift	Maintenance of market share in livestock buying and meat packing		6.9	3.9–11.1	Approximately 10% nationally[h]

[g] Target return on investment evidently characterizes company policy as much as target market share. In making investment decisions the company is quoted as follows: "The Kroger Co. normally expected a return on investment of at least 20% before taxes." See McNair, Burnham, and Hersum, *Cases in Retail Management*, New York 1957, pp. 205 ff.

[h] This represents the average share of total industry shipments of the four largest firms in 1954. *Cf. Concentration in American Industry*, Report of Subcommittee on the Judiciary, U. S. Senate, 85th Cong., 1st Sess., Washington 1957, p. 315.

	Target return on investment[1]	Promotive policy on new products; "life cycle" pricing on chemicals generally	19.2	13.5–24.3	—
Union Carbide					
U. S. Steel	8% on investment (after taxes)	(a) Target market share of 30% (b) Stable price (c) Stable margin	10.3	7.6–14.8	Ingots and steel, 30%; blast furnaces, 34%; finished hot-rolled products; 35%; other steel mill products, 37%[k]

[1] In discussions with management officials various profit-return figures were mentioned, with considerable variation among divisions of the company. No official profit target percentage was given, but the author estimates the *average* profit objective for the corporation to be approximately 35% before taxes, or an effective rate after taxes of about 18%.

[j] Chemicals account for 30% of Carbide's sales, most of which are petro-chemicals, a field that the company opened thirty years ago and still dominates; plastics account for 18%—the company sells 40% of the two most important plastics (vinyl and polyethylene); alloys and metals account for 26% of sales—top U. S. supplier of ferroalloys (e.g., chrome, silicon, manganese), and the biggest U. S. titanium producer; gases account for 14% of sales—estimated to sell 50% of oxygen in the U. S.; carbon, electrodes, and batteries account for 12% of sales—leading U. S. producer of electrodes, refractory carbon, and flashlights and batteries; and miscellaneous—leading operator of atomic energy plants, a leading producer of uranium, the largest U. S. producer of tungsten, and a major supplier of vanadium. *Cf.* "Union Carbide Enriches the Formula," *Fortune*, Feb. 1957, pp. 123 ff.; Standard and Poor's, *Industry Surveys*, "Chemicals Basic Analysis," Dec. 20, 1956, p. C44; and "Annual Report for 1955 of the Union Carbide and Carbon Corporation."

[k] The range of the corporation's capacity as a percentage of total industry capacity varies from 15% to 54%, as of January 1957. For more detail see *Administered Prices, Hearings Before the Subcommittee on Antitrust and Monopoly of the Senate Committee on the Judiciary*, 85th Cong., 1st Sess., Pt. 2, *Steel*, Washington 1958, pp. 335–36.

on investment in their price policies were those that sold products in a market or markets more or less protected and in which the companies were leaders in their respective industries. In Alcoa, du Pont, Esso, General Electric, General Motors, International Harvester, Johns-Manville, Union Carbide, and U. S. Steel, the pricing of many products was hinged to this particular objective, and with the expectation of being able to reach the target return. Target-return pricing was usually tied in with a long-run view of prices, especially on new products where an "orderly" stepping down ("cascading") of prices was followed by du Pont, Union Carbide, and Alcoa.

A distinction should be made, however, between those companies that use target return on investment as a rigid and primary guide to pricing and those to whom it is more useful as a benchmark in an area where prices otherwise might be subject to wide and dangerous variations.[6]

Columns 4 and 5 of Table 1 show the average and range of the profit rates realized by the twenty companies over the 1947-1955 period. It will be noted that the target figures are *less* than the actual returns: for the nine-year period, the target-return companies earned on the average slightly more to substantially more than their indicated profit objective (International Harvester being the only exception). Also, there is a rather wide range in the profit rates for each company.[7]

The actual profit rates may be higher than the targets for several possible reasons: (a) the targets may only be nominal or minimal goals (which is suggested by footnote 7); (b) the generally prosperous nature of the period in question in which company operations exceeded

[6] To illustrate, the use of rate-of-return pricing by U. S. Steel (likened by its officials to a public utility's "fair return"), apparently has not always been consistently followed. Under market pressure, U. S. Steel has at times had to accept much less than this return; when desperate for business, as in 1938, its competitors offered substantial concessions below published prices on almost every type of business. A very different situation shows up in the discussions of the target return by officials of General Motors. Instead of vainly attempting to realize its target in good years and bad, General Motors takes a long-run view and has sufficient assurance of its retention of a minimum market share to accept a diminished profit note in years when diminished output bears a heavy unallocated overhead. Du Pont seems to assume its ability to realize a target return, especially in connection with new products. The same could be said for Union Carbide, the other chemical producer in the sample. International Harvester, although as vulnerable as U. S. Steel to wide swings in volume of business, appeared to be less worried by competitors' ability to jeopardize its prices based on long-run normal cost and return. Harvester was not able to maintain its prices during the great depression, and there is no evidence that such reductions as it made correspond merely to changes in direct cost. But in spite of frank admission by Harvester's management that the company was faced by tough competition, company officials appeared to be much more independent in their pricing policy than U. S. Steel.

[7] If the lowest figure for each firm is omitted, however, the low side of the range of returns approximates the target figure. This is especially true of Alcoa, du Pont, Johns-Manville, Union Carbide, and U. S. Steel.

"normal" or average percentage of capacity upon which costs and prices were determined; and (c) some of the companies have found that pricing on an historical-cost basis using the company's traditional objective does not provide adequate capital for replacement and expansion at current costs, and accordingly have made allowance for this factor in their pricing formulas.[8] Thus, if actual profit rates were "adjusted" for changes in the price level, the actual profits would more closely approximate the stated targets.

Whichever of the foregoing may be the most plausible explanation of the differences between actual and target profit rates, the findings indicate that a distinction must be made between year-to-year and secular profits objectives. The evidence on actual profit rates, taken in conjunction with the targets mentioned, raises serious questions whether these companies are attempting to "maximize" profits on a year-to-year basis. Moreover, to construe the actual profit rates (as against target rates) as evidence of a long-run maximization policy would require the demonstration that the prices charged were based not upon the targets but on what the firms believed they could get as a maximum. In any event, for this sample of firms and for this time period, there are limitations upon profit maximization as an adequate explanation of the relationships between profit targets and actual profit rates.

It is perhaps significant that there has been an increasing tendency in recent years for the companies in the sample to adopt some form of target-return pricing, either across-the-board or at least for particular products. In a few cases it was found that managements had developed a target-return policy between the time of the first interviews with the company and subsequent interviews several years later. The reasons for this movement toward greater use of a target-return approach are varied, but the major influences seem to have been: (a) an increasing awareness of and concern by managements for profit-capital-investment planning and capital budgeting, especially in the conglomerate company within which there is keen competition for capital funds by many units; (b) the desire for a good common denominator for evaluating the performance of divisions and product groups; (c) the wartime experiences of most of the companies with "cost-plus," "cost plus

[8] When U. S. Steel, for example, announces an increase in its base prices, it usually justifies its action in terms of increased direct costs, especially labor costs. But that rising capital costs have also influenced the prices set in recent years is suggested by President Hood's announcement in connection with the $8.50 increase in 1956:

"The new prices do not provide a solution to the problem that United States Steel faces with respect to inadequate depreciation allowances for the replacement of obsolete and outworn facilities, nor do they attempt to provide a solution to the many problems attending the expansion program upon which United States Steel is currently engaged."
New York Times, August 7, 1956, p. 10.

fixed fee," and other contractual arrangements with the government
which focused attention on the return on investment; and (d) the
emulation, by competitors and others, of succesful large companies
which have followed a target-return policy for many years (several
companies in the sample mentioned that they had patterned their gen-
eral target-return policy after that of du Pont or General Motors).

It is not surprising that new products above all are singled out for
target-return pricing. Since they have no close rivals, new products are
usually expected to produce a predetermined level of profit return on
the investment represented.[9] No rigid length of time after the intro-
duction of the product was mentioned in which the target is supposed
to be achieved. However, the time horizon is more short range vis-à-vis
established products in the sense that the target payout is delineated
from the start.[10] Accordingly, pricing may take the form of "skimming"
the market by exploiting the inelasticity of demand in different markets
(maintaining a selected price as long as actual or potential competition
permits), or a "penetration" price policy designed to develop mass
markets via relatively low prices, provided a rapid expansion of the

[9] A good example of the kinds of data utilized in determining which new products will
be added or which existing facilities will be expanded is one company's procedure for
capital investment decisions. The request by a civision for new funds shows (a) esti-
mated new commitment (new fixed investment, working capital, and noncapital expendi-
tures); (b) estimated total utilized investment (the new investment plus transfer of
existing investment); (c) estimated annual operating income (i.e., income before depre-
ciation, amortization, depletion, other income and income taxes); and (d) estimated
return on investment income, which is shown both as a ratio to the new commitment and
the total utilized investment. No figure was mentioned as a minimum return; normally
new products were expected to return better than the corporate average, but expansions
of existing facilities have been made on a projected return of no greater than 20 to 25 per
cent before taxes.

An elaborate check-off list is designed to insure attention to various aspects of projected
demand, supply, costs, and competition. Of particular interest are such items as: capacities,
captive requirements and future expansion plans of competitiors; company's estimated
market share before and after expansion; degree of diversity of customers; extent to
which success of venture depends upon short- or long-term contracts; the effects of changes
in tariff rates on competition from abroad; selling prices used for sales to other units of
the company; shape of short-run unit cost curve; comparative cost position of competitors;
the degree to which an alternative exists of either making or buying important inter-
mediates; flexibility of proposed facilities for production of other products; the prob-
abilities of obsolesence of the process or products; and the relative position of the com-
pany with respect to research and development, technical knowledge, labor supply, patents,
and raw materials.

[10] The problem here is not simply one of the target return and target payout period,
but rather one of balancing the desire to recoup development and other investment costs
as rapidly as possible against the desire to prolong the period from distinctiveness to
obsolescence by discouraging potential competitors with a relatively low-price or low-
profits policy. The most rapid recovery of investment mentioned was one year, with two
years not infrequently mentioned, especially where the innovative monopoly was not
expected to last long or process secrety was not secure. Also, there did not appear to be
any consistent relationship between the presence of patent protection and the payout
period.

market and higher returns may be obtained later. This approach is most typical of du Pont, Union Carbide, Alcoa, International Harvester, and General Foods. The prescribed target for new products is usually higher than on established products, at least initially. But the target approach is not limited to unique products; it is also typical of low-unit-profit high-volume commodities (e.g., steel, aluminum, and chemicals).

Minimum target profit figures also are used by most of the companies as a basis for sloughing off products and in arriving at "make-or-buy" decisions. An exact minimum target figure was rarely mentioned, but good justifications were required of operating divisions or product departments when returns consistently fell below the corporate average. Not infrequently, officers made statements along the following lines: "If the average corporate return were, say, 20 per cent and the return on investment for a particular item kept falling below 10 per cent, it would be dropped unless (a) a good customer needs it in order to keep a full line, or (b) it is a by-product anyhow, and anything it brings in is really gravy."

A variety of explanations was given by the companies to justify the particular size of the profit target used as a guide in pricing decisions. The most frequently mentioned rationalizations included: (a) fair or reasonable return, (b) the traditional industry concept of fair return in relation to risk factors, (c) desire to equal or better the corporation average return over a recent period, (d) what the company felt it could get as a long-run matter, and (e) use of a specific profit target as a means of stabilizing industry prices. At least one of the foregoing, and most frequently the first, was mentioned by the companies interviewed, and in a few cases the entire list was offered as justification for the company profit goal.

This reinforces the observation made earlier that no one single objective or policy rules all price-making in any given company. In fact, in many companies a close interrelationship exists among target-return pricing, desire to stabilize prices, and target market-share (either a minimum or maximum objective); this is especially true of U. S. Steel, Union Carbide, and Johns-Manville. It would seem, however, that a target-return approach is ordinarily incompatible with a market-share policy; that is, if a company desires to expand its share of the market, it will be inclined to place less emphasis on rigid adherence to a predetermined target.

IV. *Stabilization of Price and Margin*

The drive for stabilized prices by companies like U. S. Steel, Alcoa, International Harvester, Johns-Manville, du Pont, and Union Carbide involves both expectation of proper reward for duty done, i.e., "proper"

prices, and a sense of *noblesse oblige*. Having earned what is necessary during poor times to provide an adequate return, they will refrain from upping the price as high as the traffic will bear in prosperity. Likewise, in pricing different items in the product line, there will be an effort (sustained in individual cases by the pricing executive's conscience) to refrain from exploiting any item beyond the limit set by cost-plus.

The distinction between target return on investment as a pricing philosophy and cost-plus pricing in the companies surveyed is difficult to define. Some of the companies that clearly employ the target-return-on-investment procedure in pricing new products—the area of most frequent use of target-return pricing—use cost-plus pricing for other products. The difference between the two rationalizations lies in the extent to which the company is willing to push beyond the limits of a pricing method to some average-return philosophy. According to a General Motors executive, the target plays a prominent role in the formulation of the cost-plus method.[11] But in the case of International Harvester, U. S. Steel, A & P, Johns-Manville, Alcoa, or Union Carbide, it seems fair to say that the pricing executive set the prices of many products on a cost-plus basis (except where competition precludes such action) without questioning the appropriateness of the traditional mark-up.

Cost-plus, therefore, may be viewed as one step on the road to return-on-investment as a guide, or precept for price policy. But some firms never go any farther. The standard can be accepted as self-sufficient; just as the target-return perhaps needs no modification to make it accord with profit maximization (with all the necessary qualifications). Pricing executives seldom look beyond the particular formula with which they are accustomed to justify their decisions. They differentiate between price policies according to the degree of control they exercise; but not by the gap between the price policy and an ideal of profit maximization. They appear as ready to accept cost-plus at a reasonable volume as an ultimate standard for pricing as any other principle.

V. *Target Market-Share*

A maximum or minimum share of the market as a determinant of pricing policy was listed almost as frequently, and seemed to govern policy almost to the same extent as target-return on investment. Share

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[11] See Donaldson Brown, "Pricing in Relation to Financial Control," *Manag. and Admin.*, Feb. 1924, 7, 195-98, 283-86, 417-22. This may seem to be a rather old reference, but General Motors officials cited it so frequently as an accurate representation of their present-day pricing that it warrants emphasis.

of the market was ordinarily thought of in terms of a maximum, bearing witness to the power of the corporations interviewed. Being giants, they were careful to limit themselves; they apparently did not wish to gobble up any market they entered, unless it was one which they had created, like nylon, asbestos pipe, aluminum screen wire, cable products, or some synthetic chemical.

Hence, the target share of the market as a guide to pricing tended to be used for those products in which the firm did not, at the outset, enjoy a patent or innovative monopoly. Du Pont made no mention of shooting for a given share of the cellophane or nylon market, nor did Union Carbide in the Prestone market; Johns-Manville set no limit to its market share in specialized insulation materials; American Can was not thinking in terms of winning against stiff competition a moderate share of the market for vacuum packed cans; nor was Alcoa in the wire and cable market. But a General Electric official spoke at length of the company's policy of not exceeding 50 per cent of any given market because it then would become too vulnerable to competition.[12] Johns-Manville officials likewise indicated that product and sales development are geared to attaining a given percentage of the market for a product line. The company endeavors, executives indicated, to maintain the offensive, rather than to be subject to attack because of their large product share. The company felt strongly that 20 per cent of competitive markets was the maximum share in which it was interested. This policy ruled in those areas where Johns-Manville was *not* the price leader. It stresses sales, service, and superior quality of its product in order to maintain its prices somewhat above those of its competitors. Apparently the program of reaching no more than a given market-share and of moving ahead against competition does not find expression in price reductions.

It is not possible to reach any general conclusions from comparisons of target market-shares and actual share of business realized by the companies mentioning this as a policy for pricing purposes. This is due on the one hand to the unwillingness of the companies to specify in detail particular target-share percentages, and on the other to the lack of sufficiently detailed information for the companies in question, es-

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[12] He stated, "The company would rather be pushing to expand a 25 per cent share than defending a 50 per cent share." As a matter of fact, he indicated, there were few instances where G. E. had more than 22 to 25 per cent of a market. In substance, this means that when G. E. enters an appliance field with a new product, it will price to match its competitors. The company believes that it has been a downward price leader on appliances generally, however, and that both its postwar attempt to lead in price reductions and its long-term reduction in margins (its over-all margins were said to be only 58 per cent as high as in 1940) demonstrates that it has not been content merely to follow the ruling price after moving into a field.

pecially for the highly diversified firms. Patently, most of these companies have very significant proportions of national markets.[13]

VI. "Meeting or Matching Competition" (4)

To some of the officials interviewed, the requirement that the product price "meet competition" appeared, at first glance, to preclude the existence of any pricing policy at all. Meeting competition according to their view cannot be regarded as a rationalization of action; it is the action itself.

The rationalization of this policy of meeting competition is far from elaborate; at first blush it is perhaps unnecessary. How can "meeting competition" be dignified as one out of several alternative guides to action? In chemicals, du Pont seems to apply a rule of thumb of adopting the going price in the markets for many standardized products where it never had or else had lost the leadership—e.g., carbon tetrachloride, hydrogen peroxide, disodium phosphate, nitric acid, hydrochloric acid, and various rubber chemicals. Moreover, in the case of many products selling on a freight-equalization basis, prices were not set at a high executive level; the pricing in many cases had not been reviewed for years, having been established beyond the ken of anyone now in the organization. Yet, even here there is perhaps more discretion than the officials are willing or accustomed to admit. In the pricing of neozone, du Pont was forced—though it had introduced the chemical —to change its price policy because of the tactics of competitors, who shifted the basing point. But need the matter have stopped there? Was there not a decision by du Pont to go no further than matching the Akron-based price? In many other cases du Pont undoubtedly could, if it chose, have altered the basing points or other features of the marketing of chemicals of which it produced more than an inconsequential market share.

[13] One interesting example of the connection between pricing (livestock bidding), market share, and investment policy is found in Swift. An analysis of livestock buying raises the question whether there is something of an understanding by the major packers of what constitutes their "normal share" of the animals sold in given public stockyards, which was the essence of the Department of Justice's complaint (1948) against Armour, Swift, Cudahy, and Wilson (since dismissed). It would seem that the relative constancy of the proportions of livestock purchased by the principal meat packers is traceable in large part to the short-run fixity of plant capacity, the desire to keep that plant operating at least up to a specific minimum level of utilization (governed partly by labor commitments), and the ever present threat that another packer may secure a larger share of the animals and the market for dressed meats. In view of these considerations, the percentages of animals purchased by the major packers would logically evidence substantial constancy over periods of weeks or months in given markets. But, unless this same approach is carried over into the planning of plant sizes in new locations (or enlargement of established plants), as well as the rate of utilization of these facilities, this would seem to be an insufficient explanation for the long-run stability of shares.

In many cases the policy of meeting competition appears to be materially influenced by market-share psychology. Esso Standard, while going to great lengths to devise a cost-plus theory, has modified it when and where it seemed necessary or desirable. Standard of Indiana was even more specific in basing its policies on "meeting"—or forestalling—competition. Esso and, to a much lesser extent, Standard of Indiana refrained from publishing or trying to reduce to definiteness the details of the policy. A number of questions related to the companies' rationalizations are basic to understanding the functioning of the policy, for clearly neither company changed prices instantaneously when facing "competition": Did they meet the exact price charged, at the refinery or to the retail dealer? How long did a substandard price have to prevail before it could undermine a cost-plus price? Whose competitive price brought action? How were competitors rated in effectiveness? Answers to these questions are basic to an understanding of the policy. But the oil companies have not divulged the facts that would permit full and consistent treatment of the theory of "meeting competition" as seen by their managements.

It seems also that in some cases the companies are not simply meeting competition—they are preventing it. This appears to have been the purpose of A & P in localizing price cuts to make matters difficult for a competitive store on its opening day, or General Foods in reducing the price of Certo and Sur-Jell in the Northwest where rival pectins were strong.[14] Standard of Indiana, a dominant seller not overfond of price wars, may easily justify meeting competition locally on the basis that the policy offers a permanent threat to potential price-cutters.

In other cases, the companies are aware of specific competitive products whose prices must be matched by their own if volume is to be expanded. Union Carbide knew that its synthetic organic chemicals, like the various alcohols, had to meet or undersell the price of the natural products if the investment was ever to be returned. In other cases, where a standardized commodity—e.g., bakery flour, livestock feeds, and frozen fish sold by General Foods, flour by General Mills, or wholesale meat by Swift—is simply marketed at a price over which no firm, or even small group of firms, can have control, then pricing policy ceases to have meaning. The phrase "meeting competition" is either

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[14] This information was not provided by the companies when interviewed, but is based on statements in the A & P antitrust case and the General Foods F. T. C. case made by officials of the respective companies. An A & P official of the Atlantic Division, for example, said, "It might be necessary for us to operate unprofitably for several weeks . . . reducing our line of [sic] 10% several weeks prior to the time the competitor plans to open so that people in the comunity will be impressed with our low prices. . . ." *U.S. v. New York Great Atlantic and Pacific Tea Co., Inc.*, 67 F. Supp. 626 (1946), p. 668; see also *ibid.*, pp. 667, 669, and Government Brief, pp. 909, 931; and General Foods, F. T. C. Docket No. 5675, Complaint, July 7, 1949.

inapplicable or inaccurate, since there is no specific competition to meet
—only the market price.

VII. *Other Rationalizations*

There are other pegs on which managements hang pricing decisions.
In view of American Can's undisputed (at least until 1954) leadership
in the metal container industry, and its bargaining power vis-à-vis
both its suppliers and customers, it is somewhat surprising that the
company should not have set out an explicit pricing goal in terms of
return on investment. The management seems to be more concerned
with the assurance of funds for innovating research than any particular
target return on investment, although the maintenance of its market
share through its closing-machine leasing policy indirectly accomplishes
the same objective. The company's pricing policy could be construed
as "marginal" in the sense that it automatically (via its contracts)
transmits to its customers increases or decreases in costs of materials
(tin plate) or labor in the can factories. In turn, this adjustability in
price seems to have had the effect of stabilizing American Can's
margin, the price of its services as the owner of can-closing equipment
and engineering services, and, at the same time, the price of cans
throughout the canning season.

The companies cited many instances involving the need for resolu-
tion of conflicts of interest between integrated and nonintegrated firms
and between established giants and newcomers, which displaced the
usual bases for their pricing decisions. The Robinson-Patman and
Sherman Acts, even when they have not been the basis for actions
against the companies, were used as fundamental rationalizations of
policy.

VIII. *A Composite View of Pricing Objectives*

Because it is big the large firm envisages itself as a part of a socially
integrated group, with responsibilities for the whole pipeline and produc-
tion (including full-line offerings) and associated distribution. They see
themselves in a continuing relationship not only with their own distrib-
utors, but even with dealers and ultimate customers, and with their
suppliers—even when the latter lacked, or especially when they lacked,
the bargaining power of a larger firm. The market, in effect, is regarded
as a creature of the firm, and the firm has the responsibility for preserv-
ing these relationships and perpetuating its own position.

The size of these firms also makes them an obvious target for anti-
trust suits, legislation, Congressional investigation, and similar restrain-
ing forces. To a certain extent, size thus entails a vulnerability and gen-
erates a sense of *noblesse oblige*. This is reinforced by the disposition

of the government and the community generally to look on and appeal to these firms as "pattern-setters" for industry generally; and in pricing they are expected to avoid taking full advantage of immediate profit opportunities. This attitude is perhaps most clearly expressed in the *Economic Report of the President* of January 1957, which stated:-

> Specifically, business and labor leadership have the responsibility to reach agreements on wages and other labor benefits that are fair to the rest of the community as well as to those persons immediately involved. . . . *And business must recognize the broad public interest in price set on their products and services.* (p. 3, italics added.)

From this point, it is an easy step to the position taken by the typical large firm that it is entitled to a "just price" and "fair return" on investment. In the case of some companies, like U. S. Steel, the resolution of conflicts of interest between integrated and nonintegrated firms, between established giants and newcomers, and between the pattern-setter and the community generally, has modified company price policy to a point where even the managements have come to refer to it as akin to that of a public utility. This may be a logical development in cases where unpleasant experiences of cutthroat competition—especially in fairly standardized products like steel, copper, gasoline, and aluminum —have generated a disposition by management to avoid price changes except through periodic, thoroughly considered, and well-publicized alterations in recognized base prices. By relating price revisions to changes in direct costs (especially increases in wage costs), the firm avoids the annoyance to itself and its customers (who they claim vastly prefer stable prices) of frequent changes in price structure.

This desire for stabilized pricing, oftentimes described with a blanket adjective as "administered," usually implies that the company or companies set some kind of target to which their price policies conform. The price, according to this view, is under the control of one firm acting as the price leader or a group of firms that make policy for the industry. The contention of the business executives themselves is that an administered price, like the tank-wagon price of gasoline, far from being an independent creation of the price leader, is merely a device for approximating a market equilibrium. According to this view, there are so many possibilities of substitution of one product for another, or an off-brand for a name brand, that the limits of discretion are much narrower than is generally supposed. Administration of prices, officials contend, thus merely avoids the decision to use cutthroat competition—which itself would be another form of administered pricing; it also avoids temporary exploitation of shortages. Refraining from raising prices when a higher price is necessary to equate supply with demand, is also

justified by management on the grounds that over the long run higher prices would disturb equilibrium by bringing unneeded capacity into the industry. But it is impossible to accept the conventional justification for leadership. It can masquerade as resulting in a genuine "equilibrium" only if the word is made equivalent to whatever is the decision of the leading firms.

The foregoing data, above all, make it clear that management's approach to pricing is based upon *planned* profits. The company proceeds on the assumption of the need for a certain amount of capital to undertake the investment in plant expansion and new facilities which are envisaged for the long haul in order to maintain and/or improve market position. In some cases, quite in contrast to the thinking of management before the second world war, this desire to hold position and to penetrate wider markets requires that capital investment should be planned with built-in excess capacity (this is best illustrated by the fact that prices are premised on the assumption of operating at a rate of 75 or 80 per cent of capacity, which is assumed to be the long-run normal). In deciding upon which products and productive facilities will be added or expanded, the top-level corporation appropriations committee relies upon estimates of returns on utilized investment. The only way in which price policy can be viewed in such companies as these, with their wide variety of products and selling in a large number of different markets, is in terms of profits-investment ratios. This criterion serves as an effective guide for pricing decisions at divisional and departmental levels. If we are to speak of "administered" decisions in the large firm, it is perhaps more accurate to speak of administered *profits* rather than administered *prices*.

IX. *Conclusions*

The principal purpose of this paper has been to contribute to our knowledge of the actual process by which prices are formed in industry, with the expectation that the data will help in constructing a more realistic theory of the firm capable of yielding useful predictions of industrial price behavior. The general hypothesis which emerges is that (a) the large company has a fairly well-defined pricing goal that is related to a long-range profit horizon; (b) its management seeks— especially in multiproduct multimarket operations—a simultaneous decision with respect to price, cost, and product characteristics; and (c) its pricing formulas are handy devices for checking the internal consistency of the separate decisions as against the general company objective. Under this hypothesis no single theory of the firm—and certainly no single motivational hypothesis such as profit-maximization— is likely to impose an unambiguous course of action for the firm for

any given situation; nor will it provide a satisfactory basis for valid and useful predicitions of price behavior.

In pursuit of price policies that will yield the maximum satisfaction of the company's community of interests, the findings show that one company will prefer stability, another will seek to expand its market share, or to engage in continuous discovery and pre-emption of new fields, while others will be content to meet competition, to satisfy a set target, or to aim at combinations and variations of these goals. It seems reasonable to conclude that the pricing policies are in almost every case equivalent to a company policy that represents an order of priorities and choice from among competing objectives rather than policies tested by any simple concept of profits maximization. Managerial specialists down the line are given a framework of requirements that must be met, while managers at the top, of course, are free to and do change these requirements to meet particular situations.[15]

Another relevant aspect of the data for theoretical analysis is the conception of the market held by managements of large corporations. Individual products, markets, and pricing are not considered in isolation; the unit of decision-making is the enterprise, and pricing and marketing strategies are viewed in this global context. Because of the tremendously complex joint-cost problems and the lack of knowledge of actual relationships between costs and output or sales, on the one hand, and the joint-revenue aspects of multiproduct companies, on the other, pricing is frequently done for product groups with an eye to the over-all profit position of the company. This means that costing of products ends up as a result of price policy rather than the reverse. In view of the various external pressures on the company and the nature of the strategy of the enterprise, however, it is doubtful if prices would bear any closer relationship to actual costs were detailed cost data available to management. The incentive to realize target rates of profits for the long haul better suits the objectives of management-controlled companies than any desire to profiteer or to seek windfall profits.

It might appear that there are conflicts between the objectives of price leaders and price followers, e.g., between such companies as U. S. Steel and National Steel. Actually, however, it is a matter of leaders having fairly well-defined target objectives, whereas price followers evidently do not have independent targets. Their objective, especially where undifferentiated products make up the bulk of the product line,

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[15] "The managerial philosophy not only calls into question the assumption of profit maximization as a workable description of entrepreneurial behavior but denies the institutional basis of the classical profit motivation." E. S. Mason, "The Apologetics of 'Managerialism'," *Jour. of Bus.*, Jan. 1958, *31*, 6.

will be determined by the target set by the price leader. If the target is acceptable, the follower is content to hold a market share and will adjust price policy accordingly.

In more general cases, including differentiated product markets as well as undifferentiated, the extent to which companies—with the dimensions and diversification of those under discussion—serve as leaders or followers on individual products or product groups depends upon the profit-importance of a particular product in a given company's line, the nature of the product—whether a producer or a consumer good—and the size and degree of diversification of companies with which there are product overlaps. Moreover, the manner in which interfirm policies will be coordinated will depend upon the above factors as they bear upon particular products, plus the over-all objectives of the enterprise as a unit and its general market strategy.

A further implication of the findings for the theory of the firm is the relationship found between price and investment decisions. The information on this aspect is limited, but nevertheless the setting of and attempt to follow specific target returns on investment are manifest at two separate levels of operations: short-run pricing and investment decisions. The investment decision presupposes a price (and usually a market-share) assumption, which, in turn, determines short-run price decisions thereafter. Thus, investment decisions in effect are themselves a form of pricing decision, and over time become an inherent part of price policy.

Finally, the general approach of these large corporations to price policy, and the attendant price behavior, raise some important issues for public policy. Their very size—both absolutely and relatively—permits the managements to select from among various alternative courses of action. This is a fairly clear manifestation of economic or market power. In partial reflection of this power, plus a variety of other reasons related to their size, vulnerability to public criticism, and potential antitrust action, these corporations tend to behave more and more like public utilities, especially the target-return-minded companies. To complicate the issue further, target-return pricing implies a policy of stable or rigid pricing, even though exceptions are found within particular product lines.

A crucial question raised by these facets of policy is: What is the net impact on economic growth and stability? More specifically, do target-return pricing, profits planning, and the attendant price behavior, tend to promote or inhibit stability and growth? Much more adequate empirical data on corporation objectives and detailed study of individual company pricing, profits, and investment planning over the course of economic fluctuations are needed before answers can be given to this question.

INTERNATIONAL TRADE AND ECONOMIC EXPANSION

By Jagdish Bhagwati*

The recent literature on the effects of economic expansion on international trade has been concerned with two principal problems: the impact of the expansion on the terms of trade; and the resultant change in the welfare of the trading nations. The solutions offered, however, are not fully satisfactory. Thus H. G. Johnson [5] and W. M. Corden [3], who attempt to tackle the first problem, succeed only in establishing the *direction*, as distinct from the *extent*, of the consequential shift in the terms of trade. In so far as the full impact of the expansion on the terms of trade must be known prior to determining the change in the welfare of the countries involved, it is not surprising that the second problem has received scant attention.[1]

It is intended in this paper to resolve principally the problem of bringing the different factors that affect the terms of trade into a single formula to determine the extent of the shift in the terms of trade consequent upon economic expansion. The analysis is further rendered geometrically by translating the usual textbook back-to-back partial diagram, depicting international trade equilibrium in a single commodity, into a general equilibrium framework. The argument is then extended, in a brief section, to the welfare effects of the expansion. To the gain from growth must be added the gain or loss from the resultant shift, if any, in the terms of trade; conditions are derived to determine whether the growing country will experience a net gain or loss from the expansion. The final section of the paper is concerned with the concept of the "output elasticity of supply" (to be used in the paper) and the analytical methods that can be employed to investigate the output elasticity of supply of different activities under specified varieties of expansion.

I. *Formula to Determine Change in the Terms of Trade*

The model used here is the familiar two-country (I and II), two-commodity (X and Y), "real" model with continuous full employment of all factors. Transport costs and intercountry factor movements are ab-

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[1] It should be mentioned, however, that Johnson [5] has an excellent analysis of this problem in the context of a model of complete specialization, although the concern of the article is principally to evolve a criterion to determine the impact of expansion on the terms of trade.

sent. To simplify the analysis, economic expansion, defined as the country's capacity to produce extra output at constant relative commodity prices, is confined to country I. We wish to determine the consequent impact on I's commodity terms of trade. The analysis is conducted in terms of I's importable good (Y); it is one of the advantages of our two-good model that the analysis can be couched entirely in terms of one good and yet will hold generally.

The total impact on the terms of trade of country I as a result of the expansion is compounded of six effects:

1. *Change in the Output of Y due to Economic Expansion.* The change in the output of importables (Y) in country I, at constant relative commodity prices, as a result of the economic expansion, is given by:

$$(1) \qquad \frac{\delta Y}{\delta K} \cdot dK = Y \cdot E_{SY} \cdot \overline{K}$$

where Y is the domestic production of importables in I prior to the expansion; K is I's productive capacity which is assumed to be kept fully employed and is measured by the value, in terms of exportables (X), of the output the country would produce at the initial terms of trade;

$$\overline{K} = \frac{dK}{K}; \text{and } E_{SY} = \frac{K}{Y} \cdot \frac{\delta Y}{\delta K}$$

is the output elasticity of supply of importables at constant relative commodity prices. This represents, therefore, the change in the domestic production of importables directly as a result of expansion, at constant terms of trade. If (1) is positive, the supply of Y is increased; if it is negative, the supply of Y is reduced.[2]

2. *Change in the Demand for Y due to Economic Expansion.* We must now consider the change in the demand for importables, at constant relative commodity prices, as a direct result of the expansion. This is given by:

$$(2) \qquad \frac{\delta C}{\delta K} \cdot dK = C \cdot E_{DY} \cdot \overline{K}$$

where C is the pre-expansion consumption of importables (Y) in I and

$$E_{DY} = \frac{K}{C} \cdot \frac{\delta C}{\delta K}$$

is the output elasticity of demand for importables at constant relative

[2] Formula (1) may be negative under certain circumstances. This possibility is outlined again in Section III and is actually demonstrated, in the context of our highly simplified model, in Section IV. Also see Bhagwati [1].

commodity prices.[3] If (2) is positive, there is an increase in the demand for Y; if it is negative, the demand for Y is decreased.

It follows that the net change in the demand for imports, at constant terms of trade, will be given by $[(1)-(2)]$. If this expression is positive, there is a net decrease in I's demand for imports at the pre-expansion terms of trade and hence the terms of trade will tend to improve for I (unless II's offer curve is infinitely elastic); if the expression is negative, there is a net increase in I's demand for imports and the terms of trade will tend to deteriorate for I.[4] In order to determine the extent of the shift in the terms of trade which will be necessary to restore equilibrium, however, we must introduce the following four factors, three domestic and one foreign. Each of them measures one aspect of the changes in the supply of and demand for importables induced by a shift in the terms of trade.

3. *Change in the Demand for Y due to Price Shift.* The change in the demand for Y due to the shift in the terms of trade may be measured by the following expression:

$$(3) \qquad \frac{\delta C}{\delta p} \cdot dp = -\frac{C}{p} \cdot \epsilon \cdot dp$$

where p is the terms of trade, measured as the number of units of exportables required to buy a unit of importables; and

$$\epsilon = -\frac{p}{C} \cdot \frac{\delta C}{\delta p}$$

is the income-compensated or constant-utility demand-elasticity for importables (Y), representing movement *along* the indifference curve in response to the price-shift. If (3) is positive, there is an increase in the demand for Y; if it is negative, there is a reduction in the demand for Y. The demand for Y will be negatively correlated with changes in the price of Y relative to the price of X.[5]

4. *Change in the Supply of Y due to Price Shift.* The change in the domestic supply of Y due to the shift in the terms of trade is:

$$(4) \qquad \frac{\delta Y}{\delta p} \cdot dp = \frac{Y}{p} \cdot \sigma \cdot dp$$

where

$$\sigma = \frac{p}{Y} \cdot \frac{\delta Y}{\delta p}$$

[3] Output elasticity of demand is used in preference to income elasticity to describe the behavior of aggregate consumption as aggregate income rises, to include the effects of population changes, growth of per capita incomes, and resultant changes in income distribution.

[4] This is, in effect, Johnson's [5] central argument.

[5] This is so because we normally assume, for well-known reasons, that the substitution effect, with which (3) is concerned, is negative.

is the supply-elasticity of importables based on movement along the transformation curve in response to the price-shift. When (4) is positive, the supply of Y is increased; when it is negative, it is decreased. The supply of Y will normally be positively correlated with changes in the price of Y relative to the price of X.[6]

5. *Change in the Demand for Y due to Change in Real Income resulting from Shift in the Terms of Trade*. A change in the terms of trade leads to a consequent change in real income. This income change is approximated here by the usual method employed widely in the theory of international trade and based on the theory of value, namely, by the difference in the cost of the initial quantity of imports. The resultant change in the demand for Y is:

$$(5) \qquad -\frac{\delta C}{\delta K} \cdot M \cdot dp = -\frac{C}{K} \cdot M \cdot E_{DY'} \cdot dp$$

where $M \equiv C - Y$ is the quantity of initial imports; and

$$E_{DY'} = \frac{K}{C} \cdot \frac{\delta C}{\delta K}$$

is the elasticity of demand for Y with respect to a change in income resulting from changed terms of trade. If (5) is positive, there is an increase in the demand for Y; if it is negative, there is a reduction in the demand for Y.

6. *Change in the Supply of Y by Country II due to Price Change*. As a result of the shift in the terms of trade, the supply of Y by II to I changes. This is given by:

$$(6) \qquad \frac{\delta S_m}{\delta p} \cdot dp = \frac{M}{p} \cdot r_m \cdot dp$$

where $S_m \equiv M$ and

$$r_m = \frac{p}{M} \cdot \frac{\delta S_m}{\delta p}$$

is the *total* elasticity of II's supply of its exports (commodity Y) to I, in response to a shift in the terms of trade. II's supply of Y to I increases or decreases according as (6) is positive or negative.[7]

The total impact on the terms of trade is then derived from the simple proposition that, in equilibrium, the excess demand for Y should be zero. Thus we can collect all the effects into two groups: those on the

[6] This holds again because we normally assume that the transformation curve is a convex set.

[7] The elasticity r_m will be negative, for instance, when the exports of I are Giffen goods in II; though this is by no means a necessary condition for r_m to be negative.

supply side, $[(1)+(4)+(6)]$; and those on the demand side,$[(2)+(3)+(5)]$. We subtract the latter from the former and set the expression equal to zero. Solving for dp, we get the magnitude of the shift in the terms of trade consequent upon economic expansion:

(7)
$$dp = \frac{(C \cdot E_{DY} - Y \cdot E_{SY}) \cdot \overline{K}}{\left[\dfrac{Y}{p} \cdot \sigma + \dfrac{M}{p} \cdot r_m + \dfrac{C}{p} \cdot \epsilon + \dfrac{C}{K} \cdot M \cdot E_{DY}'\right]},$$

which may be rewirtten as:

(8)
$$dp = \frac{p \cdot dM}{M\left[\dfrac{Y}{M} \cdot \sigma + r_m + \dfrac{C}{M} \cdot \epsilon + p \cdot \dfrac{C}{K} \cdot E_{DY}'\right]},$$

provided it is remembered that dM refers to the income effect of expansion on imports at constant terms of trade.

We have thus succeeded in bringing together into a single formula, and thereby establishing the relative significance of, the different factors (elasticities) which simultaneously determine the impact of expansion on the international commodity terms of trade. The analysis can be readily extended to the case of simultaneous growth in *both* countries. This can be done by replacing

$$\left[\frac{M}{p} \cdot r_m \cdot dp\right]$$

by an elaborate expression derived by extending to country II an analysis exactly analogous to that we have applied to country I.[8]

Some interesting results follow from our analysis. Thus in order for the terms of trade to turn adverse to the growing country it is not sufficient that the income effects of the expansion should be unfavorable and should create an increased demand for imports (that is, $C.E_{DY} > Y.E_{SY}$). It is also necessary that the expression

$$\left[\frac{Y}{M} \cdot \sigma + r_m + \frac{C}{M} \cdot \epsilon + p \cdot \frac{C}{K} \cdot E_{DY}'\right],$$

which constitutes the denominator in (8), should be positive. Since both σ and ϵ are normally positive, and since E_{DY}' is also positive (unless either the commodity Y is an inferior good in the strict Hicksian sense or the expansion is accompanied by a redistribution of income biased strongly against the consumption of Y), it follows that r_m will have to be not merely negative but also sufficiently large in magnitude

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[8] Nothing substantive is gained by carrying out this exercise.

in order that the entire expression should be negative. The converse is also true: where the income effects are favorable and lead to a reduction in the demand for imports, the terms of trade may still worsen for the growing country I if the total elasticity of II's supply of its exports (Y) to I is sufficiently large and negative to make the denominator in (8) negative. These conclusions are, no doubt, intuitively plausible, which is perhaps an advantage; what is chiefly claimed is that the precise relation in which the different operative factors stand vis-à-vis one another, which has been attempted here, lends a needed element of rigorousness to these qualitative results. Besides, it enables us to investigate more satisfactorily the related problem of the impact of economic expansion on the welfare of the growing country (Section III).

II. *Geometrical Analysis*

Using the familiar partial equilibrium back-to-back diagram determining the flow of exports of a single commodity from one country to an-
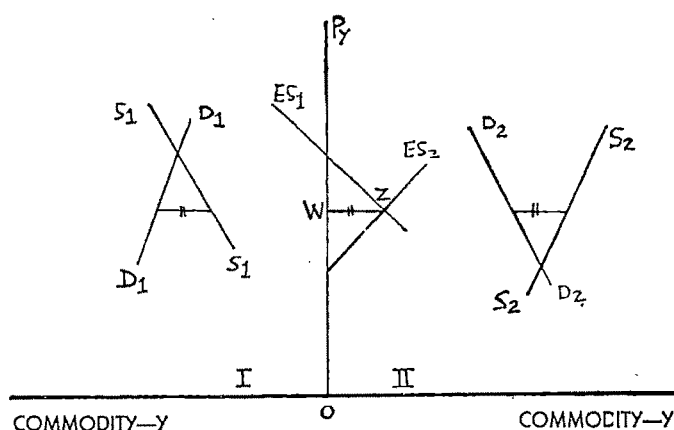
FIGURE 1

other, we propose now to: (1) show how this partial diagram can be transformed into a general equilibrium diagram; and (2) relate the diagram to, and thereby illustrate, the argument algebraically presented in Section I.

Figure 1 shows the usual partial equilibrium diagram for depicting international trade equilibrium in a two-country model. Transport costs are assumed to be zero. D_1D_1, S_1S_1 and D_2D_2, S_2S_2 are the domestic demand and supply curves of countries I and II respectively. ES_1 and ES_2 are the excess-supply functions, as Samuelson [11] calls them, of I and II respectively. Equilibrium is at Z where the exports of Y from II match the imports of Y into I; the equilibrium price of Y is OW.

Now this diagram can be readily converted into a general equilibrium diagram in the following fashion. Relabel the vertical axis $p = p_y/p_x$, the terms of trade, instead of p_y, the price of Y. Further, instead of regarding D_1D_1, D_2D_2, S_1S_1 and S_2S_2 as partial curves,[9] treat them as general equilibrium or total curves. Thus S_1S_1 and S_2S_2 now represent schedules of varying supply of Y as the change in the terms of trade shifts production along the transformation curve. The reinterpretation of D_1D_1 and D_2D_2 is slightly more involved as the schedules are now compounded of two effects: (1) the shift in the demand for Y caused by the real-income change resulting from the change in the terms of trade; and (2) the change in the demand for Y as the change in the terms of trade shifts consumption along the indifference curve (i.e., the substitution effect).
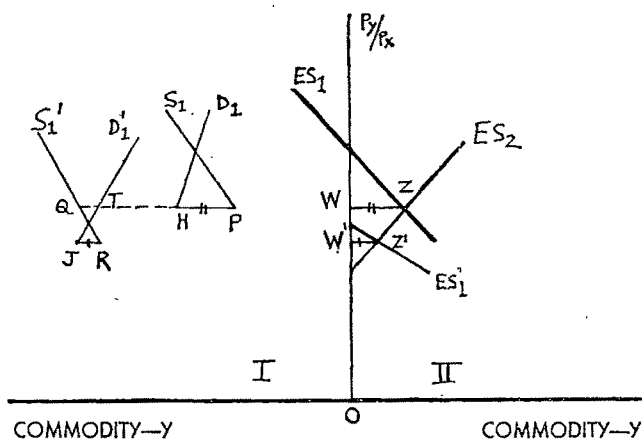
FIGURE 2

The reader may still doubt whether the transformation of the partial into a general equilibrium diagram has been accomplished. Equilibrium in the diagram, as now interpreted, is still in the Y market. What about the X-market? The answer is straightforward. As argued before, it is one of the advantages of a two-good model, such as the one employed here, that the argument can be couched entirely in terms of one good. Equilibrium in the Y-market implies equilibrium in the X-market as well.

We have thus accomplished our first task of transforming the partial into a general equilibrium diagram. We can now proceed to derive geometrically the argument and result of Section I. In Figure 2 we assume that, as a result of economic expansion, S_1S_1 and D_1D_1 shift to $S_1'S_1'$ and $D_1'D_1'$ respectively. ES_1 correspondingly shifts to ES_1' and the new

[9] See an interesting note by Hicks [4] on how the Marshallian supply curve, corresponding to Marshall's demand curve, should be derived.

equilibrium terms of trade are at OW'. Country II now exports $W'Z'$ of Y and I imports JR $(=W'Z')$ of Y. The total impact on imports and exports can then be analyzed as follows:

1. *Total Effect on Demand for Y in I.* (a) The movement from H to T is the income effect of expansion, measured by (2) in Section I. (b) The movement from T to J is compounded of the income effect on consumption of Y due to the shift in the terms of trade and the substitution effect on consumption of Y due to the same shift. It is measured therefore by (5) and (3) in Section I. (c) The movement from H to J, representing the total effect on the demand for Y in I, is thus measured by [(2)+(3)+(5)].

2. *Total Effect on Supply of Y in I.* (a) The movement from P to Q is the income effect on production of the expansion, measured by (1). (b) The movement from Q to R is the substitution effect on production of Y due to the shift in the terms of trade and is measured by (4). (c) The movement from P to R, representing the total effect on the supply of Y in I, is thus measured by [(1)+(4)].

3. *The Net Effect on the Excess Supply of Y in I.* This is then given by: $(1)+(4)-[(2)+(3)+(5)]$. This corresponds to the difference between HP $(=WZ)$, the old volume of imports, and JR $(=W'Z')$ the new volume of imports. If the expression is positive, there is a net reduction in the demand for imports into I; if it is negative, there is a net increase.

4. *The Net Effect on the Excess Supply of Y in II.* This is similarly given by (6). It corresponds to the difference between WZ and $W'Z'$. If it is positive, there is a net increase in the supply of imports to I; if it is negative, there is a net decrease.

The formula for determining the shift in the terms of trade is now easily derived. In equilibrium, the net changes in the excess supplies of Y in I and II must match each other and also add up to zero:

$$(1) + (4) - [(2) + (3) + (5)] + (6) = 0.$$

Solving the above for dp, we can derive formula (7). The geometrical construction not only represents the transformation of a useful diagram of partial analysis into a general equilibrium framework but also serves the purpose of deriving the results of Section I visually. There is an additional advantage that follows from our transformation; this is a direct result of Samuelson's ingenious use of this diagram (in a partial framework) to convert the international trade problem involved into a maximum problem, thereby enabling the anlayst "to make rigorous predictions as to the qualitative direction in which the variables of the system will change when some change is made in the data of the problem" [11, p. 299] and to derive generalized reciprocity relations.

III. *Expansion and Economic Welfare*

Having derived an expression to measure the precise impact of expansion on the commodity terms of trade, we can now analyze the net effect of expansion on the welfare of the growing country. Economic expansion, while increasing output, might lead to a deterioration in the terms of trade and a corresponding reduction in the growth in real income of the country experiencing the expansion. Where expansion leads to deterioration in the country's commodity terms of trade, there are three possible outcomes for the country's economic welfare:[10] net gain, no gain, or actual loss. We propose now to investigate the conditions under which these possibilities will respectively materialize.

Let dK denote the gain in real income that results from growth of output, at constant relative commodity prices. From this we must subtract the loss of real income that arises from the attendant deterioration in the terms of trade by approximating this loss with the familiar expression: $M.dp$. Using formula (8), we can say that the growing country, I, will, as a result of growth, experience net gain, make neither gain nor loss, or actually suffer immiserizing growth according as:

$$dK \gtreqqless \frac{p \cdot dM}{\left[\dfrac{Y}{M} \cdot \sigma + r_m + \dfrac{C}{M} \cdot \epsilon + p \cdot \dfrac{\delta C}{\delta K}\right]}$$

which simplifies to:

(9)
$$\left[\frac{Y}{M} \cdot \sigma + \frac{C}{M} \cdot \epsilon + y\right] \gtreqqless -r_m$$

where $y = p.(\delta Y/\delta K)$ and it is assumed that $E_{DY'} = E_{DY}$, so that a change in real income due to a reduction of import prices has the same effect on the demand for importables as a change in real income due to growth.[11]

It may be of interest to note that, since ϵ and σ are necessarily positive,[12] the possibility that growth might be immiserizing would arise only if either the demand for the growing country's exports is inelastic (r_m is negative) or growth actually reduces the domestic production of importables at constant relative commodity prices (y is negative). (Neither of these conditions, it should be noticed, is sufficient for im-

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[10] The analysis outlined here is subject to all the familiar caveats attending on discussions of social welfare.

[11] For a similar assumption, see Bhagwati [1]. For further observations and an able discussion of related issues, see Johnson [7].

[12] This argument is again based on the assumption of convex indifference and transformation curves, convexity being defined in the strict mathematical sense.

miserizing growth to occur.[13]) Although, as indicated in Section I, y will normally be positive, it is possible to postulate assumptions under which it will be negative; this possibility is demonstrated in Section IV where the concept of the output elasticity of supply is further explored.

IV. *Increased Factor Supply and Output Elasticity of Supply*

Our formulae for determining the change in the terms of trade and the impact of growth on the welfare of the growing country draw upon elasticity concepts that are familiar to economists from the modern theory of value.[14] The concepts of output elasticity of supply and of demand (at constant relative commodity prices), E_{SY} and E_{DY}, however, are fairly recent concepts although they have already been widely used [2] [3] [5] [6]. They would appear far more familiar if they were described as yielding respectively Engel's curves of production and consumption of the commodity in question. Whereas, however, Engel's curves of consumption are respectable in the literature, those for production are still a sufficiently rare phenomenon to justify a sketch of the analytical techniques by which they may be derived. Of the two major sources of economic growth, namely expansion of factor supply and technical progress, the former is analyzed in our simple model, and the output elasticities of supply of the two activities X and Y implied thereby are investigated.

Let a and b be the amounts of the two factors employed in industry X and a', b' the amounts employed in industry Y. The prices, p_x and p_y of X and Y respectively, are assumed to be constant throughout the analysis. $a+a'=A$ and $b+b'=B$ where A and B are the total factor endowment enjoying full employment. It is assumed that B is constant. Therefore, $db+db'=0$. A is assumed to change infinitesimally so that $da+da'=dA$. The production functions are assumed to be linear and homogeneous and remain unchanged throughout the analysis. We can now proceed to analyze the impact of the change in A on the output of Y as follows:

From equilibrium conditions, we have

$$p_x \cdot \frac{\delta X}{\delta a} = p_y \cdot \frac{\delta Y}{\delta a'} = \Pi_a$$

$$p_x \cdot \frac{\delta X}{\delta b} = p_y \cdot \frac{\delta Y}{\delta b'} = \Pi_b$$

[13] This is best seen by rewriting the criterion thus:

$$\left(\frac{Y}{M} \cdot \sigma + \frac{C}{M} \cdot \epsilon + y + r_m \right) < 0$$

For further discussion of the economic implications of this criterion, see Bhagwati [1].

[14] Our substitution elasticities are not identical with, though similar to, the elasticities of substitution in the literature. See Morrissett [8].

Differentiating these, with p_x and p_y constant, and then using the relations $da+da'=dA$ and $db+db'=0$, we get:

(10) $\left(p_y \cdot \dfrac{\delta^2 Y}{\delta a'^2} + p_x \cdot \dfrac{\delta^2 X}{\delta a^2}\right) \cdot da' + \left(p_y \cdot \dfrac{\delta^2 Y}{\delta a' \delta b'} + p_x \cdot \dfrac{\delta^2 X}{\delta a \delta b}\right) db' = p_x \cdot \dfrac{\delta^2 X}{\delta a^2} \cdot dA$

(11) $\left(p_y \cdot \dfrac{\delta^2 Y}{\delta a' \delta b'} + p_x \cdot \dfrac{\delta^2 X}{\delta a \delta b}\right) \cdot da' + \left(p_y \cdot \dfrac{\delta^2 Y}{\delta b'^2} + p_x \cdot \dfrac{\delta^2 X}{\delta b^2}\right) \cdot db' = p_x \cdot \dfrac{\delta^2 X}{\delta a \delta b} dA$

Using equations (10) and (11) and the identity

$$p_y \cdot \frac{dY}{dA} = \Pi_a \frac{da'}{dA} + \Pi_b \frac{db'}{dA}$$

and choosing units such that all prices are equal to unity, we arrive at the following formula:[15]

(12) $$dY = \frac{b \cdot Y}{(a'b - ab')} \cdot dA.$$

Some interesting conclusions emerge from this formula. First, the formula has the property that the output elasticity of supply of Y (as also X) is independent of the scale of the two activities, X and Y, and depends exclusively on the factor proportions in the two activities. This is easily demonstrated by rewriting the formula thus:[16]

[15] Since the change in A is assumed to be infinitesimal, formula (12) can be derived much more readily by using the Samuelson theorem [12] that the relationship between commodity and factor price ratios is unique under the conditions postulated. J. Black informs me that the following alternative proof is available, if the Samuelson theorem is used: Assume, by choice of units, that all prices equal unity.

$$da + da' = dA \cdot \quad dc' = \frac{a'}{(a' + b')} \cdot dY.$$

Similarly,

$$dY = \frac{(a' + b')}{b'} \cdot db' = - \frac{(a' + b')}{b'} \cdot \frac{b}{(a + b)} \cdot dX \cdot (db = - db').$$

Therefore,

$$da = \frac{a}{(a + b)} \cdot dX = - \frac{ab'}{b(a' + b')} \cdot dY.$$

Therefore,

$$dA = da + da' = - \frac{ab'}{b(a' + b')} \cdot dY + \frac{a'}{(a' + b')} \cdot dY.$$

Therefore,

$$dY = \frac{b(a' + b')}{(a'b - ab')} \cdot dA = \frac{b \cdot Y}{(a'b - ab')} \cdot dA.$$

The analytical method employed in the text, however, is more general and can be used for other similar problems where nothing comparable to the Samuelson theorem is available.

[16] This property is geometrically demonstrated in a different context by Mundell [9].

$$(13) \qquad dY = \frac{1 + \dfrac{a'}{b'}}{\left(\dfrac{a'}{b'} - \dfrac{a}{b}\right)} \cdot dA$$

Further, the familiar Rybczynski [10] proposition that the output of the B-intensive industry will contract, under the assumptions made here, when the supply of A increases, at constant relative commodity prices, follows quite readily from (12). If Y is B-intensive, it follows that

$$\frac{a'}{b'} < \frac{a}{b}$$

and thus $a'b < ab'$. Under our assumptions, therefore, it can be established that dY/dA is negative if Y is B-intensive. It follows then that E_{SY} (and y) may be negative, indicating that the domestic output of importables declines absolutely, at constant terms of trade, as a result of the expansion.

The analytical technique outlined above is perfectly general and can be utilized for determining the output elasticities of supply under other types of expansion as well, such as neutral technical progress in an activity. It would thus be feasible to undertake an interesting taxonomic exercise: to consider different varieties of expansion and investigate the output elasticities of supply (and of demand) implied by them; and to relate them with different values for the substitution elasticities to discover the full impact on the terms of trade of expansion under different circumstances. Such an analysis, however, cannot be attempted here as the task would take us far afield.[17]

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UNITED STATES FOREIGN TRADE POLICY

A Review Article

By J. M. Letiche*

The stark fact of international trade policy is that its leadership has in the last few years been transferred from the United States to Western Europe. The reasons for this transfer have been neither simple nor singular, but a review of the voluminous literature on the recent renewal of The Trade Agreements Act reveals the major causes of this development as well as its relevance to contemporary conditions.[1]

I. Trade Agreements Extension Act of 1958

Public Law 85-686 marks the eleventh time that the Congress has extended the President's authority to negotiate reciprocal tariff reductions with foreign countries since its original enactment in 1934. Previous extensions had been for 3 years or less; the current one is for 4 years—through June 30, 1962. The President is empowered to reduce tariffs to the lowest rates obtainable by any one of three alternative methods: (1) The July 1, 1958 rates may be lowered by 20 per cent. The reductions would have to be made gradually: 10 per cent of the total reduction would generally be the maximum in any one year. (2) The July 1, 1958 rates may be lowered by 2 percentage points. Such reductions would also have to be made gradually: no reduction of more than 1 percentage point would generally be made effective in any one year. This alternative authority, which has not been provided for in previous legislation, would be significant in cases where 2 percentage points would permit a larger reduction than the maximum reduction obtainable under the first method. Thus, if the July 1, 1958 tariff rate on a commodity were 5 per cent, the second method would permit a reduction to 3 per cent, whereas the first method would permit a reduction to only 4 per cent. (3) The July 1, 1958 rates which were higher than 50 per cent ad valorem may be lowered to that rate. Similar authority was granted in previous trade agreements legislation. Reductions under this method would also have to be made gradually and seriatim. One-third of the total reduction is the maximum that may be made effective in any one year. This alternative method would be significant in cases where rates exceed 62 per cent, for it would permit a greater reduction than that obtainable under the first method.

Regardless of the form of tariff reductions. in no case may there be more

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[1] The volumes under review [5] [7] [1C] [11] [12] [13] [14] are staggering in length. If the usefulness of Congressional hearings is not to be further impaired, more effective procedures which economize on testimony will have to be devised.

than 4 stages; nor may separate stages be less than a year part; and the last stage may not be later than 3 years after the first. If the authority to reduce tariffs is not put into effect before July 1, 1962, the power to do so does not automatically lapse. The President's authority to enter into new trade agreements expires on July 1, 1962, but he may enter into agreements before this date with the proviso that the full authorized reduction authority shall be put into effect thereafter. However, no part of a reduction may be put into effect for the first time after June 30, 1966.

P.L. 85-686 provides for the continuation and strengthening of the safeguards for American industry and labor against serious injury from imports. As in previous trade agreements legislation, the proposed changes in U. S. tariff rates would be subject to the peril-point provisions. These provisions direct the President, before entering into negotiations, to furnish the Tariff Commission with a list of articles, together with their tariff rates and other customs treatment, which may be considered for modification. The Tariff Commission, in turn, is directed to make an investigation and report to the President its findings as to (1) the peril-point or rate below which U. S. duties may not be reduced without resulting in serious injury to domestic industry; and (2) the minimum increases in U. S. duties or additional import restrictions required to avoid serious injury. The new law extends the period within which the Tariff Commission has to make its investigations and to report to the President from 120 days to 6 months so that the Commission may make more extensive investigations. In addition, the law amends the peril-point provisions by directing the Tariff Commission, as previous legislation did not, to institute an escape-clause investigation promptly whenever it finds any article on the list upon which a tariff concession had been granted in the case of which an "increase in duty or additional import restriction is required to avoid serious injury to the domestic industry producing like or directly competitive articles" [7, p. 3, Sec. 4 (b)]. A new provision states that in each such investigation the Tariff Commission shall, to the extent practicable and without excluding other factors, ascertain for the last calendar year preceding the investigation the average invoice price on a country-of-origin basis at which the foreign article was sold for export to the United States and the average prices at which like or directly competitive domestic articles were sold at wholesale in the principal markets of the United States.

The escape-clause provisions put into effect by previous legislation are also continued, but with several new amendments. Under previous legislation, the Tariff Commission was required, upon application of an interested party, to make an investigation to determine whether imports of a particular commodity subject to a trade agreements concession were causing or threatening serious injury to a domestic industry. If the Commission found evidence of injury, it was required to submit a report to the President within 9 months with a recommendation for remedial action. The President made the final determination regarding the acceptance of a Tariff Coommission recommendation that a duty be raised or a quota be imposed on imports. These procedures are maintained under the new law except that it provides for Congressional review when the President rejects recommendations of the Tariff Commission

for import restrictions to shield domestic industries claiming injury from competition. It empowers Congress to reverse the President in such cases by two-thirds majorities in both houses. Another amendment reduces the time that the Tariff Commission has to make escape-clause investigations and reports thereon from 9 to 6 months. The powers of the Tariff Commission to obtain information by subpoena, and related powers, are extended and expanded. Furthermore, the law now provides the President with authority in escape-clause cases to impose a duty as high as 50 per cent ad valorem on a duty-free item which has been bound in a trade agreement. Previous legislation forbade transfer by the President of articles from the free list to the dutiable list. Finally, as regards eligibility to file an application for an escape-clause investigation, the Trade Agreements Extension Act of 1951 is amended by striking out the phrase "any interested party" and substituting "any interested party (including any organization or group of employees)" [7, p. 4, Sec. 5 (a)]. This provision was added in order to make it clear that such bodies could make application even though management was not a party to the application.

The national security amendment of the previous legislation has been stiffened rather than liberalized. Under the terms of the amendment, the President may not decrease duties on imports, but may restrict imports that are found to threaten the national security. More detailed standards and criteria are provided for the guidance of the President and the Director of the Office of Defense and Civilian Mobilization (ODCM):

> For the purposes of this section, the Director and the President shall, in the light of the requirements of national security and without excluding other relevant factors, give consideration to domestic production needed for projected national defense requirements, the capacity of domestic industries to meet such requirements, existing and anticipated availabilities of the human resources, products, raw materials, and other supplies and services essential to the national defense, the requirements of growth of such industries and such supplies and services including the investment, exploration, and development necessary to assure such growth, and the importation of goods in terms of their quantities, availabilities, character, and use as those affect such industries and the capacity of the United States to meet national security requirements. In the administration of this section, the Director and the President shall further recognize the close relation of the economic welfare of the nation to our national security, and shall take into consideration the impact of foreign competition on the economic welfare of individual domestic industries; and any substantial unemployment, decrease in revenues of government, loss of skills or investment, or other serious effects resulting from the displacement of any domestic products by excessive imports shall be considered, without excluding other factors, in determining whether such weakening of our internal economy may impair the national security [7, pp. 6, 7, Sec. 8(c)].

A provision has been inserted requiring that the Director of the ODCM issue regulations for the conduct of investigations under this section. In addition, procedural changes have been made toward eliminating multiple investigations possible under previous legislation and requiring that the

ODCM publish a report on the disposition of each national security case. The Director, with the approval of the President, shall also submit to the Congress a report on the administration of investigations and action to prevent a threat of impairment to national security.

The new law authorized the President to raise duties as much as 50 per cent over the rates which existed July 1, 1934. In the case of specific duties the President is empowered to convert such duties to their July 1, 1934 ad valorem equivalent, using 1934 values, and to increase such equivalents 50 per cent. Previous legislation provided the President with authority to increase rates as much as 50 per cent over the rates existing on January 1, 1945. Since the 1934 rates were substantially higher than the 1945 rates, this change in the base date increases the extent to which duties on such items can be raised.

The law continues the requirement that the President submit to the Congress an annual report on the operations of the trade agreements program, but in addition, directs him to include in such reports a statement on results of action taken to obtain the removal of foreign restrictions, including discriminatory practices against U. S. exports and the measures available for seeking the removal of such remaining trade barriers. It declares it to be the sense of the Congress that during the course of negotiating a trade agreement, the President should seek information and advice from representatives of American industry, agriculture, and labor.

II. *The Hearings*

Prior to this extension of the Trade Agreementts Act, U. S. foreign economic policy underwent thorough Congressional evaluation. As compared with previous *Hearings*, those under review reveal a critical sense of relevance and urgency. Numerous executives, representatives of the press, government, and private associations furnished responsible testimony concerning the national interest. More than 30 university economists were invited to prepare papers on the objectives, administration and operation of domestic and foreign economic policy. With one exception, these papers were carefully prepared, representing the highest quality of professional work. But the resulting legislation is disappointing; it is not to be matched with the need, even though it represents victory in a hard-fought battle by Congressional advocates for a more liberal foreign-trade policy.[2] Powerful pressure groups have been successful in obtaining so many exceptions to our trade program that its effectiveness has been seriously undermined. If left unchecked, this trend may make it very difficult to maintain the political and economic solidarity of the noncommunist world.

The agenda of the Subcommittee on Customs, Tariffs, and Reciprocal Trade Agreements included testimony on U. S. trade policy and the national interest; on the emerging pattern of foreign trade and payments; on foreign commercial policies and their relation to U. S. economic activity, with particular emphasis

[2] The bill was approved by the House Ways and Means Committee in an 18-to-7 vote. Committee Democrats voted 14-to-1 for the measure, Republicans 6-to-4 against. In a standing vote, the bill was passed in the House 161-to-55. In the Senate, it was passed 72-to-18; Democrats voted in favor 40-to-6, Republicans 32-to-12.

on the escape clause and peril-point procedures, the national security amend-ment, antidumping, countervailing duties, unfair competition, quotas, the Buy-American Act, section 22 of the Agricultural Adjustment Act, and restric-tive trade practices engaged in by foreign countries.

The testimony furnished by representatives of government agencies is most clearly indicative of the complex forces that must be harmonized in the formation of United States foreign trade policy. Thus the basic work of preparing the list of concessions to be requested from foreign countries is the responsibility of the Department of Commerce. But the work of preparing the list of concessions to be granted by the United States is primarily the responsi-bility of the Tariff Commission. However, as soon as the Tariff Commission makes its list available to Commerce (through the country committees), the Bureau of Foreign Commerce and the Business and Defense Services Adminis-tration undertake independent studies to determine which of these items should be dropped from this list. The problem of reconciling disagreements between agencies is to be matched only with that of reconciling disagree-ments within an agency. It would be surprising, for example, if within the De-partment of Commerce conflicting interests were not represented by the Bureau of Foreign Commerce, on the one hand, and the Business and Defense Services Administration, on the other. Nonetheless, it is commendable that the differences which arise are satisfactorily resolved. The Department of Commerce presents a reasonably consistent position in the field of foreign trade. In providing testimony, it is the over-all national interest that appears to predominate. The domestic and international facets of various measures are carefully weighed and, on the whole, well integrated. Moreover, the techni-cal work of the Department of Commerce is exemplary; it provides objective information, powerful analysis, and relevant background papers for Congres-sional, and other use.[3]

Considering the inconsistencies between international trade policy and agri-cultural policy in the United States, the testimony of representatives of the Department of Agriculture, and of farm associations, deserves notice. The Secretary of Agriculture not only recommended the extension of the Trade Agreements Act in 1958, but has continually advocated proposals to reduce the area of conflict between trade and agricultural policy. Hence, since 1954 the President has made proposals to Congress that the basic provisions of the Agricultural Act of 1949 be allowed to become effective; viz., that legislation be passed establishing variable price schedules and a modernized parity formula. He has also made the proposal to insulate from the market a large part of the existing government-held surpluses by creating an emergency re-serve, together with a provision that might make it possible to use the variable price schedules for the basic commodities (cotton, wheat, rice, corn, peanuts, and tobacco) and to rely less upon the use of acreage allotments and market-ing quotas. The testimony of government officials, representatives of farm organizations, and agricultural economists reveals that although the adoption,

[3] Cf. the testimony by Sinclair Weeks, Henry Kearns, R. E. Simpson and J. J. Schalet, [12, Pt. 2, pp. 2713-2831]; Harold P. Macgowan and Harold C. McClellan [10, Pt. 3, pp. 1306-26]; [11, pp. 23-37, 303-16, 385-442].

in part, by Congress of the President's proposals was a step in the right direction, the task of reconciling agriculture and trade policies requires a sharper break with past agricultural price policies than has been involved in these proposals.[4]

The State Department has labored incessantly to reduce the inconsistencies in our foreign trade policy. But its testimony in the volumes under review is wanting in confidence and direction. It is based on justifiable fear that restrictive amendments might compromise the important provisions of the Reciprocal Trade Agreements program: that the recommendations of the Tariff Commission should not become mandatory; that quotas should not be imposed on the importation of oil, textiles, lead, and zinc; that beggar-my-neighbor policies should not be put into effect during recessions; that the renewal of the Act should not be of short duration. Thus the provisions empowering the President to raise tariffs to a higher level than at any time since 1934 were originally introduced by the State Department in the hope of preventing the imposition of new quotas. But experience in the case of lead and zinc has recently proven this compromise to be of no avail. The strongest criticism that can be legitimately directed against the State Department in the foreign-trade field is that events have too often been permitted to rule policies rather than the other way around. This appears to be particularly true of our trade relations with Canada and Latin America. Given the importance of United States trade with these countries, there is an urgent need for the establishment of joint agencies between the United States and Canada, and also with Latin American countries, charged with the duties of keeping under constant survey economic matters of common interest.[5]

Owing to the lack of adequate information, United States government agencies have even found it difficult to implement national policies. The Presidential Advisory Committee on Energy Supplies and Resources Policy, for example, has recommended to the President that the 1954 ratio of oil imports to domestic production of oil should be maintained, if possible by voluntary individual action of importers. The Committee further recommended that if this ratio should not be maintained, appropriate governmental action should be taken. The Director of ODCM has been charged with the responsibility of coordinating this program. To do so, he has required information on actual and planned imports of oil. In the past, such information has been unavailable. For the guidance of government policy in such matters and, more generally, in attempting to maintain reasonable equilibrium in our international accounts, information on planned imports and exports is essential. I believe, in fact, that tentative budgets of this kind should be developed for our entire international accounts.[6]

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[4] E. T. Benson testified that in the fiscal year 1957 exports of agricultural products from the United States totaled $4.7 billion, of which only $1.7 billion were sold in ordinary commercial markets. I.e., more than 60 per cent of our agricultural exports were the result of some form of government assistance [12, Pt. 1, pp. 121-47]; cf. also the papers by O. B. Jesness, J. C. Lynn, and Lawrence Witt [10, Pt. 1, pp. 391-416, 533-49] and those by D. G. Johnson, E. T. Baughman, J. C. Lynn, and J. G. Patton [11, pp. 679-760].

[5] For the testimony of the State Department see in particular [10, Pt. 2, pp. 1070-1227].

[6] See the testimony of A. S. Fleming [10, Pt. 3, pp. 1247-50].

The Department of Labor and representatives of trade unions have generally supported the Trade Agreements Program, but their evidence suggests apprehension about the future. The difficulties have been primarily related to the textile industry in New England and the newly developing industries in the South. The Director of Research of the Textile Workers Union of America testified that, "we have come, in many industries and certainly in the textile industry, to the point of great danger; sufficiently so that the political parties, the Democratic Party on the one hand and the Republican Party on the other have learned that we can no longer expect a sweeping change of duties and that we must reexamine the effects of some of the duty changes in the past under these agreements."[7] He added that the change in the 1956 Democratic Party platform with respect to trade policy represents a very radical departure from the past and his union, among others, urged this type of change. The Director of Research of the AFL-CIO staunchly supported the Reciprocal Trade Agreements program. As an alternative to raising tariffs, he recommended that the President should be authorized to provide certain types of governmental assistance to communities, workers, and employers adversely affected by import competition. He provided an excellent summary of the particular forms that such assistance might take.

Among the many business executives who gave useful testimony, the statement of Charles H. Percy, President of the Bell & Howell Co., is the most stimulating. The paper illustrates the dynamic process whereby successively higher levels of mechanization raise levels of real income and provide the stimulus for constant innovation, adjustment and growth [10, Pt. 2, pp. 691-705]. The massive testimony dealing with problems of specific firms—as well as the Hearings as a whole—suggests the need for an independent body which could objectively appraise these matters for Congress.

Of the university economists Edward S. Mason and Willard L. Thorp presented two particularly informative, judicious papers on the relation of trade policy to the national interest. Mason pointed out that subjecting domestic business to a greater degree of foreign competition would, over time, lead to a transfer of resources from low-productivity to high-productivity uses and thus increase efficiency in the American economy. By reason, however, of the smallness of our present and potential dependence on imports, he maintained that this tendency would not be large. "Obviously this is worth doing if the transfer costs are not excessive, but there is no use expecting from a change in trade policy a very large contribution to efficiency and the rate of economic growth," [10, Pt. 1, p. 9]. Nonetheless, he observed that with increasing dependence on foreign sources of mineral supplies, the range of action open to trade policy may have a substantial impact on efficiency and growth rates. With respect to iron ore, copper, lead, zinc, and oil, he noted that if we attempt to be more largely self-sufficient than we are now, it can only be at the expense of sharply rising costs. We are close to the end of our high-grade

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[7] Cf., the testimony of Solomon Barkin [10, Pt. 2, pp. 738-54 and especially p. 746]; and that of S. H. Ruttenberg [10, Pt. 2, pp. 915-31, especially pp. 920-21]. See also the informative paper by I. B. Kravis on the role of wages in trade patterns [10, Pt. 1, pp. 297-331].

supplies of Mesabi iron ore. New sizable discoveries of copper, lead, and zinc in the United States have been few and far between in recent years. The 11-barrel-a-day average of American oil wells as against the 200-barrel-a-day average in Venezuela and the 5000-barrel-a-day average for the Middle East gives some indication of the relatively high real cost of current American oil production. Moreover, Mason referred to the ample evidence that costs of oil discovery in this country are rising. He drew attention to the fact that despite the relatively high current cost of United States mineral production and the expectation that it will rise rapidly in the future, there is continuous pressure from producer groups for a reduction of imports. He concluded that this can, over the long run, lead only to a curtailment of efficiency and a reduction in the rate of economic growth in the United States.

Mason and Thorp both stressed that an expanded opportunity for other countries in the free world to earn dollars by exports to the American market would facilitate cooperation in the interests of common defense. But they cited evidence to the effect that an expansion of American imports under the most favorable short-run conditions is unlikely to produce in dollar earnings more than a fraction of what is currently made available under foreign-aid programs. Nevertheless, they agreed that considerations of efficiency and mutual defense justify a persistent attack on our own as well as other trade barriers. Mason cautioned that the attack must be selective, since a displacement of resources much faster than they can be reabsorbed into employment is a doubtful contribution to efficiency and to mutual defense. Adverse repercussions, he said, are likely to be least in expanding industries in which multiproduct firms predominate. In the older, relatively stagnant, nondurable-goods industries, he thought progress toward trade liberalization should be much slower.

Thorp emphasized that the basic issue involved is between the long run and the short run. For our long-run economic benefit, we must accept the costs of disturbance of a great many short-term adjustments. The trade agreements program, he noted, has been responsibly administered: "I think it is surprising that there have been so few cases where problems have arisen. It shows that the situations have been carefully evaluated by our negotiators" [10, Pt. 1, p. 88]. In a growing economy, he found it difficult to believe that we need to give special attention to minor adjustments which might result from continuing the gradual liberalization of our trade policy. He drew attention to the fact that during the last several years, there has been an obvious increase in the intensity of demand for increased protection by various interested individuals and industrial groups. Even state legislatures have imposed discriminatory restrictions against foreign goods, especially goods from Japan. The greatest difficulty in the trade field, he asserted, has been the special case. The hardest issues we are likely to face are those where the general interest, widely diffused, must be balanced against some limited special interest, concentrated and intense.

Mason and Thorp considered the argument that readiness for war requires the protection of domestic production to be, in general, a weak one, though this proposition may permit of one or two exceptions. The situations that

are usually cited as justifying tariff protection for national defense deal with strategic materials or with types of production facilities difficult to construct or expand or skills that are difficult to acquire. For materials capable of stockpiling—and most strategic materials are—both men found stockpiling in peacetime from low-cost sources, either domestic or foreign, much to be preferred to protection of domestic output, which means procurement from high-cost sources both in peace and war. It was observed that the existence of an adequate stockpile would make it unnecessary in wartime to divert scarce manpower away from military service and into the wartime production of strategic materials. No case could be found that makes sense for protection of any domestic metal on security grounds. Oil does represent a special case [10, Pt. 1, pp. 12, 351-75, 491-507; Pt. 3, pp. 1676-82] [11, pp. 1059-87] [12, Pt. 1, pp. 1026-1138].[3]

As regards skills of various sorts, it was shown that the transferability of technical skills in a highly industrialized economy demanding precision work of all types, is such as seriously to weaken the claims to indispensability of any particular group. The ease with which the supposedly difficult task of lens-grinding, for example, was subdivided and taught to rank amateurs during the war, suggests that in time of need almost any skill can be quickly acquired. Recent evidence has revealed that, on defense grounds, the case for tariff protection of the watch industry has been excessively weak [8] [9]. The manufacturing industries required for military production are in the main our export industries. It is our imports that are marginal and they can readily be reduced in time of emergency without great loss. Mason and Thorp were very skeptical about the claims that protection would give us more security. They believed that increased imports of raw materials—and the consequent husbanding of our own resources—the stimulus from foreign

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[3] Approximately 40 per cent of our total energy requirements are currently met from petroleum sources, and our European allies are increasingly dependent on oil to meet their energy needs. About 15-20 per cent of United States oil consumption is covered by imports. Most of these imports come from Venezuela which, in the event of war, must be judged a relatively safe source. Europe imports almost all its oil from the Middle East. Experts generally agree that we need a protective reserve representing the difference between current Western Hemisphere production and what could be produced in an emergency. Our present system of oil proration plus tax incentives, together with official and unofficial limitations to imports, is designed to provide this protective cushion. The cost of maintaining this cushion is very high; the important question is whether this cost is not excessive as compared with possible alternatives. The Royal Commission on Canada's Economic Prospects has complained that the depletion allowances received by American oil companies under the American income-tax laws constitute an unfair handicap to Canadian-controlled companies in the exploration and development of oil fields, and it recommends that corresponding concessions, though not to the same extent, be granted under Canadian income-tax law to Canadian-controlled companies. "To the extent that they exceed the actual expenditures on exploration and development," states Jacob Viner, "the American depletion-allowances are an outright scandal. I doubt, however, whether they apply to operations in Canada via Canadian-chartered companies. The Commission's objective, in any case, could be met without extending the American corruption to Canadian income-tax law if the excess American depletion allowances were restricted to operations within the United States. It may be that a suggestion along these lines from Canada would meet with a sympathetic reception in Washington. But I wonder whether Alberta feels unhappy about what is in effect American subsidization of Albertan oil-development?" [4, pp. 316 et seq.].

competition, and the industrial base supported by exports would tend to strengthen our economy.

With respect to American manufacture in general, Mason took his stand with the report of the Randall Commission: "If the maintaining in being of a section of American industry is sufficiently important to the national defense to make the Defense Department willing to include its cost of maintenance in the defense budget, tariff protection might well be considered as one of the methods of maintaining this industry" [10, Pt. 1, p. 13]. Thorp preferred to deal with weak spots in our mobilization base directly by devices such as government contracts, rather than to use the uncertain and blunt instrument of trade barriers. When a security requirement is perfectly clear, he thought the device used to meet it should be certain. But he reiterated the view that defense preparedness cannot be viewed solely in domestic terms. Our defense program depends today upon a number of interlocking commitments with other countries; in large measure it is a program of common defense. Trade restrictions which are proposed by the advocates of increased protection would be against these same countries. In emphasizing the more general point that our trade policy is less important with respect to our own economy than it is in connection with our influence in the world, he found it unnecessary to belabor the importance of the nonmilitary foundations of military preparedness.

Seymour E. Harris, Arthur Marget, and Judd Polk contributed papers on the emerging pattern of trade and payments. Harris paid particular attention to the dollar gap; he felt this to be the crucial problem that must be solved before we tackle broader issues. Marget, in a paper on "The Mechanism of the Balance of Payments," discussed the improvement in the kind of economic policies—fiscal, monetary, and commercial—which the countries of Europe had come to adopt increasingly in the postwar period. He showed that even while the European recovery program was in progress, the general trend of commercial policy was in the direction, not of increasing the level of direct restrictions on imports in general and of restrictions on imports from the dollar area in particular, but in the direction of relaxing such restrictions. This trend has been more marked since the end of the European recovery program.[9] What is particularly significant is the high degree of external balance which has been maintained at progressively higher levels of production and trade, and with progressively less reliance on direct controls as a way of maintaining that balance. Although Marget rightly stressed the importance of improved fiscal and monetary policies to economic growth and external balance, he would doubtless agree that the remarkable economic growth of Western Europe has played a key role in the reduction of their trade barriers. I am convinced that the pressure of economic growth will be an outstanding factor contributing to the expansion of European trade over the coming decades.

As Polk shows in his excellent paper on "The Sterling Area," world trade has come to be dominated by big national markets. From 1938 to 1958 world trade has expanded more rapidly than world output; but domestic markets

[9] See the testimony by Warren Shearer and the appended O.E.E.C. document *Liberalization of Europe's Dollar Trade*, [10, Pt. 1, pp. 154-297] and [3, Ch. 2].

predominate in size and importance. The domestic market for British producers now is about three times as large as their international market. The U. S. market, in turn, is about four times the size of the entire market of the sterling area. Hence, the major trading nations, without exception, find that priority in policy must be given to their national markets. "The significance of this fact for international finance," writes Polk, "is that the dominance in the international market of a nation whose basic economic orientation is inward has impeded the development of an international credit structure suitable to meet the fluctuations of international trade" [10, Pt. 1, p. 137].

On the basis of the evidence presented by Polk, one can but conclude that, as of the present, the sterling system has more elements of strength than weakness. Nonetheless it cannot finance more than a declining sector of world trade, the volume of which is now larger than the entire production of the sterling area. Since the United States began to replace Britain as the pivotal country in world trade, the trend has been toward the consolidation of the sterling "region" in a historical sense. The region has scattered, the political and economic relations among the members have undergone continual change —as have the relations with outsiders—and yet this trend has continued. It has not reflected a deliberate belief on the part of the British in the monetary, political, or economic virtue of a regional system, but rather, an understandable effort to resist the shrinkage of sterling's once worldwide scope. In short, the trend has been a defensive one against the disintegration of the international position of sterling.

Since the second world war a similar powerful force has been at work with respect to foreign trade among the six nations that have signed the treaty forming the European Economic Community (Belgium, Federal Republic of Germany, France, Italy, Luxembourg, and the Netherlands).[10] As political and economic ties of these countries with their overseas territories have been severed or have corroded, they have sought a more stable European common market. The political forces have been even more powerful in impelling Europe to unite. The six nations have agreed that, among other things, they will gradually eliminate their trade barriers within the Community and adopt a common external tariff applicable to imports from countries outside the Community. By January 1, 1959, they are to take the first step in reducing their tariffs by 10 per cent. The second 10 per cent cut is to be introduced July 1, 1960. But the really significant beginning toward the consolidation and establishment of a common tariff is scheduled for January 1, 1962. Hence, the years 1959-1962 provide a breathing spell; it may well be a critical period of decision and transition in Western foreign trade policy.

The U. S. government has rightly, I think, supported the creation of the European Economic Community. However, if it is not to become a divisive force in Europe, it is now important that the United Kingdom and other countries of the Organization for European Economic Co-operation be able

[10] The compendium of papers on United States foreign trade policy contains articles analyzing this treaty, and factors incident to it, by the United States Department of State, United States Department of Commerce, Miriam Camps, R. F. Mikesell, Gottfried Haberler, and Robert Triffin [11, pp. 361-477]. Cf. also the background article by Marc Ouin [2].

to associate with it. The *Hearings* and *Reports* of the Committee on Ways and Means indicate that the Congressional approval of the 4-year extension of the Trade Agreements Act, with the new negotiating authority, was granted in no small measure to enable the administration to deal with these emerging developments. The general impression one obtains from comparing the testimony reported in these publications and the resulting legislation is that the U. S. Congress can be relied upon to pass responsible legislation in this field whenever the American national interest and her international responsibilities are made reasonably clear. But the testimony also reveals that the pressures upon individual Congressmen with respect to the multifarious "special cases" are overwhelmingly strong. For this reason the U. S. Congress is not well suited to deal with the particular problems of specific commodities or local dislocations. The restrictive provisions of P.L. 85-686 bear witness to this fact; and for the same reason its implementation may not correspond with the need.

For the effective implementation of U. S. foreign-trade policy, strong and consistent leadership from the administration is a primary requisite. In effect, leadership in this field has recently been transferred from the United States to Western Europe because a vacuum has been created. We have espoused a more liberal trade policy on the one hand, and imposed new quantitative restrictions on the other. The U. S. Tariff Commission was at one time the most outstanding government agency of its kind. Now it consists of a majority of protectionist appointees and is championed by the American Tariff League. At least since 1952, the U. S. tariff has in fact not been more restrictive than those of the larger European countries.[11] The escape-clause and peril-point provisions have been judiciously administered. But the instability of the American market and especially the uncertainty concerning American foreign trade policy during recessions—has been at the root of the difficulty.

The evidence on the long-term trend of U. S. foreign trade and payments suggests that strong underlying forces are operating toward equilibrium in our international accounts. But the testimony furnished in the *Hearings* under review indicates that protection will be accorded to the weak segments of the American economy. The only question is whether it will be administered in a manner which is conducive or inconducive to the long-term economic growth and international equilibrium of the United States as well as its friends and allies.[12] Congress has provided the President with legislation which, if liberally interpreted, can be used as a catalyst to integrate the trading policies of the

[11] For 10 countries, the unweighted average of the 1952 percentage tariff rates on the same 78 representative commodities (including foodstuffs, raw materials, semifinished goods and manufactures) were as follows: Italy 24, France 19, Austria 17, United Kingdom 17, Germany 16, United States 16, Canada 11, Benelux 9, Sweden 6, Denmark 5. Calculated by the Secretariat to the General Agreement on Tariffs and Trade. [6, p. 62]; also cited in [1, p. 421].

[12] Structural shifts and recent trends in the United States balance of payments are discussed in an article by Fred H. Klopstock and Paul Meek, and in a paper by the United States Department of Commerce [11, pp. 303-17]. The problems of internal and external adjustment are dealt with in the same publication by Samuel Lubell, C. P. Kindleberger, Stephen Enke, Howard Piquet, W. S. Salant, D. D.•Humphrey, Klaus Knorr, D. G. Johnson, S. H. Ruttenberg, and I. B. Kravis.

noncommunist world. Just as we have given out support to the establishment of the European Economic Community, so it is now urgent that we provide the leadership in the General Agreement on Tariffs and Trade Organization by ratifying the Organization for Trade Cooperation and supporting Britain and those other members of the OEEC who are genuinely striving to create a reasonably free outward-looking common market. Not only would this serve the best interest of Europe and the United States, but it could help to solidify our respective trade relations with Canada, Latin America, and the newly developing countries of Africa and Asia.

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COMMUNICATIONS

The Wage-Push Inflation Thesis, 1950-1957

In the period 1950-1957 between the two most severe downturns in the American economy since the second world war, two economic phenomena of great importance stand out: (1) a sustained period of relatively full employment, and (2) a persistent tendency towards substantial price inflation. The latter has produced a good deal of soul searching on the part of professional economists, and various explanations have been developed to account for the steady rise in the price level. One such explanation, the "wage-push" inflation thesis, forms the subject matter of this paper. Simply stated, the wage-push inflation thesis holds that money wage rates have increased more rapidly than physical productivity and, consequently, have exerted upward pressure on costs and prices.[1]

There can be little quarrel with the view that money wage rates have, in recent years, risen more rapidly than physical productivity. Table 1 presents

TABLE 1.—MANUFACTURING WAGES PER HOUR AND MANUFACTURING
PRODUCTIVITY PER HOUR, UNITED STATES, 1950-1957[a]
(1950 = 100)

Year	Manufacturing Wages per Hour	Manufacturing Productivity per Hour
1950	100	100
1951	109	99
1952	114	101
1953	121	104
1954	124	107
1955	129	112
1956	136	113
1957	142	115

[a] The index numbers for manufacturing wages per hour were computed on the basis of gross hourly earnings for manufacturing workers. The index numbers for productivity per hour in manufacturing were computed by correcting the Federal Reserve Board index of manufacturing production for changes in total manufacturing employment and weekly hours worked in manufacturing.

Sources: *Monthly Labor Review*, Bureau of Labor Statistics and *Federal Reserve Bulletin*.

data for 1950-1957 which show that manufacturing money wage rates increased some 40 per cent in this eight-year period while physical productivity in manufacturing rose only 15 per cent. The explanation is usually stated in

[1] See E. H. Chamberlin [1] or J. M. Clark [2]. The following excerpt from Chamberlin [1, p. 28] is illustrative of this position: "There seems to be no reason to doubt that the upward pressures exerted by unions and transmitted to prices through the law of costs may well proceed (as they have recently) at a rate greater than the rate of increase in productivity for the economy *as a whole*, with a resulting general rise in prices."

one of two forms: (1) the unqualified wage-push inflation thesis: the major (if not sole) factor responsible for inflationary pressures is increasing money wage rates; or (2) the qualified wage-push inflation thesis: in addition to the responsibility of increasing money wage rates for inflationary pressures, the full-employment guarantee contained in the Employment Act of 1946 provides assurance to trade unions that any unemployment effects resulting from excessive money-wage-rate increases will be compensated for by government contracyclical policies.

This second form of the wage-push inflation idea introduces the question of the unemployment effect which may result from an excessive money-wage-rate increase and, in view of the relatively full employment experienced between 1950 and 1957,[2] suggests the basic question to be considered in this paper: What conditions would have had to prevail for the wage-push inflation thesis to be an adequate explanation of the inflation that has characterized the United States economy in the period 1950-1957?

I. The Theoretical Model

A major problem in answering this question arises from the interdependence between the general level of money wages and the level of income and employment. If the income effect of a general increase in money wages could
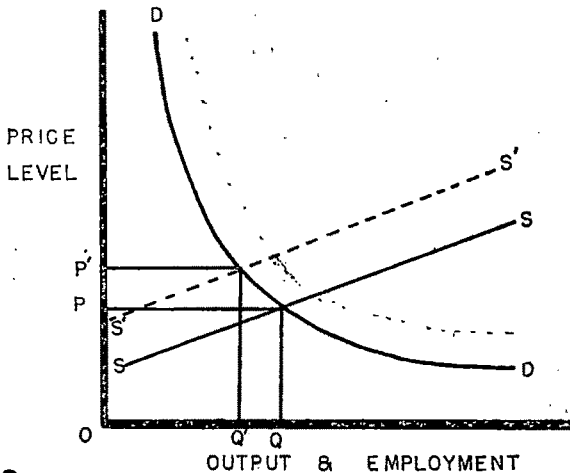
FIGURE 1. AGGREGATE DEMAND AND AGGREGATE SUPPLY OF GOODS AND SERVICES

be ignored, the analysis of the relationship between money wage rates and the level of employment could be handled in a very simple manner. By assuming (1) a fixed level of income, and (2) that the level of output and the level of employment are uniquely related, an aggregate demand function DD for the economy's goods and services could be constructed by relating the general price level to the level of output and employment (see Figure 1).

[2] The annual percentage rates of unemployment for these years were: 1950—4.98, 1951—2.99, 1953—2.51, 1954—5.01, 1955—4.03, 1956—3.78, and 1957—4.32. Source: Bureau of the Census estimates.

Such a function is necessarily of unit elasticity. An aggregate supply function *SS* can also be derived, its shape being dependent upon the conditions of marginal cost existing in the economy.[3] The result is a determinate solution for the level of output and employment *OQ* and the general price level *OP*. For the purpose of this analysis, *OQ* will be assumed to represent a full-employment situation. If we now assume an increase in the general level of money wage rates and an unchanged set of production functions for the economy, the result is an upward shift of the aggregate supply function *SS* to a position *S'S'*. This produces a higher equilibrium price level *OP'* and a lower equilibrium level of output and employment *OQ'*.

However, the assumption that a change in the general level of money wage rates has no income effect is either (1) simply a more sophisticated way of stating the wages-fund doctrine, or (2) requires that any change in the total wage bill be offset by changes in the other distributive shares. When the assumption is discarded, a family of *DD* curves must be substituted for the single curve previously employed. Each of these curves is related to a particular *SS* curve and represents a different level of income (a higher level of income is represented by a curve farther to the right.) Each set of *DD* and *SS* functions describes an equilibrium situation, and the locus generated by the various equilibrium points defines an aggregate demand function for labor *D_L* (see Figure 2).[4] A more definitive formulation of such a function is contained in a recent article by Sidney Weintraub in which he suggests three general types of *D_L* functions: (1) a Keynesian, or perfectly inelastic type, (2) a classical, or negative sloping type, or (3) an underconsumptionist, or positive sloping type [5].

If the *D_L* function is of the Keynesian type, an excessive[5] increase in money wage rates generates a rightward shift of the *DD* function sufficient to compensate for any potential unemployment effect that may have been implied in the original wage-rate increase. However, if it is of the classical type, the income effect of the wage-rate increase will be insufficient to maintain a full-employment equilibrium. In either case there will be an increase in the price level, but the increase will be less than proportional to the money-wage-rate increase in the case of the classical *D_L* function. In general, the greater the elasticity of the *D_L* function the smaller will be the price-level increase that results from an excessive money-wage-rate increase and the greater will be the unemployment effect.[6] Weintraub's underconsumptionist version of the

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[3] Rising marginal costs are assumed. Under conditions of constant marginal cost the conclusions of the analysis that follows would have to be slightly modified. The preference for rising marginal cost is based upon the belief that at near full-employment levels conditions of decreasing returns prevail. It should also be noted that the *SS* function assumes (1) a given wage rate, and (2) a given set of production functions for the economy.

[4] As used in this discussion, the term "aggregate demand function for labor" implies a function that recognizes that a change in the general level of money wage rates may have an effect upon the level of money income with consequent effects upon the position of the individual firms' demand functions for labor and the level of employment.

[5] The term "excessive" as used here denotes an increase in money wage rates with no change in the economy's production functions.

[6] If constant marginal costs are assumed, this proposition must be modified to read as

D_L function may be omitted from consideration as, under conditions of full
employment, it degenerates into the Keynesian case, except that the effect on
the price level is magnified due to a greater upward shift in the SS function.
    From this analysis it becomes obvious that the only situation in which the
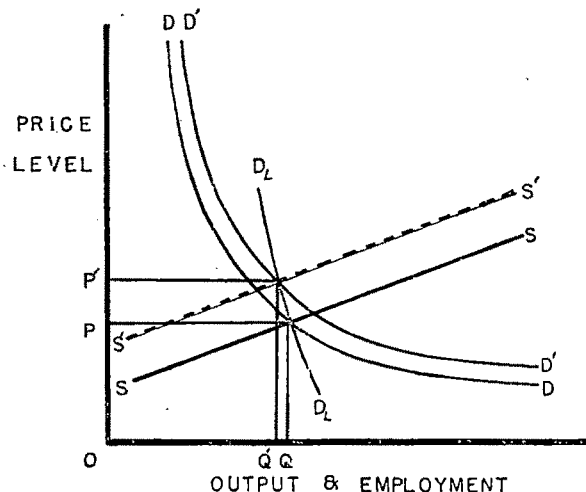
FIGURE 2. AGGREGATE DEMAND AND SUPPLY OF GOODS AND SERVICES
AND AGGREGATE DEMAND FOR LABOR

unqualified wage-push inflation thesis can be applied to the relatively full-
employment conditions of the period 1950-1957 is one in which the aggregate
demand function for labor is of the Keynesian, or perfectly inelastic, type.
    If it is believed that the aggregate demand function for labor is not per-
fectly inelastic, there must have been some exogenous factor that produced a
positive shift in the DD function sufficient to maintain full employment. The
qualified wage-push inflation thesis provides one such factor: government
policies are oriented towards inducing the compensatory shift in the DD func-
tion that is needed to offset any unemployment effect generated by excessive
money-wage-rate increases.[7] Consequently, the thesis must be valid for any
form of the aggregate demand function for labor.
    There is ample evidence that government activity has been sufficient to
produce whatever compensatory shift in the DD function may have been re-
quired to maintain full employment in the period 1950-1957. Federal expendi-
tures increased from $39.6 billion in the fiscal year 1950 to $69.3 billion in
fiscal year 1957 [6]. In the aggregate these expenditures totaled nearly $500

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follows: In general, the greater the elasticity of the aggregate demand function for labor
the greater will be the unemployment effect that results from an excessive money wage
rate increase. With conditions of constant marginal cost the excessive money wage rate in-
crease will have its full effect upon the price level.

    [7] Gottfried Haberler poses the basic problem rather aptly when he says, "If wages are
pushed up faster [than average productivity], society will have to choose between unem-
ployment and inflation" [3]. The qualified wage-push inflation thesis maintains that so-
ciety has chosen inflation.

billion in the period and exceeded receipts by some $17 billion.

Serious question may be raised, though, whether this inflationary pressure was the result of deliberate policies on the part of the federal government designed to offset possible unemployment attributable to rising wage rates. An alternative explanation would be that these large expenditures were the result of factors unrelated to the relationship between money wage rates and the employment level: for example, the pressing defense needs of the period, which accounted for a sizeable portion of the increase in spending. It is especially difficult to interpret the activities of the Eisenhower administration in the period 1956-1957 (during which time the consumer price index rose from 114.9 in January 1956 to 121.1 in September 1957) as designed to support a full-employment level in the face of excessive money-wage increases.[8] If the large federal expenditures are viewed as independent of money-wage-rate increases, a more logical explanation of the inflation is that the DD function shifted to the right as a consequence of such expenditures and induced the SS function to follow. This type of inflation has been variously called "demand-pull" or "gap" inflation [4 p. 197].

II. Conclusions

1. An unqualified wage-push inflation thesis is compatible only with a perfectly inelastic (or Keynesian) aggregate demand function for labor. Any amount of elasticity greater than this will result in unemployment effects if money wage rates increase faster than physical productivity.

2. A qualified wage-push inflation thesis is also valid under conditions of a perfectly inelastic aggregate demand function for labor, but it is valid under classical conditions of aggregate demand for labor only if it is presumed that the large increase in federal government expenditures was deliberately designed to support full employment in the face of excessive money-wage-rate increases. However, if these increased expenditures resulted from factors exogenous to the money-wage-rate employment-level relationship, it would seem that these factors should bear at least some of the responsibility for the inflationary bias in the economy.

If the classical form of the aggregate demand function for labor is accepted [5, pp. 842-46] and if it is felt that the large increases in federal expenditures in the 1950-1957 period were produced by exogenous factors,[9] the case for wage-push inflation loses much of its significance. Under these conditions, a more acceptable explanation of the phenomenon of persistent inflation is one that places its emphasis on pressures that have tended to increase the aggregate demand for goods and services in the American economy.

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The Permanent Income Hypothesis: Comment

In a critical review article,[1] H. S. Houthakker reports that an important testable implication of Friedman's theory of the consumption function[2] is contradicted by statistical data. Houthakker views his results as particularly significant because he believes the test he performed to have been suggested by Friedman.

In this paper I shall argue: (1) the test actually suggested by Friedman is an appropriate one. (2) Houthakker has not performed the test correctly. (3) When the test is performed correctly, utilizing the same data as Houthakker's, the results are entirely consistent with Friedman's permanent-income hypothesis. (4) A more powerful test, implicit in the underlying Friedman hypothesis, may be applied to these same data. (5) This more powerful test gives results strikingly consistent with the predictions of the Friedman model.

I. *Friedman's Theory and Proposed Test*

Friedman argues essentially that both income and consumption should be viewed as consisting of permanent and transitory components and that while the permanent components of income and consumption are positively related to each other, there is no correlation between transitory components or between either transitory component and the permanent component of the other variable. This means that total or "measured" consumption will vary systematically with total or "measured" income only to the extent that the variation in measured income represents a variation in permanent income. Where individuals receive a temporary increment in income one would expect an increase in their consumption only in response to the portion of that increase which, because of the addition to their wealth represented by the

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[1] H. S. Houthakker, "The Permanent Income Hypothess," *Am. Econ. Rev.,* June 1958, *48,* 396-404.

[2] Milton Friedman, *A Theory of the Consumption Function,* Nat. Bur. Econ. Research Gen. Ser. 63, Princeton 1957.

increment in income over any finite period of time, would be viewed as an increase in permanent income. The remainder of the temporary increase in income may be thought of as the transitory component of income and would have no effect on consumption.

Since Friedman argues that individuals are likely to consume a constant proportion of their permanent income, the elasticity of permanent consumption with respect to permanent income will be unity. It can then be shown to follow that when we compute regressions from cross-section data, if the mean transitory components of consumption and income are both zero the value of the elasticity of total consumption with respect to total income at the point of mean income will be an exact measure of P_y, the proportion of total variance of income that is due to its permanent component. And if linear regressions are calculated from the logarithms of consumption and income the estimated elasticity will of course be constant and will always measure P_y, regardless of the values of the mean transitory components. But, Friedman points out, "the permanent component of income is itself a resultant of a host of factors many of which are specifiable and observable, such as location, age, occupation and the like" (pp. 215-16). It follows that if we compute regressions within groups more or less homogeneous with respect to one or more of the factors affecting permanent income, regression coefficients (both those fitted to the arithmetical and the logarithmic data) will tend to be less than the regression coefficients computed from the data as a whole. Friedman thus proposes (p. 216, emphasis added) the following specific test:

> Suppose a regression were computed for a broad group of consumer units, say a sample of all units in the United States, and the corresponding elasticity estimated. Suppose this broad group were broken down into subgroups, say by the communities in which they reside, and separate regressions computed for each community. An *appropriately weighted* average of the corresponding elasticities should then be smaller than the elasticity for the group as a whole, and smaller by an amount calculable from the income data for the separate communities. The classification by communities eliminates one source of variability in permanent components, and so should reduce the variance of permanent components and hence the elasticity.

II. *Houthakker's Findings and the Nature of His Error*

It is this test that Houthakker has undertaken to perform. Utilizing some of the wealth of data provided by the budget survey carried out by the United States Bureau of Labor Statistics in 1950 and published jointly with the Wharton School of Finance and Commerce, Houthakker chose as his classifications: city class, occupation, and age of the head of the household. For each of 363 cells corresponding to the groups and subgroups so created, Houthakker calculated arithmetical linear regressions (marginal propensities to consume) and double-log regressions (elasticities of consumption with respect to income). He then compared the coefficients of each minor subgroup with those of the major group from which the subgroups had been formed. Each subgroup coefficient less than its corresponding major group coefficient

Houthakker classifies as "favorable" to the Friedman hypothesis. Where a coefficient of a subgroup is more than that of a major group, Houthakker classifies the result as "unfavorable." He then shows that "for almost every comparison the number of unfavorable cases exceeds that of the favorable cases." Comparing the coefficients of smallest cells with the coefficients computed for all households as a whole, Houthakker finds (p. 403):

Perhaps the most devastating evidence . . .: of the marginal propensities to consume in the 215 smallest subcells less than one-third [67] agree with Friedman's theory, and only 40 per cent [86] of the corresponding elasticities agree. Even if the coefficients for the minor cells had a fifty-fifty chance of being smaller than the coefficients for all households, the observed results would have a probability of less than one per cent. A fortiori we are compelled to reject the hypothesis that the coefficients in the minor subcells are on the whole smaller than those for all households.

Houthakker then concludes (pp. 403-4) that this:

. . . clearly reflects adversely on the permanent-income hypothesis of which it is the most direct test available so far. The fact that Friedman had confidently suggested it only adds to its significance. Nor can there be any doubt that if the concept of permanent income has any meaning at all, its numerical value must be more uniform in the minor than in the major subcells, so that the conditions for the test are indeed satisfied.

If Houthakker was right, Friedman's hypothesis, and indeed any hypothesis that consumption is related more to permanent than to transitory income (not merely the "extreme" Friedman view that consumption is related only to permanent income) would be badly damaged. But Houthakker is not right.

Houthakker has in fact "proved" too much. By his measure, the marginal propensities to consume and elasticities of consumption are actually *more* where apparently the variance of the permanent component of income is relatively less and the variance of the transitory component is relatively more. This would suggest that consumers vary their expenditures more with a transitory than with a permanent change in income. Such a result might follow from the fact that the data on consumption do not eliminate expenditures for the purchase of consumer durables, an elimination which both Friedman and Houthakker would perfer. While I believe that Houthakker is unwarranted in rejecting the possibility that this characteristic of the data is sufficient to explain the "negative" results, since I am no more prepared than Houthakker to offer data on this point I shall not press it.[3] But then what does account for Houthakker's literally negative results?

[3] While I share the view of Friedman, Houthakker and others that inclusion of only the rental or service value of consumer durables in "consumption" would permit estimation of a more stable "consumption" function, I must warn against misinterpretation of the implications of the parameters of such a function. For example, it would *not* follow then that a very low value (even zero, as suggested by Friedman) for the marginal propensity to consume out of transitory income implies that a temporary cut in personal income taxes on consumers would not be an effective antirecessionary measure. For if consumers were consequently to increase their purchases of durable goods, this increased "investment," explainable by the acceleration principle operating on an increased demand for the services

Before answering this directly, we should point to another curiosity in the Houthakker data which will help lead us to the source of the error. Recall Houthakker's finding that, taking all of the smallest subcells, the coefficients were actually significantly *un*favorable (not merely **not** significantly favorable) to the Friedman hypothesis. But when the major cell was "occupation" and the minor cell was "age and occupation" the "score" (on both the linear and the double-log coefficients) was 16 favorable and only 12 unfavorable. A chi-square test reveals that the ratio of "favorable" to "unfavorable" linear coefficients in this classification differs significantly from the corresponding ratio (of 67 to 148) involving the smallest subcells, which Houthakker, it will be recalled, found "most devastating." (With one degree of freedom, the adjusted chi-square of 6.325 on the comparison of linear coefficients in the two classifications is significant at the .02 probability level.) Indeed similarly significant (although not as sharp) discrepancies are found in other classifications where "occupation" is the major cell. Apparently some systematic factor which Houthakker has ignored is affecting his results.

⌐To understand this factor we must digress briefly to elaborate on the theoretical basis for the test proposed by Friedman. He states that where the variance of the permanent component of income is reduced the regression coefficients should be less. But this is clearly on the assumption, which Houthakker forgets, that the variance of the transitory component of income is not reduced relatively as much as or more than the reduction in the variance of the permanent component. Now if permanent income is correlated with occupation, as of course it is, the variance of permanent income around the mean of permanent income of each occupation will, *on the average*, be less than the variance of permanent income around the mean of permanent income for all households. But it does not follow that the variance of permanent income within every occupation (or even within most occupations) will be less than the over-all variance of permanent income unless the *variance* of permanent income is unrelated to occupation. One of the basic empirical characteristics of income data is that the variance of income is highly related to occupation. Indeed it is probably this fact that has given rise to much of the more recent neo-Keynesian theory of the consumption function. In par-

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of durables stemming from the (small) permanent component of the temporary increase in disposable income, and/or by the dependence of investment on the supply of funds in an imperfect capital market (one needs a down-payment for purchases of durable goods), might well increase expenditures more than one would expect even from the old incorrect estimates of marginal propensities to consume.

One should in this connection warn against the apparent misconception of Houthakker's that all of windfall gains or temporary increases in income constitute transitory income in the Friedman model. Thus, contrary to Houthakker's assertion (p. 398), a Friedman-type consumer "who has a lucky day at the races" might very correctly "buy his friends a drink." For a man who wins $100 at the races should, with the three-year horizon that Friedman postulates and a modest rate of interest, reckon that his permanent income is up by about $35. It would be only the remaining $65 that would constitute transitory income and have no effect on consumption. However, this consideration of the nature of the income components does suggest a problem as to the consistency of Friedman's definitions with his assumption of zero correlation of permanent and transitory components—a problem I shall not pursue further in this paper.

ticular, the variance of income in the "self-employed" occupation is notoriously vastly greater than in other occupations. And it is also clear that a very large part of the variance of total income in the self-employed category must be due to variance of transitory income. A self-employed person may have a high income one year and a low income or even a negative income the next.

What this means is that the variance of permanent income within the self-employed category may be much *more* than the variance of permanent income in all households, while the *ratio* of the variance of permanent income to the variance of transitory income within the self-employed category may be *less* than this ratio for all households. And what is more, and this is of crucial importance, so much of the variance of the transitory component of income may be concentrated in the "self-employed" category that the ratio of variance of permanent to variance of transitory components in all other occupations may be greater than this ratio for the self-employed category *and greater as well than the same ratio for all households as a whole.* But if this is so it would be entirely consistent with the Friedman theory to find that the "marginal propensity to consume" in the self-employed category is less than the marginal propensity to consume in each of the other categories and in all households combined, as Houthakker's work in fact reveals (see his Table I, p. 401). And it would also be consistent with Friedman's theory to find that the marginal propensities to consume in occupations other than that of the self-employed are generally *higher* than the marginal propensity to consume for all households, as Houthakker discovered.

Now the essential characteristics of the data with which Houthakker worked are in fact entirely consistent with the preceding analysis. Estimates of the variance (and standard deviation) of total income for all households and by occupation of head of household, as calculated from the mean incomes and frequencies within income intervals, made available in published B.L.S.—Wharton School budget data[4] are as follows:

	Variance	Standard Deviation
All households	6,805,262	2,609
Self-employed	21,934,339	4,683
Salaried professionals, officials, etc.	9,461,273	3,076
Clerical and sales workers	4,670,435	2,161
Skilled wage earners	2,279,554	1,510
Semiskilled wage earners	2,029,745	1,425
Unskilled wage earners	2,108,613	1,452
Not gainfully employed	4,871,268	2,207

In two of the occupations (the self-employed particularly) the variance of total income is greater than the variance of total income for all households. It is quite clear that the significant relation between income and occupation is not strong enough to outbalance the significant relation between occupation and the variance of income. But what about the ratios of permanent to transitory variances?

We can begin to answer this question on the basis of some casual em-

[4] The source of all of our basic data is *Study of Consumer Expenditures, Income and Savings,* Vol. I., *Family Accounts—1950,* Philadelphia, University of Pennsylvania, 1956.

piricism, observing that the variance of transitory income is likely to be proportionately very high both in the self-employed group, as already suggested, and in the changing, heterogeneous "not gainfully employed" group, which would include both unemployed and retired people.[5] But if transitory variance is proportionately greater relative to permanent income variance in these two categories then, since so much of the variance of total income is concentrated in the self-employed group, the ratio of transitory to permanent variance may well be less in all of the other occupations.

Fortunately, a related Friedman hypothesis enables us to be more precise in this analysis. According to Friedman the elasticity of consumption with respect to income, or $\dfrac{d\log C}{d\log Y}$, measures the ratio of the variance of permanent income to the variance of total income. Suppose that we take the double-log regression coefficients reported by Houthakker as our estimates of this ratio for both the self-employed group and all households as a whole. These estimates would then be, respectively, .523 and .696. But now suppose, to begin to anticipate the more powerful test that we have made, we assume that there is no significant difference in the mean transitory component for each occupation. Then all of the between-occupation difference in income would be due to differences in permanent income. This is an extreme assumption because in fact cyclical and other systematic factors may actually cause some differences in the mean transitory component of different occupations (for example the self-employed group probably had a greater mean transitory component in the inflation-war year of 1950 to which our data pertain). On this assumption and ignoring the estimate of the elasticity of consumption for the not-gainfully-employed group, we still find that analysis of variance (subdivision of sums of squares) reveals that the ratio of permanent income variance to total income variance for all of the other occupations, *appropriately weighted,* would average .697, slightly higher than this ratio for all households combined.

Whatever the method of estimating the proportion of transitory to permanent variance, it should be clear that where a very large proportion of the total sums of squares of deviations about the mean is concentrated in one (or two) groups and where the variance of this group (or these groups) consists disproportionately of variance of transitory income, the variance of most, if not all, of the other groups must consist disproportionately of permanent income. In such a situation, to the significance of which Friedman unfortunately did not explicitly call attention, it becomes particularly important that the marginal propensities and elasticities utilized in the Friedman test, be, as Friedman *did* state, appropriately weighted.[3] It is because of Houthak-

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[5] The households headed by a retired person may contribute to low regression coefficients, which is really the essential statistical point, for reasons other than high relative variances of transitory income. For example, one would expect to find wide disparities in the ratios of nonhuman to human wealth in this group.

[6] Concentration of "sums-of-squares" is in fact great not only in the self-employed group (25.9 billion of a total within-occupation sum of squares of 71.0 billion) but in large cities. Thus, of a total sum-of-squares of deviations of income around the nine city class means, amounting to 80.9 billion, no less than 27.0 billion are generated in "Large Cities of the

ker's failure to weight his coefficients (for him each coefficient in effect has equal weight as he merely counts the number of coefficients "favorable" and "unfavorable") that he gets his negative results. But under these circumstances such results have no bearing on the Friedman hypothesis.

III. *A Correct Application of the Friedman Test*

Once we recognize this marked relation between the proportion of variance which is transitory and the contribution of the group to the total variance, the correct application of the Friedman test becomes clear. What we wish is a regression coefficient based on the within-group variance and covariance or the average of the within-group regression coefficients, *where each within-group coefficient is weighted by the sums of squares of the deviations of income around the group mean.* We can best make this clear in symbols:

Let C_{ig} equal the consumption of the ith household in the gth group;

    Y_{ig} equal the income of the ith household in the gth group;

    n_g equal the number of households in the gth group;

    G equal the number of groups;

    $\overline{C}_g$ equal the mean consumption of households in group g;

    $\overline{Y}_g$ equal the mean income of households in group g;

    $\overline{C}$ equal the mean consumption of all households;

    $\overline{Y}$ equal the mean income of all households;

    $n \;\; = \sum\limits_{g=1}^{G} n_g$ be the total number of households in the sample;

    b_{CY} equal the regression coefficient of consumption on income for all households combined;

    $b_{CY}^{i.G}$ equal the weighted average of within-group regression coefficients appropriate for the Friedman test.

Then, of course,

$$b_{CY} = \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (C_{ig} - \overline{C})(Y_{ig} - \overline{Y})}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (Y_{ig} - \overline{Y})^2},$$

North," where the within-group linear regression coefficient is distinctly lower than in most other city classes. Further, the between-group sum of squares of income deviations is in fact frequently relatively low; it is only 1.5 billion between-city-classes, for example, out of a grand total of 82.4 billion (although the corresponding ratio is markedly higher when occupation is the variable of classification). Thus, the amount that within-group variance (and regression coefficients) could be expected to be lower than over-all variance (and regression coefficients), because of the elimination of between-group variation, is small— perhaps smaller than Friedman anticipated in proposing his test—relative to the differences among within-group variances themselves. It is these peculiarities of the population which make the failure to "weight"—in many circumstances an innocent enough omission—so serious in this test.

but

$$b_{CY}^{i \cdot G} = \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (C_{ig} - \overline{C}_g)(Y_{ig} - \overline{Y}_g)}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (Y_{ig} - \overline{Y}_g)^2} \bullet \cdot$$

So that the weighting may be seen more explicitly, we may also let $b_{CY}^{i \cdot g}$ designate the regression coefficient for consumption on income within the group g. Then,

$$b_{CY}^{i \cdot G} = \frac{\sum\limits_{g=1}^{G} \left[b_{CY}^{i \cdot g} \sum\limits_{i=1}^{n_g} (Y_{ig} - \overline{Y}_g)^2 \right]}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (Y_{ig} - \overline{Y}_g)^2} \cdot$$

To summarize in words, what we need for the Friedman test is the regression of consumption on income for all households calculated on the basis of the total within-group variance and covariance, or the sum of the variances and covariances around the group means weighted in each case by the number of households in the group. In terms of within-group regression coefficients, what we need is the average of within-group regression coefficients weighted by the within-group sums of squares of deviations of income about the group mean.[7]

Then the test of the Friedman hypothesis is whether this within-group *weighted* regression coefficient is less than the over-all regression coefficient for all households combined. Since we have concentrated our discussion thus far on the classification by occupation let us first present the comparison of the over-all coefficients with the weighted averages of coefficients for occupations:

	Linear (MPC)	Double-log (Elasticity)
Weighted average of within-group occupation coefficients	.654	.702
Over-all coefficients	.679	.727

These differences are statistically significant at the .001 probability level. The Friedman hypothesis passes the test, a test that it "failed" decisively (by a ratio of 6 to 1) with Houthakker's unweighted (or equal-weight) comparisons.

In Table 1 we present comparisons of over-all coefficients (col. 4 and 7) and appropriately weighted within-group coefficients (col. 3 and 6) for many of the classifications employed by Houthakker. The table reveals results remarkably favorable to Friedman's hypothesis. Quite contrary to Houthakker's irrelevant comparisons, in every single one of 23 sets of correct comparisons, the appropriately weighted within-group marginal propensity to consume and the appropriately weighted within-group "elasticity of con-

[7] This is developed more fully in the appendix.

TABLE 1.—WITHIN-GROUP, OVER-ALL, AND BETWEEN-GROUP REGRESSION
COEFFICIENTS OF CONSUMPTION ON INCOME*

Variable of Classification (1)	Population (2)	Linear Coefficients (Marginal Propensities)			Double-log Coefficients (Elasticities)		
		Within Group $(b^{i,g}_{cy})$ (3)	Over-all (b_{cy}) (4)	Between-Group (b^g_{cy}) (5)	Within Group $(\beta^{i,g}_{cy})$ (6)	Over-all (β_{cy}) (7)	Between-Group (β^g_{cy}) (8)
Age[d]	Total	.661	.681	.975[a]	.675	.709	.900[a]
Occupation[d]	Total	.654	.679	.806[a]	.702	.727	.798[a]
City class[e]	Total	.686	.691	.935	.715	.720	.995[c]
Age by city class[d]	Total	.656	.681	.949[a]	.668	.709	.900[a]
Occupation by city class[d]	Total	.650	.679	.808[a]	.696	.727	.806[a]
Age[f]	Large cities, North	.639	.653	.864[c]	.679	.704	.849[c]
	Suburbs, North	.721	.736	1.046[a]	.729	.751	.899[a]
	Small cities, North	.668	.694	1.067[a]	.532	.589	.966[a]
	Large cities, South	.678	.692	1.067[c]	.776	.793	.946[c]
	Suburbs, South	.528	.652	.881[c]	.664	.699	.892[a]
	Small cities, South	.812	.822	.941	.788	.797	.855
	Large cities, West	.668	.703	1.002[a]	.652	.705	.885[a]
	Suburbs, West	.604	.645	.861[b]	.435	.524	.835[a]
	Small cities, West	.546	.587	1.018[a]	.626	.690	.951[a]
Occupation	Large cities, North	.624	.642	.749	.696	.704	.733
	Suburbs, North	.709	.718	.772	.735	.750	.807
	Small cities, North	.664	.681	.763	.690	.699	.718
	Large cities, South	.679	.719	.904[b]	.756	.792	.896[b]
	Suburbs, South	.606	.649	.836[b]	.696	.742	.876[c]
	Small cities, South	.780	.824	.956[c]	.714	.785	.982[a]
	Large cities, West	.680	.710	.841[a]	.678	.709	.782[c]
	Suburbs, West	.598	.644	.810[c]	.568	.624	.740[c]
	Small cities, West	.568	.597	.701	.652	.699	.800[c]

[a] Differences significant at .001 level.
[b] Differences significant at .01 level.
[c] Differences significant at .05 level.
[d] 567 observations.
[e] 81 observations.
[f] 63 observations in each city class.
* The basic data utilized were the arithmetic means of consumption and income and the number of households in the cell, for cells classified by income (9 groups), age (7 groups), occupation (7 groups), and city class (9 groups). There were 12,489 households in all. The numbers of households in the various city-class categories, in the order in which these are listed in the table, were, 3,853, 1,242, 629, 1,923, 503, 443, 2,192, 638, and 1,066. However, no more than 3 variables of classification were employed at one time and although each observation was weighted by the number of households in the cell, the maximum number of observations (cells) underlying any weighted regression was therefore 7×9×9, or 567. The "F" test was used to ascertain the statistical significance of differences (regardless of sign) among the within-group, over-all, and between-group regression coefficients, with the residual within-group variance (degrees of freedom equal to the total number of cells minus the number of groups minus one) as the lesser mean square. Differences significant at the .05 level or better are identified by superscripts in columns 5 and 8. Standard errors of the regression coefficients

sumption with respect to income" are *less* than the respective over-all coefficients. In addition, in further accord with Friedman's suggested test, as we stratify for more variables related to income we further reduce the within-group coefficients. Thus, for example, the within-age-and-city-class "marginal propensity to consume" of .656 is less than the within-age propensity of .561 and the within-city-class propensity of .686. Similarly, the within-occupation-and-city-class propensity of .650 is less than the within-occupation propensity of .654 (as well as, again, the within-city-class propensity of .686).

IV. *A More Powerful Test*

Transitory components of income, Y_{ig}^t, may be considered as themselves the sum of two components, the mean transitory component of the group, say $\overline{Y}_g^t$, and the deviation of the individual household's transitory income from the mean transitory component of the group, that is, $Y_{ig}^t - \overline{Y}_g^t$. The cross-section variance of the transitory component of income for a sample of n households may therefore be written:

$$S_t^2 = \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} [(Y_{ig}^t - \overline{Y}_g^t) + (\overline{Y}_g^t - \overline{Y}^t)]^2}{n - 1}$$

or

$$S_t^2 = \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} (Y_i^t - \overline{Y}_g^t)^2 + \sum_{g=1}^{G} n_g (\overline{Y}_g^t - \overline{Y}^t)^2}{\sum_{g=1}^{G} (n_g - 1) + G - 1} .$$

have also been estimated. These, as well as further details as to the underlying calculations are available upon request to the author.

Each of our coefficients is derived from all observations of the groups indicated. For this reason they differ somewhat from those of Houthakker, who eliminated a number of cells "in order to avoid meaningless results based on very small numbers of households" (p. 402). In particular, Houthakker excluded all households where the age of the head was over 65 or under 25, thus restricting himself to 4 of the 7 age groups in which the data were classified, and also eliminated all remaining subcells in which fewer than 5 income groups were represented. Such eliminations were unnecessary in the case of our weighted measures. Each set of our coefficients ($b_{GY}^{i.a}$, b_{GY}, and b_{GY}^g; and $\beta_{GY}^{i.a}$, β_{GY}, and β_{GY}^g) for any group or groups is of course based on data for the same set of households.

Coefficients derived from weighted means of consumption and income of cells defined by income and other classificatory variables depend somewhat upon the identity of the other classificatory variables employed. Minor discrepancies apparent in our table among over-all regression coefficients for identical populations reflect inequalities of the amounts and proportions of *within-income group* variance and covariance (of course unavailable from the tabulated frequency distributions both Houthakker and I have utilized), when income groups are subdivided by age, occupation, city class, age and city class, or occupation and city class. Problems of lack of uniqueness of the resultant coefficients, occasioned by the form of the available published tabulations, are apparently trivial in numerical magnitude and are, in any event, largely irrelevant to the comparisons of within-group, over-all, and between-group coefficients with which we are concerned. It should of course be clear throughout that "between-group" refers to groups other than income and that all of the "within-group" coefficients are actually "between-income-group" coefficients.

Then, if the transitory component of income is uncorrelated with the variable of classification, the larger the values of n_g, the more the mean transitory component for any group, $\overline{Y}_g^t$, will approximate the over-all mean transitory component, $\overline{Y}^t$. Let us now introduce the "between-group regression coefficient," based upon the group means of consumption and income weighted by the number of households in each group. Denoting this coefficient by $b_{\overline{CY}}^g$, we have:

$$
b_{\overline{CY}}^g = \frac{\displaystyle\sum_{g=1}^{G} n_g(\overline{C}_g^p - \overline{C}^p)(\overline{Y}_g^p - \overline{Y}^p) + \sum_{g=1}^{G} n_g(\overline{C}_g^t - \overline{C}^t)(\overline{Y}_g^t - \overline{Y}^t)}{\displaystyle\sum_{g=1}^{G} n_g(\overline{Y}_g^p - \overline{Y}^p)^2 + \sum_{g=1}^{G} n_g(\overline{Y}_g^t - \overline{Y}^t)^2} .
$$

According to Friedman, the second term of the numerator, which corresponds to the covariance of the transitory components of consumption and income, should approximate zero. However, if the transitory component of income is unrelated to the variable of classification (g), the value of the second term of the denominator will also be nonzero only because of sampling fluctuations, and the ratio of its value to that of the first term of the denominator will approach zero as the values of n_g increase. For, it will be noted that, if the variable of classification *is* related to *permanent* income the values of $(\overline{Y}_g^p - \overline{Y}^p)$ will *not* approach zero, while, if the variable of classification is uncorrelated with the *transitory* component of income, the value of $(\overline{Y}_g^t - \overline{Y}^t)$ *will* approach zero.

From this it follows that, if we pick our variables of classification properly, as we increase the number of households in each group the second terms of the numerator and denominator above will tend to vanish and the between-group regression coefficient will approach the value of the coefficient of regression of the permanent component of consumption on the permanent component of income.[8] The same result will follow for the elasticity of consumption with respect to income, that is, the coefficient of regression of the logarithm of consumption on the logarithm of income. Thus, we should expect the elasticity of consumption with respect to income estimated from the group means of the logarithms of consumption and income to be close to unity if the Friedman hypothesis is correct. More accurately, we should expect this elasticity to be significantly closer to unity than the elasticity calculated from the data with regard to individual households, which we have referred to as the "over-all" coefficient. How close to unity the calculated between-group elasticity will be will depend upon the extent to which the variable of classification, while correlated with permanent income, is in fact uncorrelated with the transitory component of income.

While much of the transitory component of income is unique to the individual—layoffs, illness, windfall gains and losses—and individual differences

[8] Expressed another way, for appropriate choices of the variable of classification, g, $b_{\overline{CY}}^g$ will be an asymptotically unbiased estimate of Friedman's k $\left(\text{or } \dfrac{C_p}{Y_p}\right)$.

will tend to wash out in any large-group averaging, which is the basis of our hypothesis, some of it does, of course, vary systematically. Thus, for example, year-to-year changes in relative prices and in averages or indices of prices may have different effects upon the transitory income of different groups. In 1950, in particular, it may be expected that the rise in prices and money incomes and other consequences of the beginning of the Korean war would influence unequally the transitory incomes of different occupations, city classes, and age groups. One might expect the self-employed group, and possibly wage earners, to have sharply positive transitory components. Similarly, retired people and older age groups might suffer declines in real income which would in part represent declines in transitory components.[9] Cities containing plants operating on military contracts might evidence greater increases in transitory income than other cities, and so forth.

In spite of all these qualifications, it would seem clear that if our hypothesis is correct (and ultimately if Friedman's hypothesis as to the roles of permanent and transitory income is correct) we should expect our between-group regression coefficients to be substantially higher than the over-all coefficients and, a fortiori, consistent with the original Friedman test proposal, to be higher than the within-group coefficients. And, particularly, we should expect the between-group elasticities of consumption with respect to income to be substantially closer to unity than the over-all elasticities (and again, a fortiori, than the within-group elasticities). Thus while the Friedman test involves only the prediction that within-group coefficients will be less than the over-all coefficients, our test demands that the between-group linear regression coefficient approximate the ratio of permanent consumption to permanent income, k, and that the between-group log-linear coefficient (elasticity of consumption with respect to income) approximate unity for appropriately selected variables of classification. And we should be able to anticipate at least relatively the amount of departure from unity as a result of the nature of the variable of classification and its relation or lack of relation to the transitory component of income (presuming, of course, that the variable of classification is related to the permanent component of income).

The results of our test are striking (see Table 1, col. 5 and 8). The between-group city-class cross-section marginal propensity to consume based upon the means of income and consumption in the 9 different city classes was .935, far above the corresponding .691 figure for the over-all marginal propensity to consume, as well as, of course, the within-city-class weighted MPC of .686. The between-city-class elasticity is .995, about as close to unity as anyone could wish,[10] and far above both the over-all elasticity of .720 and the weighted within-city-class elasticity of .715. The between-occupation marginal propensity to consume based upon means of consumption and income of each occupational group is .806, which is again substantially above the over-all figure of .679, as well as the weighted within-occupation coefficient of .654.

[9] Some members of older age groups whose retirement had been postponed or who had been called out of retirement as a result of the war-induced increase in the demand for labor might, of course, have substantial, largely transitory *increases* in income.

[10] Standard error of .038.

The between-occupation elasticity is .798, again substantially above both the over-all elasticity of .727 and the weighted within-occupation elasticity of .702.

The between-group coefficients, while bound mathematically to conform (in the opposite direction) to the relation of the within-group to over-all coefficients, are thus particularly revealing. That the elasticities are in many instances close to unity is in accord with Friedman's view of the relation between consumption and the permanent component of income and our suggestion that for appropriate variables of classification the variance in the transitory component of income "washes out" between groups. It may be noted, however, that the elasticities between-occupations are conspicuously less than unity. But is this not precisely what we might have expected in view of the likelihood that the transitory component of income would in fact be much less *unrelated* to occupation than to other variables of classification? Particularly, in the year of 1950, to which our data pertain, the transition from recession to war-induced inflation would appear likely to have involved transitory components of income that were markedly related to the occupation of the head of the household.

Our between-group coefficients suggest strongly that with appropriate variables of classification unrelated to the transitory component of income, with other relevant variables accounted for, and with consumption defined so as to include only the service or depreciation (not the purchase price) of durable goods, the elasticity of consumption with respect to income may indeed be close to unity. What is more, it may be noted, the "marginal propensity to consume" derived from such intergroup data would appear strangely close to unity, as well. Is all of the Keynesian concern about saving without base? Lest we leap to unwarranted conclusions we should remind ourselves that the relevant (and Keynesian) marginal propensity relates consumption to *national* income. We deal only with disposable personal income. These statistics may, however, be a useful reminder that most of saving is to be accounted for and explained in terms of undistributed corporate profits and "capital gains," rather than personal income. But all this is another story.

V. Conclusion

Both the test proposed by Friedman, involving appropriately weighted within-group regression coefficients, and our test, involving between-group regression coefficients, give results which are consistent with the Friedman hypothesis (that consumption is unrelated to nonpermanent components of income.) This should not be taken to mean that Friedman's theory of the consumption function is sufficient for all purposes of prediction and analysis. Our results are consistent as well with the hypothesis that consumption is positively related to temporary components of income, but not as positively related as it is to the permanent component. It should also be recognized that the averaging process involved in working with group means may have "washed out" many relevant variables, other than the transitory component of income, which should play a significant role in the development of a stable consumption function. It would be in order, too, to utilize data such as we

have employed to choose between elements of the Friedman model and the frequently similar but sometimes critically different elements of the Modigliani-Brumberg model of consumption.

But with all of these challenging issues still to be resolved, it should be clear that contrary to Houthakker's report, Friedman's permanent income hypothesis does not fail one of the important appropriate tests Friedman proposed. And further, it comes through with flying colors on the more powerful test of between-group coefficients presented in this paper.

APPENDIX

The basis for both the Friedman test and our own may be indicated rigorously as follows. Utilizing symbols introduced in the text and, to simplify the exposition, letting:

$$c_{ig} = C_{ig} - \overline{C}, \qquad y_{ig} = Y_{ig} - \overline{Y},$$

$$c_{i.g} = C_{ig} - \overline{C}_g, \qquad y_{i.g} = Y_{ig} - \overline{Y}_g,$$

$$c_g = \overline{C}_g - \overline{C}, \text{ and } y_g = \overline{Y}_g - \overline{Y},$$

we have, for the sums of squares of deviations of income from its mean,

$$(1) \quad \sum_{g=1}^{G} \sum_{i=1}^{n_g} y_{ig}^2 = \sum_{g=1}^{G} \sum_{i=1}^{n_g} (y_{ig}^p + y_{ig}^t)^2 = \sum_{g=1}^{G} \sum_{i=1}^{n_g} (y_{i.g}^p + y_g^p + y_{i.g}^t + y_g^t)^2.$$

Similarly, we have for the sums of products of deviations of consumption and income from their means,

$$(2) \quad \begin{aligned} \sum_{g=1}^{G} \sum_{i=1}^{n_g} c_{ig} y_{ig} &= \sum_{g=1}^{G} \sum_{i=1}^{n_g} (c_{i.g} + c_g)(y_{i.g} + y_g) \\ &= \sum_{g=1}^{G} \sum_{i=1}^{n_g} c_{i.g} y_{i.g} + \sum_{c=1}^{G} n_g c_g y_g. \end{aligned}$$

Then in terms of total or measured consumption and income the "overall" least squares regression coefficient is:

$$(3) \quad b_{CY} = \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} c_{i.g} y_{i.g} + \sum_{c=1}^{G} n_g c_g y_g}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} y_{i.g}^2 + \sum_{g=1}^{G} n_g y_g^2}.$$

The within-group coefficient appropriate to the Friedman test, based only on within-group variances and covariances, is:

$$(4) \quad b_{CY}^{i.G} = \frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} c_{i.g} y_{i.g}}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} y_{i.g}^2}.$$

The between-group coefficient which we have proposed is:

$$(5) \qquad b_{\overline{CY}}^{\,g} = \frac{\displaystyle\sum_{g=1}^{G} n_g c_g y_g}{\displaystyle\sum_{g=1}^{G} n_g y_g{}^2} \ .$$

It may be noted first that since

$$(4.1) \qquad \sum_{i=1}^{n_g} c_{i.g} y_{i.g} = (n_g - 1) S_{c_{i.g} y_{i.g}};$$

where $S_{c_{i.g} y_{i.g}}$ is of course the covariance of C and Y within the group g, and

$$(4.2) \qquad \sum_{i=1}^{n_g} y_{i.g}^{2} = (n_g - 1) S_{y_{i.g}}^{2},$$

where $S_{y_{i.g}}^2$ is the variance of Y within the group g, we may give (4) the alternative formulation which makes clear the appropriately weighted average of regression coefficients that we require. Substituting (4.1) and (4.2) in (4), we secure:

$$(4.3) \qquad b_{CY}^{i.G} = \frac{\displaystyle\sum_{g=1}^{G} (n_g - 1) S_{c_{i.g} y_{i.g}}}{\displaystyle\sum_{g=1}^{G} (n_g - 1) S_{y_{i.g}}^{2}} = \frac{\displaystyle\sum_{g=1}^{G} \left[b_{CY}^{i.g} \left(\sum_{i=1}^{n_g} y_{i.g}^{2} \right) \right]}{\displaystyle\sum_{g=1}^{G} \sum_{i=1}^{n_g} y_{i.g}^{2}} \ .$$

Now by Friedman's assumption that permanent and transitory components of income are uncorrelated in the population from which our sample is drawn, or that:

$$(6) \qquad r_{Y^p Y^t} = 0,$$

we can, from (1) write the mathematical expectation of the variance of income in a sample of n households, recalling that

$$n = \sum_{g=1}^{G} n_g,$$

as:

$$E\left[\frac{1}{n-1} \sum_{g=1}^{G} \sum_{i=1}^{n_g} y_{ig}^{2} \right]$$

$$= E\left[\frac{1}{n-1} \left(\sum_{g=1}^{G} \sum_{i=1}^{n_g} (y_{i.g}^{p} + y_g^{p})^2 + \sum_{g=1}^{G} \sum_{i=1}^{n_g} (y_{i.g}^{t} + y_g^{t})^2 \right) \right]$$

(7)
$$= E\left[\frac{1}{n-1}\left(\sum_{g=1}^{G}\sum_{i=1}^{n_g}(y_{i.g}^{p})^2 + \sum_{g=1}^{G}n_g(y_g^{p})^2 + \sum_{g=1}^{G}\sum_{i=1}^{n_g}(y_{i.g}^{t})^2 + \sum_{g=1}^{G}n_g(y_g^{t})^2\right)\right].$$

Similarly, we can write the mathematical expectation of the covariance of consumption and income as:

$$E\left[\frac{1}{n-1}\sum_{g=1}^{G}\sum_{i=1}^{n_g}c_{ig}y_{ig}\right]$$

$$= E\left[\frac{1}{n-1}\left(\sum_{g=1}^{G}\sum_{i=1}^{n_g}(c_{i.g}^{p}+c_{i.g}^{t})(y_{i.g}^{p}+y_{i.g}^{t})\right.\right.$$

(8)
$$\left.\left. + \sum_{g=1}^{G}n_g(c_g^{p}+c_g^{t})(y_g^{p}+y_g^{t})\right)\right]$$

$$= E\left[\frac{1}{n-1}\left(\sum_{g=1}^{G}\sum_{i=1}^{n_g}(c_{i.g}^{p}y_{i.g}^{p}+c_{i.g}^{t}y_{i.g}^{p}+c_{i.g}^{p}y_{i.g}^{t}+c_{i.g}^{t}y_{i.g}^{t})\right.\right.$$

$$\left.\left. + \sum_{g=1}^{G}n_g(c_g^{p}y_g^{p}+c_g^{t}y_g^{p}+c_g^{p}y_g^{t}+c_g^{t}y_g^{t})\right)\right].$$

Then, on Friedman's hypothesis that only the permanent components of consumption and income are systematically related, or that, for the population,

(9)
$$r_{C^t Y^t} = r_{C^p Y^t} = r_{C^t Y^p} = 0,$$

we can reduce (8) to

$$E\left[\frac{1}{n-1}\left(\sum_{g=1}^{G}\sum_{i=1}^{n_g}c_{ig}y_{ig}\right)\right]$$

(10)
$$= E\left[\frac{1}{n-1}\left(\sum_{g=1}^{G}\sum_{i=1}^{n_g}c_{i.g}^{p}y_{i.g}^{p} + \sum_{g=1}^{G}n_g c_g^{p}y_g^{p}\right)\right].$$

Thus, if Friedman's assumptions are correct, by rearranging terms and dividing (10) by (7) we can derive a consistent[11] estimate of the over-all least-squares coefficient of regression of total consumption on total income, as:

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[11] We cannot be sure that our estimates are unbiased because the mathematical expectation of a ratio is not generally equal to the ratio of the mathematical expectations of the numerator and denominator taken separately.

$$(11) \quad \operatorname*{plim}_{n \to \infty} b_{CY} = \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} c_{i.g}^{p} y_{i.G}^{p} + \sum\limits_{g=1}^{G} n_g c_g^{p} y_g^{p}}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} \left[(y_{i.g}^{p})^2 + (y_{i.g}^{t})^2 \right] + \sum\limits_{g=1}^{G} n_g \left[(y_g^{p})^2 + (y_g^{t})^2 \right]} \right\}.$$

The corresponding consistent estimate of the within-group regression coefficient is indicated by:

$$(12) \quad \operatorname*{plim}_{n \to \infty} b_{CY}^{i.G} = \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} c_{i.g}^{p} y_{i.g}^{p}}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} \left[(y_{i.g}^{p})^2 + (y_{i.g}^{t})^2 \right]} \right\},$$

and for the corresponding estimate of the between-group coefficient,

$$(13) \quad \operatorname*{plim}_{n \to \infty} b_{C\bar{Y}}^{g} = \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} n_g c_g^{p} y_g^{p}}{\sum\limits_{g=1}^{G} n_g \left[(y_g^{p})^2 + (y_g^{t})^2 \right]} \right\}.$$

Now, if the covariance of c^p and y^p is not related to the variable of classification, g,

$$(14) \quad k = b_{C^p Y^p} = \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} c_{i.g}^{p} y_{i.g}^{p}}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (y_{i.g}^{p})^2} \right\} = \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} n_g c_g^{p} y_g^{p}}{\sum\limits_{g=1}^{G} n_g (y_g^{p})^2} \right\}.$$

Friedman's argument, then, is that the ratio of permanent to total income variance within groups is likely to be less than this ratio over-all, or that,

$$(15) \quad \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (y_{i.g}^{p})^2}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} \left[(y_{i.g}^{p})^2 + (y_{i.g}^{t})^2 \right]} \right\}$$

$$< \lim_{n \to \infty} \left\{ \frac{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} (y_{i.g}^{p})^2 + \sum\limits_{g=1}^{G} n_g (y_g^{p})^2}{\sum\limits_{g=1}^{G} \sum\limits_{i=1}^{n_g} \left[(y_{i.g}^{p})^2 + (y_{i.g}^{t})^2 \right] + \sum\limits_{g=1}^{G} n_g \left[(y_g^{p})^2 + (y_g^{t})^2 \right]} \right\}.$$

But for Friedman's argument [inequality (15)] to hold it is necessary that

the ratio of permanent to total income variance *within* groups also be less than this ratio *between* groups, or

$$(16) \quad \lim_{n \to \infty} \left[\frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} (y_{i.g}^p)^2}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} [(y_{i.g}^p)^2 + (y_{i.g}^t)^2]} \right] < \lim_{n \to \infty} \left[\frac{\sum_{g=1}^{G} n_g (y_g^p)^2}{\sum_{g=1}^{G} n_g [(y_g^p)^2 + (y_g^t)^2]} \right],$$

which is the basis for our own test.

This last condition will in fact be met if the variable of classification, g, is related to permanent income but unrelated (or at least relatively less related) to transitory income. A sufficient, but of course not necessary, condition for this last is that:

$$(17) \quad \lim_{n_g \to \infty} \overline{Y}_g^t = 0 \qquad\qquad g = 1, \cdots, G$$

whence it will follow that:

$$(18) \quad \lim_{n \to \infty} \overline{Y}^t = 0$$

and

$$(19) \quad \lim_{n \to \infty} \frac{1}{n} \sum_{g=1}^{G} n_g (y_g^t)^2 = 0.$$

Then if the Friedman hypotheses as to the relationships among the covariances of the permanent and transitory components of income and consumption [(6) and (9)] are correct, and the variables of classification are such that equation (14) and inequality (16) are also satisfied, we may multiply (14) by (16) to obtain

$$(20) \quad \lim_{n \to \infty} \left[\frac{\sum_{g=1}^{G} \sum_{i=1}^{n_g} c_{i.g}^p y_{i.g}^p}{\sum_{g=1}^{G} \sum_{i=1}^{n_g} [(y_{i.g}^p)^2 + (y_{i.g}^t)^2]} \right] < \lim_{n \to \infty} \left[\frac{\sum_{g=1}^{G} n_g c_g^p y_g^p}{\sum_{g=1}^{G} n_g [(y_g^p)^2 + (y_g^t)^2]} \right],$$

whence, substituting from (12) and (13),

$$(21) \quad \operatorname*{plim}_{n \to \infty} b_{CY}^{i.G} < \operatorname*{plim}_{n \to \infty} b_{C\overline{Y}}^{g}$$

and, since numerators and denominators of (20) are all nonnegative, inspection of (11) reveals that:

$$(22) \quad \operatorname*{plim}_{n \to \infty} b_{CY}^{i.G} < \operatorname*{plim}_{n \to \infty} b_{CY} < \operatorname*{plim}_{n \to \infty} b_{C\overline{Y}}^{g}.$$

It may also be seen readily that in the limiting case where

$$(19) \qquad \lim_{n \to \infty} \frac{1}{n} \sum_{g=1}^{G} n_g (\overline{y}_g^t)^2 = 0,$$

that is, where the mathematical expectation of the mean transitory component of income is the same in all groups (including hence the case where $E(\overline{Y}_g^t) \equiv 0$),

$$(23) \qquad \operatorname*{plim}_{n \to \infty} b_{\overline{C}\overline{Y}}^{\theta} = \lim_{n \to \infty} \frac{\sum\limits_{g=1}^{G} n_g \overline{c}_g^p \overline{y}_g^p}{\sum\limits_{g=1}^{G} n_g (\overline{y}_g^p)^2} = k.$$

An analogous exposition in terms of the logarithms of consumption and income can be used to demonstrate that if the Friedman hypotheses are correct, with appropriate variables of classification, g,

$$(24) \qquad \operatorname*{plim}_{n \to \infty} \beta_{CY}^{i.G} < \operatorname*{plim}_{n \to \infty} \beta_{CY} < \operatorname*{plim}_{n \to \infty} \beta_{\overline{C}\overline{Y}}^{\theta} = 1,$$

where β refers to the least-squares estimate of the log-linear coefficient in a stochastic relation of the form

$$(25) \qquad \ln C = \alpha + \beta \ln Y + u.$$

ROBERT EISNER[*]

The Permanent Income Hypothesis: Comment

Prior to receiving Robert Eisner's comment I had similar calculations made.[1] These differ from those reported in Eisner's note in two respects. (1) Since H. S. Houthakker kindly made available to me his data and calculations, my calculations were based on identically the same data as his, whereas Eisner used data derived from the same source but covering a somewhat larger number of households. While Eisner's more comprehensive coverage seems preferable for substantive purposes, it has the defect of introducing a difference of coverage in a comparison with Houthakker's results. (2) My calculations were comprehensive in the sense that they covered every com-

parison that is possible with Houthakker's data between an "appropriately weighted" average elasticity for minor cells and the elasticity for the corresponding major cell, whereas Eisner is somewhat selective.

In view of these differences, it seems worth recording that my calculations, like Eisner's, yield results strikingly in accord with those predicted by the permanent-income hypothesis.[2]

In proposing in my book the test that Houthakker misinterpreted, I was elliptical in not specifying explicitly what weights are "appropriate"; and an explanation, if not justification, may be in order. The test in question was suggested as one in a list of eight possible additional tests of the hypothesis, all directed at the specialized research worker rather than the general reader. I thought it would be clear to a statistically sophisticated reader that the key sentence[3] was a verbal description of a mathematical formula and that he would be able to reconstruct the formula for himself—especially since, as Eisner notes, the weights are the standard weights relevant in any analysis of variance or regression problem.

MILTON FRIEDMAN*

[2] I shall be glad to make available to any interested reader tables summarizing the results of the calculations.

[3] "An appropriately weighted average of the corresponding elasticities should then be smaller than the elasticity for the group as a whole, and smaller by an amount calculable from the income data for the separate communities" (p. 216).

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The Permanent Income Hypothesis: Reply

Robert Eisner has put the profession greatly in his debt by his brilliant filling-in of some of the gaps in Friedman's' argument, and indeed by his lucid discussion of the permanent-income hypothesis generally. I also had tried to find the weights which Friedman neglected to state, but without success, and a look at the lengthy derivations in the appendix to Eisner's paper may suffice to show that Friedman had strained the statistical sophistication of his readers to the limit. Even with the benefit of hindsight I can detect no clue in the book that Friedman thought of his test as an application of the analysis of variance. Eisner's contribution, of course, is the more remarkable.

I agree that the two tests which Eisner has set out and performed are appropriate, given the permanent-income hypothesis, and that the results of the first test (described in Section III of his paper) go in the direction required by the hypothesis (though not, I think, far enough), but I do not agree that the results of his more powerful test (Section IV) are "strikingly consistent with the predictions of the Friedman model" as Eisner maintains.

As to the first test it should be noted that the differences between the "within-group" and the "over-all" estimates in Eisners Table 1 are all relatively small. The difference between column (3) and column (4) nowhere exceeds .046, and of the 23 comparisons made 16 result in a difference of less than .031, or less than 5 per cent. The differences between columns (6) and (7) are on the whole somewhat larger, but rather small even so. The Friedman-Eisner effect (if I may so designate the difference between the within-group

and the over-all regression coefficients) is evidently of limited quantitative importance.[1]

The results of the test which I performed can also be explained along these lines. The elasticities and marginal propensities to consume for the "minor subcells" which I calculated can be divided into two groups. Most of the subcells, including a majority of the households, had relatively high elasticities, while a smaller number, mostly in the self-employed and not-gainfully-employed classes, had much lower elasticities. Misled by Friedman's "eliptical" statement, I disregarded the variance of transitory income within the minor subcells, which is relatively smaller for the first group than for the second. Although there are consequently more cells whose elasticities apparently disagree with the hypothesis, an average with appropriate weights (as derived by Eisner in the appendix to his paper) goes the right way. If the Friedman-Eisner effect had shown up more strongly in these data, however, it would have been almost impossible to obtain such negative results as I obtained by counting. Though the test I applied was not in complete accordance with the permanent-income hypothesis I still feel, therefore, that it has helped towards a proper evaluation.

While the results of Eisner's first test do go *in the direction* predicted by Friedman, this is not by itself a confirmation of the hypothesis. As Eisner points out (p. 984), his "results are consistent as well with the hypothesis that consumption is positively related to temporary components of income, but not as positively related as it is to the permanent component," whereas it is a crucial assumption of Friedman's theory that consumption is not related to the temporary component of income. According to Friedman's original statement of the test, in fact, the within-group elasticity is required to be not merely smaller than the over-all elasticity, but "smaller by an amount calculable from the income data for the separate communities" (*A Theory of the Consumption Function,* p. 216). He does not say how to calculate it, but presumably the specialized, statistically sophisticated research worker would also be able to work this out for himself. Since I am clearly not entitled to this august status, I merely observe that until Friedman shows the difference to be equal to the amount thus calculated, his claim that Eisner's results (or his own) are "strikingly in accord with those predicted by the permanent-income hypothesis" is without adequate foundation.

Now it may be surmised that the unspecified and unperformed calculation required by Friedman is actually equivalent to the "more powerful test" discussed by Eisner in Section IV of his paper, though Eisner does not present it as such. If so, it is only necessary to go into the results of the latter test, as given in columns (5) and (8) of Eisner's Table 1. I shall confine my remarks to the between-group elasticities, since, as Eisner notes, the permanent-income hypothesis gives a more definite prediction concerning those than it does concerning the between-group marginal propensities to consume. The

[1] This also appears clearly from the calculations made by Friedman on the basis of my earlier calculations and referred to in his comment. For the finest breakdown I used (dividing households by city class, age of head and occupation simultaneously), the Friedman-Eisner effect is less than 5 per cent of the elasticities involved.

latter should approximate the ratio of permanent consumption to permanent income, which is unknown; whether or not they are "substantially above" the over-all marginal propensities is irrelevant.

Returning then to the elasticities, we see that a few are indeed very close to unity, which is what the hypothesis requires. But most of them are not much closer to one than they are to the within-group elasticity, and therefore hardly serve as favorable evidence for the hypothesis. Eisner attempts to explain this defect as derived from an alleged correlation between the transitory component of income and the classifying variables, particularly occupation. He suggests that such a correlation would be especially likely in the economic conditions of 1950, the year to which the data refer. This explanation calls for three comments: (1) Eisner does not show that this correlation, if present, could account for the large discrepancies from unity apparent in his table. (2) Without such a proof the alleged correlation is merely a *deus ex machina*, which would turn the permanent-income hypothesis into a tautology, or something close to it. (3) The national income statistics do not indicate any marked change in the occupational distribution of income from 1949 to 1950, except for a relatively large increase in transfer payments. It is therefore hard to see how the alleged correlation could have come about.

To sum up, it seems to me that Eisner's contribution, while removing one apparent contradiction of the permanent-income hypothesis, has at the same time uncovered new evidence unfavorable to the permanent-income hypothesis, along with other evidence that agrees with Friedman's theory but can also be explained by less extreme assumptions. The process of testing the hypothesis has only just begun.[2]

HENDRIK S. HOUTHAKKER*

[2] One other matter in Eisner's paper requires a reply. In his footnote 3 he warns against my "apparent misconception . . . that all of windfall gains . . . constitute transitory income in the Friedman model." Any other interpretation, however, would violate Friedman's definitions, as Eisner appears to realize at the end of his footnote. If a windfall gain were to cause an increase in permanent income, as well as an increase in transitory income, the two would be correlated, and this Friedman rules out. Eisner's strictures are based on an economic interpretation of permanent income which, whatever its other merits, has little or nothing to do with the statistical assumptions in which the permanent-income hypothesis should be expressed, according to Friedman's own exposition.

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·BOOK REVIEWS

General Economics; Methodology

The Long View and the Short—Studies in Economic Theory and Policy. By
JACOB VINER. Glencoe, Ill.: The Free Press, 1958. Pp. 462. $6.00.

In honor of Professor Jacob Viner's 65th birthday an anonymous group of
students and friends has brought out this volume of his essays and reviews
written between 1921 and 1954. It is a marvelously rich and varied collection
even though it excludes his papers primarily concerned with international
economics, which have, of course, been collected by Viner himself in a volume
which appeared in 1951.

Part I, on Economic Theory and Policy, opens with papers from the
'twenties on price theory, which demonstrate what a leading and original con-
tribution Viner was then making to this subject. It continues with two criti-
cisms of Keynesian theory and full-employment policies, and with some very
interesting essays on the principles of economic policy. Part II is on the
History of Economic Thought and here all his well-known essays in this field
are to be found from that on the Utility Concept (1925) to the review of
Schumpeter's *History of Economic Analysis* (1954). Part III on Scholarship
and Graduate Training consists of a single item, and Part IV brings together
a miscellany of shorter reviews. A valuable bibliography of all Viner's pub-
lished writings completes the volume.

It is quite impossible here to enumerate further—much less deal with—the
immense range of subjects illuminated during the last three and a half decades
by Viner. Let us rather try to work downwards, so to speak, from a generali-
zation or two about the salient qualities to be found throughout his writings.
Above all, there is the lucidity, precision, and liveliness of all his writing, and
the weight of scholarship behind it, so easily born and so deftly employed.
The important point is that Viner's style and scholarship are not just orna-
mental embellishments, but are the essential expression of the clarity and
cogency of his analysis and arguments. In his "Modest Proposal for Some
Stress on Scholarship in Graduate Training," he mentions that, "the scholar
needs to be watched lest he use technical jargon to conceal the absence of
precision." No economist is so conspicuously and invariably far from such a
sin. Nor, despite his own overdiffident suggestions, is Viner's vast but meticu-
lous learning in the history of economic thought merely an ornament or a
hobby. His decades of devoted scholarship have expressed or developed the
scrupulous critical judgment which he applies so illuminatingly to policy is-
sues, to the consideration of which, however, he also brings a set of very
vigorously held political principles.

I must admit that before I had studied this volume I would have thought
that it might have been possible to find a general charge to put in the scale
against these outstanding general virtues. Schumpeter criticized Viner for

failing "to distinguish between progress in analysis and progress towards free-trade opinions," or, more generally, the charge might be formulated of a methodological failure to distinguish between economic analysis and the advocacy of particular (liberal) policies. Indeed, a brief note dating from 1922 on "Economics and Ethics" might seem to provide some evidence for such a general charge. There Viner argued against "purification from ethical considerations" as rendering economics "socially useless," which might be taken to point towards a rather dangerous confusion. It is not "purity" but *clarity* which is surely the duty for an economist—or for anyone engaging in communication, "scientific" or otherwise. It is a duty constantly ignored in persuasive definitions, "welfare" judgments, ambiguous distinctions between production and distribution effects, and the omission of political assumptions when advocating policies.

Nevertheless, regarding such a charge, the balance of evidence throughout this great volume is markedly in Viner's favor. He is not one of the political economists, liberal or socialist, who argues for his policies on any and *every* ground—production, growth, freedom, security, equality, distributional justice, and everything else. He recognizes, for example (in the paper on "The Role of Costs in a System of Economic Liberalism"): "I am for it [economic liberalism] even if it could be shown to be an economically inefficient system. I like it; it is itself one of my values, and it fits well enough the rest of my system of values." How refreshingly honest compared with the ambiguous but tendentious dogmatizing of so much of the economic advocacy of liberalism and socialism, or with the pseudo-neutrality of "Cohenism" in Britain. Furthermore, Viner emphasizes the great need for clarification in this field and was suggesting ten years ago to the American Economic Association "that a large scale review of the methodological state of our discipline is a project which our Association might well take in hand as its next major experiment," urging the need to examine "how economists should choose their premises, or, as to what are the criteria by which an economist advises as to the goodness or badness of policy" (p. 433).

The deepest gratitude is due to those responsible for this volume, surely one of the finest and richest collections by an economist ever published.

T. W. HUTCHISON

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Economics as a Science. By ANDREAS G. PAPANDREOU. Philadelphia: J. B. Lippincott Co., 1958. Pp. x, 146. $3.50.

". . . [Men] differ not solely in what they believe themselves to see, but in the quarter whence they obtained the light by which they think they see it."[1] Those who seek to direct light into the prism of economics still do so from many directions. Some points of view are illuminating. Some are not. Papandreou's is, on the whole, in the first of these two groups, though this brief book has its limitations.

[1] J. S. Mill, *Essays on Some Unsettled Questions of Political Economy.* London 1948, Essay V, p. 141.

He correctly assumes that comparative statics is still the hard core of our discipline. Through analysis of its methodological nature he wishes to show us how it can be made more operational. He comes to his task in the belief that ". . . the practicq of economic theorists, including the strong proponents of the operationalist view, does not disclose any eagerness to state operationally meaningful theories."

On the face of it, this is a peculiar assertion. If our theories are not operational, could it be for lack of eagerness on the part of theorists and the proponents of operationalism? Surely no one likes to feel that he is perennially treading water, and most economists make more or less sustained effort to contact solid ground. Yet he means what he says. He sees a lack of eagerness implicit in our failure to perceive our plight, which in turn is a product of our unwillingness to analyze theoretical structures—or perhaps to contemplate the results of such analysis.

He begins, then, by suggesting that it is in the construction of theories that practice is deficient. Some will see justice in this complaint. For several decades, collectors and colligators of statistics, under the banner of operationalism, have clipped the wings of theory, leaving it to others to restore the plumage so that the science can fly. Champions of crude empiricism have nourished the antitheoretical tenor of opinion and their slogans have fostered a general devaluation of theory, its construction and uses. But is it fair to suggest that the theorist has acquiesced in this? Not altogether. Papandreou's own work, along with that of Marschak, Simon, and others, belies this.

Having set the stage, Papandreou raises the old question, where do economic laws apply? His answer takes up the bulk of this volume. He attempts to make of Frank Knight's notorious response—that they "apply where.they do apply"—something more than a mere truism. Through the instrument of set theory he seeks, first, to clarify the notion of "structure" itself. His immediate aim is to show what structural elements force arbitrary decisions as to their applicability upon the theorist, as in the identification of exogenous variables, and the employment of shift parameters. Our need, he concludes, is for independent definition of the "social space" within which our theories apply. This is his solvent for arbitrariness.

Independent definition is essential, he argues, for if we depend upon the theory to define social space we become involved in a circularity that insures a priori the irrefutability of our theories and consigns us to a purgatory of model building. This is how he feels matters stand: we construct explanatory schemata, valid only in the case of historical "individuals." What is needed is ". . . construction of a theory which is predictive of behavior for all historical individuals that share certain attributes." One leaves his book feeling that Papandreou believes also that the best path to this goal lies along the angle of his own approach: the logical analysis of theoretical structure.

"We have come a long way," Papandreou says of his argument in closing. From his early premise that our practices in theory construction are inadequate, he has proceeded to the conclusion that our applications of theory to data are defective. This is indeed a long way. The construction and testing (applying) of theories are two distinct, though not wholly separable, processes. Improvements in the generality of our theories, which he holds so desirable,

rest upon the former. Improvements in their empirical relevance, which he espouses so forcefully, rest upon the latter. The principal specific remedy that he recommends—independent and empirical delineation of the proper social space—is only relevant to one of the two processes: to theoretical testing.

Papandreou's notion of "social space" is analagous to the "state of the body" in physical theory. But these notions play a different role in constructing theories than they do in testing them. In construction, they are primitive concepts that are convenient in formulating laws. In testing, social space provides, at best, only a rule of thumb, because the limits of applicability of a theory are set empirically by the confrontation of derived propositions with observables. He says that we must define it independently, but it is in its very nature an independent concept. Thus circularity is not inescapable. After all, we do not use our rules of thumb in a vacuum, but come to testing equipped with historical knowledge of the phenomena with which we deal. So the circle can be broken, though not all tests—or testers—escape the trap.

His use of other notions may give pause. For example, he declares that our models have explanatory power (for historical individuals) but lack predictive power. But if they have anything of the former, can they be totally lacking in the latter? Our great economists have not had to resort to crystal balls for their insights or foresights. In this connection, it is still not clear to me how models, as he defines them, differ generically from theories. If it is only a matter of the degree of articulation and specification, then Carl Hempel's phrase "explanation sketch" is more suggestive of our difficulties. To imply that there is a difference between explanatory and predictive power is to run counter to scientific convention and gives rise to the question of what purpose the distinction serves. To prescribe the operationalizing of social space, without recognizing its character, may do no harm when it comes to testing, but it can lead us on a wild goose chase in theory building.

Questions of semantics are not unimportant. Obviously the coinage of such terms as "explanatory power," "social space," "predictive power," and a host of others, is a necessary evil. Some of these concepts, used in talking about a science, have referents in theory construction, some in testing, some in theory itself, and some have multiple referents. The language is inescapably ambiguous. To make sense of it we must draw on all of the disciplines that can throw light on the problem—not only logic. We cannot proceed without calling on the history and philosophy of science. Social history is surely fundamental to social scientific testing. We need a sociology of economics, for in the conflict of interests that attends the division of labor in research, values and procedures are born that are terribly hostile to empirical practice. Where does this leave *Economics as a Science?*

Papandreou's approach can be used to specify the points at which we must call on other disciplines. As a result of his work one can see these points a bit more clearly. The importance of this should not be underestimated, though his failure to mention the other requisites to making economics a science can give a false picture of the problem. He does not survey the whole of economics as a science, but only a part of it. Nevertheless, he is among the modern pioneers applying the tools of contemporary mathematics to the logic

'of economic explanation. The mathematics sometimes makes for tough going, but it is what makes the book important.

JAMES F. BECKER

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The Skills of the Economist. By KENNETH E. BOULDING. Cleveland: Howard Allen, Inc.; Toronto: Clarke, Irwin. 1958. Pp. vi, 193. $3.50.

Mr. Boulding describes his new book as a popular essay on the scope and method of economics. It is concerned with the subject matter of economics, the usefulness of economists, and the applicability of economic analysis to a wide range of problems extending even beyond the conventional boundaries of economics. Professional economists are likely to take considerable satisfaction in his ardent defense of them as indispensable members of society.

But the book is far from a conventional treatise on scope and method. It has a kind of autobiographical quality. On nearly every page, it bears the imprint of Boulding and of his views on the subject matter and purposes of economics. His earlier writings and teachings are suggested by the frequent use of terms such as *topology, preferred asset ratio, information system, homeostasis, organizational systems, learning theory, ecological equilibrium, servo-mechanisms,* and *heroic ethic.* Indeed, he uses his main theme, scope and method, as a kind of clothesline on which to hang brief analyses of various theoretical issues, comments on social policy, and ethical judgments. Altogether the book is as much a guide to Boulding as a guide to economics. But Boulding is an imaginative and original social scientist, who commands a bright literary style, and his book is rewarding fare. Graduate students and advanced undergraduates would gain important insights from it; and the book fully deserves the attention of professional economists. I have some reservations as to whether it fully achieves a stated purpose of helping the layman to understand what economics is about.

The author begins by placing exchange at the center of the economist's concern, and discussing general and partial equilibrium as an illustration of the process of abstraction. Since the parameters are often unknown, or must be inferred introspectively, he describes the economist's solution as "the method of plausible topology" (p. 17). He justifies economic analysis for its interpretive as well as predictive power (p. 26), a useful distinction. I was confused by his emphatic statement that the economist is not concerned with human behavior but only with the behavior of commodities (p. 29). This confusion was confounded by the fact that later chapters are devoted largely to human behavior.

He proceeds to a discussion of the ambiguities in the concept of profit maximization and develops a general theory of organizational behavior with special emphasis on information and learning theory. His chapter on "The Economist's Vision of the Economy" provides a perceptive description of the characteristic social goals of economists, namely, progress, income redistribution, and stabilization. I was surprised that he did not include efficiency as one of the built-in values of economists. He then discusses the applicability of

economic analysis to areas outside conventional economics and illustrates this applicability by showing the relevance of oligopoly theory to international politics. The final chapter is an eloquent discussion of the relation of economics to ethics. He stresses the relevance of marginal analysis to ethical issues and the importance of the religious and ethical goals as dynamic forces in economic life.

The book is interesting, imaginative, and suggestive. It will fully repay the few hours required to read it.

HOWARD R. BOWEN

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International Economic Papers, No. 7. Translations prepared for the International Economic Association. Edited by ALAN T. PEACOCK, RALPH TURVEY, WOLFGANG F. STOLPER and ELIZABETH HENDERSON. New York and London: Macmillan, 1957. Pp. 184. $3.50.

This is the seventh volume in this series of translations sponsored by the International Economic Association. Three of the items are older pieces, the other five appeared in the past few years.

The opening piece consists of two papers by Sismondi on the problem of general gluts. The first appeared in 1820 as a refutation of an article by Torrens, the second in 1824 subsequent to some discussions with Ricardo in Geneva. Sismondi's case here against the Ricardian analysis of the problem consists chiefly of his insistence that actual events are much more complex than Ricardian examples and the suggestion that continuous problems of adjustment can produce oversupply.

A 1928 paper by Luigi Einaudi on the effect of special as compared to general taxes on capital values, income and the rate of interest is included as a sample of the work of this well-known Italian economist and former president of Italy. His main points concern the care that must be exercised in defining a general tax and the need to include the benefits the tax makes possible in discussing the effect on income.

Robert Gibrat's work on the application of the log-normal distribution to skewed frequency distributions—number of families by size of income, number of firms by size of assets, etc.,—is a first-rate piece of work that is little known. In this excerpt from his book *On Economic Inequalities* (1931), he shows for four bodies of data how much better a fit is obtained with this distribution than with a Pareto or Pearson Type III curve; examines the conditions leading to log-normal distributions, his "Law of Proportional Effect," and, finding that most measures of inequality vary inversely with one of the parameters of the log-normal distribution, takes the inverse of this multiplied by 100 as a measure of inequality.

Erik Lindahl's "The Basic Concepts of National Accounting" (1954) will interest those who want to keep abreast with the theory of national accounting. Building on his own earlier thoughts plus a recent book by I. Ohlssohn, Lindahl presents a system of accounting which he argues would be a desirable system in general as well as better than the present Swedish system.

Borje Kragh's "A Basic National Budgeting Model" (1955) is in an area which Dutch and Swedish economists have almost monopolized—model-building as a basis for official forecasts and analyses of movements of the economy. He presents three models: a simplified version of the system used in drawing up the Dutch national budget, a modification of this covering the essential features of Swedish national accounting, and a third, more complicated, model. Such models provide a basis for *ex ante* and *ex post* analysis of changes in production and other variables as functions of investment, profit margins, wage rates, etc.

A paper by Wlodzimierz Brus is stated to have been a Polish contribution to a discussion carried on in East Germany in 1955. The central theme is that, although a socialist economy is fundamentally different from a capitalist economy, the "law of value" is an objective principle which socialist economies cannot ignore. Suggesting more use of the price system, this paper illustrates clearly the difficulties communist economists encounter if concerned with economizing, and it might be assigned to students with this in mind.

Brus's paper makes clear that the Polish economy is not functioning perfectly. Oscar Lange's "Outline of a Reconversion Plan for the Polish Economy," published in 1956, suggests near or actual crisis. Lange's program emphasizes the importance of economic rather than technical criteria, the need for better incentives, and clearly recommends more, though restricted, use of the price system.

Herbert Giersch's "The Trade Optimum" (1956) merits attention not only from those especially interested in location and international trade theory but economists generally. Elaborating Ricardo's proposition that an indirect tax levied on the producers of one commodity should be offset by an equivalent import duty and export bounty for the commodity, Giersch presents a welfare argument which can support, among other things, differentiation of freight rates according to raw materials, semimanufactures and manufactures; a similar tariff structure, accompanied by a corresponding system of export bounties, and protection for a country's monopolies.

As a welfare proposition, the results apply for a given income distribution only, and where proportionality, not equality, of prices and marginal social costs is the goal. More important, all Giersch's conclusions represent arguments for the "second best" since they are entirely dependent on accepting distortions due to taxes, monopoly or whatever, which a better policy presumably would try to eliminate.

<div align="right">H. Laurence Miller, Jr.</div>

University of California, Los Angeles

Lectures on Economic Principles. Volume 1. By Dennis H. Robertson. London: Staples Press; New York: John de Graff, distributor, 1957. Pp. 172. $3.50.

This is the first of three volumes based on the lectures Robertson used to give at Cambridge to second-year economics students. It deals with value theory. Volume 2 will take up distribution theory, Volume 3 money and fluctuations.

The auditors of the lectures in Volume 1 were expected to read Marshall, Pigou (*Welfare*), Chamberlin, and J. Robinson plus a number of articles. As the author points out, he did not attempt a textbook (more's the pity) and felt at liberty to neglect some topics while paying "what may seem . . . excessive attention to certain matters on which I felt myself . . . to be the custodian of a viewpoint in danger of neglect" (p. 8). Specifically, he discusses marginal utility and consumers' surplus at some length, mentioning indifference curves only to suggest that the student learn about them elsewhere. On this and other topics, Robertson seems to say that modern theoretical advances are all very well but they don't go much beyond Papa Marshall. A little old-fashioned, perhaps, but in Robertson's treatment it sounds convincing.

The book has all the qualities economists have learned to expect from Robertson—charm, lucidity, and a skillful blend of simplicity and sophistication. The reviewer fluctuates between conviction that all economics students above the sophomore level ought to read it and despair over where to fit it into the curriculum. Although it seems simple enough for those with little background, its greatest value will be for those who have already worked their way through the complications of theory and want it summed up in a nutshell —the senior reviewing for a comprehensive examination, the Ph.D. candidate about to take his preliminaries, the professional economist with his graduate-school days long behind him.

RENDIGS FELS

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The Economic Order—An Introduction to Theory and Policy. By PAUL T. ·HOMAN, ALBERT G. HART, and ARNOLD W. SAMETZ. New York: Harcourt, Brace and Co., 1958. Pp. xix, 839. $6.95.

Most introductory texts have their season in the sun and then retire to the obscure recesses of professors' bookshelves and the three-for-a-dollar tables in secondhand bookstores without having won many friends or influenced many people. Professors Homan, Hart, and Sametz, I am happy to report, have come out with a text which will see many seasons of active use and will introduce economics to many classes of American students. It is clearly written, technically competent, informative, interesting, and at times provocative. The authors have not attempted any pedagogical innovations. Rather they have dealt with the corpus of contemporary economics in seven straightforward sections: (1) The Economic Order; (2) Markets, Prices, and Allocation of Resources; (3) The Sharing of Income: Functional and Personal Distribution; (4) Public Policy towards Sectors of the Economy; (5) Income Determination and Economic Stabilization; (6) International Economic Relations; and (7) Alternative Approaches to Economic Development.

Section I contains the usual introduction to the scope and method of economics and puts the student on his guard against the logical fallacies he may encounter in economic reasoning. It then provides the institutional background for the subsequent analysis, with chapters on population, resources, and technology, the elements of a free private-enterprise system, and a survey of the basic data—labor force, GNP, etc.—of the American economy. Section

II treats supply and demand, perfect competition, pure monopoly, and the market situations in between. Throughout this section the authors use exceptionally clear graphs to very good advantage and students will be less than justified in plaguing their instructor with that hoary complaint that they understood everything in the course except the graphs.

The chapters in the section on distribution are, as I believe, pitched at the proper level for introductory students, with the marginal productivity theory of distribution holding no more than equal billing with the discussion of the empirical data about income shares. While the next section on public policy has fine descriptions of the role of government towards business, agriculture and labor and lucid discussions of the problems involved, I would have wished for a more explicit application of the analysis developed in the previous two sections. As it stands, the authors leave the students with the impression that their excellent graphs, for example, have no usefulness for clarifying policy issues.

Any introductory text has a certain hurried quality because of the impossibly wide range of material that it is called upon to cover. In the section devoted to Income Determination and Economic Stabilization I sometimes felt a little out of breath as I tried to keep pace with the authors. Even they seemed winded, for in one chapter in which, within the brief compass of 25 pages, they treat classical employment theory and modern employment theory with its propensities, multipliers, and accelerators, they remark: "the test of equilibrium or disequilibrium is not *whether* spending matches output—it simply has to—but *how* the matching is brought about" (p. 462), a statement which, though undoubtedly true, sounds like the counsel of despair. The balance of this section deals very adequately with money and banking, the national debt, the business cycle, and stabilization policy and includes a very reasonable discussion of the quantity theory of money which will satisfy all but the most doctrinaire.

Of the last two sections, I should like to call special attention to the chapters on Alternative Approaches to Economic Development (without meaning to slight the good chapters on International Economic Relations). At a juncture of world history when foreign affairs are becoming more important to the United States, it is particularly appropriate for economists to acquaint students with systems of economic organization different from their own. The discussions of the Soviet economy, the British economy, and the contrasting methods for accelerating economic growth in China and India provide a stimulating introduction to the subject. The criticisms I have here relate largely to the tone of the chapter on the Soviet economy where the authors' repugnance of Soviet totalitarian controls has led them to inject more of a value bias into the analysis than I consider necessary. For example, they cite per capita production of consumer goods in the Soviet Union and the United States for the year 1955 as an indication of relative standards of living without pointing out the difficulties inherent in making such a comparison (p. 736). In discussing Soviet pricing, they observe that "these price tags are arbitrary, not equilibrium, prices" (p. 739), thus applying normative standards derived from a competitive market to a system designed to achieve dif-

ferent objectives. They note the patent inefficiencies of the Soviet allocation
system, comparing them by implication with the optimal allocation of the
competitive market, and not with the imperfect allocation of an economy
dominated by large-scale enterprise and surfeited with the taste-creating pres-
sure of advertising, such as we have in the United States (p. 743). But these
criticisms perhaps should be leveled at neoclassical economics rather than at
the authors of this particular volume.

The Economic Order is a major contribution to the teaching of introductory
economics. It will equip terminal students with a sound knowledge of eco-
nomics and will whet the appetite of others for more.

<div align="right">KARL DE SCHWEINITZ, JR.</div>

Northwestern University

Price and Allocation Theory; Income and Employment Theory; Related Empirical Studies; History of Economic Thought

Towards a More General Theory of Value. By EDWARD HASTINGS CHAMBER-
LIN. New York: Oxford University Press, 1957. Pp. xii, 318. $5.00.

What we need, a colleague of mine urges on occasion, is a persuasive essay
on the various shades of gray. This need should be felt by anybody reading
the book under review. For in it Professor Chamberlin stoutly argues, once
again, that black and white do not exist, and that there is only one shade of
gray.

The book is a collection of sixteen essays, twelve of them reprinted and four
new. With a few exceptions, these assume the role of defending or praising the
Chamberlinian theory of monopolistic competition against every alternative.
I shall focus my few comments on the new essays and on this pervasive theme.

The book takes its title from the first essay, an essay that leaves me be-
wildered. Chamberlin offers here six elaborated reasons why "monopolistic
competition is more general than pure competition." The first reason is Mar-
shall's motto: "nature makes no leap." Hence, the argument continues, sharp
lines cannot be drawn between monopoly and competition. This is immedi-
ately followed (p. 18) by the statement: "The classification of pure compe-
tition, monopolistic competition, and pure monopoly appears to me by its
nature exhaustive." One wonders how the lines get drawn if they are not
drawable. One also wonders why it is necessary to specify pure competition
and monopoly if monopolistic competition encompasses both. Each of the re-
maining five reasons for the greater generality of monopolistic competition
specifies, in a similar fashion, a particular respect in which monopolistic com-
petition presumably avoids "leaps" that nature does not make.

We are not brought to face the real issues: Is there in fact a legitimate
theory of pure competition and pure (or, in Chamberlinian terms, isolated)
monopoly? If so, does "the" theory of monopolistic competition, if we accept
that there is one, tell us everything that Marshallian theory does? Is there
some analytical machine uniquely defined by "the" theory of monopolistic
competition that will always lead to the same conclusions as, say, supply and

demand in the appropriate cases? Finally, do theories of monopolistic or imperfect competition explain behavior in the middle ground as adequately as Marshallian theory does in the polar areas?

Chamberlin places himself on the defensive in these matters against a formidable opposition: P. W. S. Andrews, Martin Bronfenbrenner, Milton Friedman, Roy Harrod, R. L. Hall, J. R. Hicks, C. J. Hitch, Nicholas Kaldor, Frank H. Knight, Abba Lerner, James Meade, Austin Robinson, Joan Robinson, Henry Simons, and George Stigler—to mention the better-known of his contemporaries. The basic driving force for this widespread and diverse opposition is said (pp. 13ff.) to be an emotional resistance to the revolutionary implications of Chamberlinian theory, which presumably threaten, among other things, the vested intellectual interests of economists.

Chamberlin devotes an entire essay to the "Chicago School," which he apparently sees as the heart of the opposition. This group of economists is to be distinguished from others, as far as economic theory is concerned, not by any positive contributions, but rather "by the zeal with which the theory of monopolistic competition has been attacked, and in particular by the extraordinary set of misconceptions as to the nature of this theory which have emerged in the process" (p. 296). He suggests that it be renamed the "Chicago School of Anti-Monopolistic Competition."

When he breaks away from sterile exegetic controversy, the results are salutary. Thus his essay on "The Product as an Economic Variable" will reward all readers, no matter what their attitudes are toward Chamberlinian doctrines. Similarly, his essay on "Proportionality, Divisibility, and Economies of Scale" is a provocative discussion of some disturbing inadequacies in the theory of costs. Finally, his excursion into the labor problem in "The Monopoly Power of Labor" is, in the eyes of this reviewer, a praiseworthy contribution to economic analysis in this area.

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An Approach to the Theory of Income Distribution. By SYDNEY WEINTRAUB. Philadelphia: Chilton Co., 1958. Pp. x, 214. $6.50.

Who gets what, and why, has long been a major concern of economic theory. This concern may be presumed to stem from the ideological importance of explaining (if not justifying) existing relative incomes, predicting their course within the framework of the existing economic system, and evaluating the possibilities of changing them.

Weintraub's ambitious work is concerned almost exclusively with one facet of the problem, that of the relative shares accruing to the owners of broadly categorized factors of production. His approach is essentially that of "marginal productivity theory," conceived in static form, married uneasily to Keynesian concepts of aggregate demand. However, Weintraub sets most of his key relations in money terms, generally holding the wage unit constant and allowing prices to vary. His concepts are usually those of partial equilibrium analysis, supplemented by observations as to how the demand and

supply curves relevant to partial equilibria move when it is recognized, appropriately, that partial equilibrium analysis is inappropriate. The mathematics, interspersed liberally throughout the book, are very simple, frequently tedious, and occasionally incorrect.

After an introductory chapter in the course of which Euler's Theorem concerning linear, homogenous production functions is presented, Weintraub proceeds, except for occasional digressions for such problems as aggregate demand, underemployment, and profits, on the usually implicit assumption that each factor's return is equal to its marginal product multiplied by the quantity of the factor employed. Attention is given in a number of instances to the role of "monopoly" but none, curiously, to monopsony or oligopsony, which one might expect to be particularly important at least in many labor markets. Much of the analysis involves exercises in the implications of various elasticities (some familiar and some in unfamiliar money terms) and various relative positions and shapes of marginal and average product curves. Somewhat startlingly, Weintraub emerges from time to time from his deductive chains to suggest, on the basis of some casual empiricisms, weighty conclusions with regard to effects of wage changes, monetary policy or technological innovation.

Weintraub seems inclined to follow his logic and algebraic manipulations wherever they may lead. The results are sometimes striking but not always such as to inspire confidence. In one instance (p. 54), Weintraub concludes that "the income share of factor i, when there are n-variable factors, is given by

$$\frac{w_i N_i}{Z} = \frac{M_i}{n A_i} \tag{32}$$

[w_i = the money wage, N_i = the quantity employed, M_i = the marginal product and A_i = the average product of the ith factor. Z = money proceeds or money national income]. Thus the ratio of marginal to average product still [as in the one-variable-factor case] governs the result, though the income share is reduced in ratio $1/n$, with the denominator signifying the number of factors." Fortunately for the consistency of marginal productivity theory, Weintraub here is quite wrong. Under the customary assumptions, the share of each factor is equal to the ratio of its marginal and average product, regardless of the number of different factors. Weintraub's variant conclusion is based on an incorrect substitution of a partial for a total derivative in the mathematical note in which he derives his equation (32). [See equation (5), page 64.]

The book indicates substantial familiarity with the literature devoted explicitly to the theory of factor shares. Yet one is surprised by statements such as "a check of much of the literature on liquidity preference revealed that only Joseph F. McKenna has suggested that the money supply be defined" so as to include "demand deposits based on utilization of excess reserves" and thus permit an explicit consideration of banks' liquidity preferences (pp. 162-63, 163n), and, referring to the interrelations of supplies and demands of different kinds of financial assets, "Joan Robinson almost alone has endeavored to fill this gap in the liquidity analysis" (p. 168n).

This reviewer would have welcomed attention to dynamic problems of varying degrees of mobility and speeds of adjustment which may well deserve a major role in any relevant theory of income distribution. Weintraub touches on this only briefly in connection with considerations of quasi rent and the nature of profits. I wonder, too, whether fruitful use might not be made of Neumann-Morgenstern, Friedman-Savage conceptions of the role of risk and uncertainty and people's reaction to them as an explanation of relative incomes. This might at the least allow Weintraub to explain not merely the existence of profits along with losses as a result of unforeseen change, but the secular persistence of positive average profits (by most, useful definitions of the term).

But these are perhaps overly critical "might-have-beens." Weintraub's book is replete with perceptive and interesting observations regarding both factor shares and their implications. His remarks about a negative relation between (1) the level of employment and (2) real incomes of rentiers and even employed workers seem particularly relevant to an explanation of the curious, current (June 1958) concern about "inflation" and the reluctance to take strong antirecessionary action. I welcome Weintraub's insistence that, except for imperfections in the capital market and tax considerations dictating a preference for "capital gains" over income, current or past profits per se should not induce investment. The book includes an interesting criticism of "Mr. Kaldor's 'Keynesian' Theory" (pp. 104-7) which calls attention to its "tautologous" character and criticizes its elimination of "the influence of marginal-productivity ideas entirely." A chapter on "A Modified Liquidity-Preference Theory of Interest," while suffering from lack of attention to pertinent sections of Patinkin's *Money, Interest and Prices,* has some suggestive ideas on the effect of changing money supplies on prices and amounts of asset holdings, and, consequently on the demand for liquidity. All in all, Weintraub's book will merit attention from those interested in certain aspects of the theory of income distribution and some related aggregative problems.

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Essays in the Theory of Economic Growth. By EVSEY D. DOMAR. New York: Oxford University Press, 1957. Pp. ix, 272. $4.50.

The mushrooming literature on economic growth already has its classics. Among them Professor Domar's essay on "Capital Expansion, Rate of Growth and Employment" (*Econometrica*, Apr. 1946), in which he produced his version of the Harrod-Domar theorem, holds a prominent place. He has now collected in one volume nine essays related to the same subject, eight of which have been published earlier in various periodicals. These essays form a contribution to a discussion in which a large number of economists have taken part. This is, therefore, not the place to review the individual articles and their approach to the problems. I shall only try to indicate some general features of Domar's work and discuss its relation to pertinent research problems.

This task has, however, been performed in part by Domar himself in an

analytical foreword to this new volume as well as in his paper "A Theoretical Analysis of Economic Growth" (1952). The remaining eight essays present various model constructions which are intended to constitute elements in a wider theoretical structure. In his introductory essays Domar places these models in their proper perspective. He is the first to realize that they are based on very special assumptions; that they are not intended to be used directly for the interpretation of historical processes of growth. He regards them as contributions to the development of the theory of growth, at the same time illuminating earlier theories of a more stationary nature. It is only natural that an enthusiast like Domar sometimes seems to forget the limitations on the direct applicability of his explorations.

Domar has made a lasting contribution to the understanding of growth processes. His reader may, however, ask a number of questions: Are the relationships on which Domar bases his models the most essential? Should they be formulated a different way? And in which direction should the theory of growth be developed?

What strikes the reader, looking back over Domar's work, is the persistency of his effort. Around his basic formula have been grouped a number of complementary studies of foreign investment, depreciation and replacement which are designed to apply to a capitalist economy. Various references, and especially the new essay "A Soviet Model of Growth," however, indicate that his theory can just as easily be applied to a planned economy of the Soviet type. It is a theory that first of all appeals to the technocrat and the certified accountant, whether capitalist or Soviet. The models can be used as a starting point for comparisons of the two types of economic systems from a planner's point of view. They, however, give very little guidance towards the understanding of the *nature* of growth in a capitalist economy. Their relation to the institutions of a capitalist economy, with decentralized powers of decision, is actually slight. Thus, they do not provide a *theory* of growth which takes into account the incentives that determine the relevant parameters of action in a market economy. Domar's problem is not why and how growth takes place. He investigates only the *balance* of growth, i.e., the balance between over-all capacity to produce and over-all demand in a growing economy. The essence of his theory is to establish in this sense the conditions for dynamic equilibrium which, on the given assumptions, have a wide applicability to various types of economic systems.

Even within this limited framework there are certain basic problems of model construction which can be discussed. One of the essential features of every growth theory must be how it takes into account the effect of technical progress on growth of capacity to produce. In some theories, for example those along Schumpeter's lines, this effect enters explicitly into the theory. In Domar's growth models, technical progress can be said to be implied. His assumption of a constant investment productivity (σ) during the process of growth may be regarded as a result of a balance between, on the one hand, a declining marginal productivity of capital—as the stock of capital is increasing in relation to other factors and as the most productive uses of capital available given the existing state of technical knowledge are exhausted—and,

on the other hand, a rise in marginal productivity that follows from the time-flow of technological improvements. It is true that variations in this balance can be taken into account by allowing for variations in investment productivity from one period to another. But we arrive at a new basic relationship between investment and the growth of capacity when we take into account that σ will vary with the *size* of investment or the rate of growth of total capital in any period, if the flow of new technical knowledge is independent of the process of growth. The result will be a more dynamic formulation of the productivity function, for example, along the lines indicated by Kaldor in a recent article (*Econ. Jour.*, Dec. 1957, pp. 595-99).

A more explicit acceptance of technical progress as a basic fact also makes it impossible to maintain the assumption that the replacement of old capital is independent of the process of growth itself. The rate of replacement per unit of time becomes dependent not only on the rate of investment but also on the growth of income and wages and on the changes in relative prices which result from the new technique being exploited by investment. In his first classical essay, Domar approaches this problem. He discusses what he calls the "junking" process, i.e., the obsolescence of old capital assets that follows from new investment. But he never integrates this phenomenon into his basic model, even though he confesses that he is unhappy about this treatment of the problem.

The transformation of the capital structure by addition of new and by elimination of old units of capital equipment is, however, only one aspect of the general transformation of an economy during the process of growth. Similarly there is the change which occurs in the pattern of consumption, the input-output structure, the composition of foreign trade, and the distribution of labor among occupations and skills. It is actually impossible to envisage an expanding society in which a fundamental transformation in these respects does not take place. As a matter of logic, a proportional enlargement of all economic elements is actually excluded, if we discuss an economy where capital equipment is growing faster than the supply of manpower. In a theory that can be used to explain growth in a decentralized market economy, the process of transformation must necessarily be a central theme.

By developing models on a level of total aggregation, Domar has cut himself off from the problems that arise in connection with the process of transformation. In his introductory essays, he is himself concerned with this fact. He can rightly defend himself by claiming that in the pioneering stage elementary relations must be explained in as simplified form as possible. However, a development of the theory in the direction that has been indicated cannot be regarded as a refinement of the basic Domar models. It may more properly be described as a new approach.

Domar has by his pioneering work cleared much ground that will be useful for new more advanced theoretical structures. He has done it with sharpness of mind, lucidity of style, and wit of comment. We should be grateful that he has collected in one volume his contributions to the subject over a period of fourteen years. Growth is a cumulative process, and the sequence of events depends on the first steps that are taken. Similarly, the growth of the theory

of growth will depend on the impetus that Domar has provided by his brilliant essays.

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Decision Making. By DONALD DAVIDSON and PATRICK SUPPES, with SIDNEY SIEGAL. Stanford: Stanford University Press, 1957. Pp. 121. $3.25.

Empirical work on decision-making under uncertainty has scarcely begun. Even though basic advances have been made (in the work of Frank Ramsey, von Neumann and Morgenstern, and L. J. Savage) toward useful operational definitions of such familiar concepts as "utility" and "subjective probability," there has been no rush to apply them in actual measurement or prediction. In part this inertia reflects shyness in the face of experimental problems with which few economists are familiar. Yet without an attempt to fit numbers into propositions, without the vital interaction of hypotheses with data, decision theory must remain largely without content: advice that does not advise, predictions that do not predict.

The authors of this volume (two of them philosophers, one a psychologist) have stepped forward to launch what may become the new field of "experimental economics." Basing their investigation mainly on Ramsey's 1931 paper, "Truth and Probability," they begin by fashioning axiom systems designed for empirical application. They then take the radical step of dirtying their hands with actual experiment data.

In their attempt to improve upon and extend the pioneering experiments by Mosteller and Nogee (measuring subjects' utilities for small amounts of money on the basis of their choices in a game of chance) they undoubtedly succeed; their experimental technique, resembling psychometric measurements of sensory perceptions, is sophisticated in concept, design and application. Their main innovation, suggested by Ramsey, is to use in their lottery tickets a chance event whose subjective probability for the subjects is experimentally measured (instead of "objective probabilities" of dubious validity).

This book is not a suitable introduction to the general subject of decision-making under uncertainty, to the axiomatic method, nor to psychological experimentation. A good deal is taken for granted in all these fields; allusions to other work and alternative approaches are regrettably few and brief, the axiomatics are not easy reading, and discussion (which echoes Ramsey closely in its conceptual aspects) is held to a minimum. The book is rather like a long article, dealing fairly technically with problems connected with this particular set of experiments. In the course of their argument, the axioms play a rather peculiar role. Unlike their predecessors, who lent credibility to their hypotheses by deriving them from insidiously plausible axioms, these authors *derive* their "axioms" from initial hypotheses (all about equally opaque) and then derive measurement theorems from the axioms (pp. 30-34). It is not clear to me that this form of exposition is well-chosen.

Can utility and subjective probability be "measured"? Yes: it is clear from

these results that in a meaningful sense they can (though not, I suspect, in *all* circumstances). Familiar statements to the contrary simply do not reflect an awareness of the operational definitions proposed, nor of the nature and scope of psychological measurement processes. The results presented here are imprecise, though positive; but it would be fatal to any empirical investigation to be disheartened by crude or ambiguous findings at a stage as early as this.

Is the effort worthwhile? Surely this question can await a later answer. Economic writing for so long found it convenient to talk of "utility" and "subjective probability" *as though* they were measurable, there does seem a prima facie case that it should be useful to turn these notions into genuine empirical variables, actually measuring them. Who knows what new relationships might turn up, once it became feasible to assign numbers to these factors?

Still, this approach does have inherent limitations. The authors' insistence that the experiments must involve "real" decisions, in which actual prizes are won or lost, has obvious merits but also some disadvantages. One problem, to which economists seem more sensitive than psychologists, is that changes in players' assets, as they win or lose significant amounts, might change their marginal utilities for prizes remaining at stake. (Consider their experiment awarding phonograph records, Ch. 3.)

More importantly, the practical upper limit to the prizes that experimenters can offer is likely to remain well under the amounts that would really be interesting to an economist; and it seems doubtful that the utilities of these larger sums can be extrapolated from bets on the order of 17¢. Thus, it may be infeasible to predict, say, investment decisions from experimental data, unless the size of the stakes could be drastically increased. (Here, at last, is a project that would offer the Ford Foundation real scope.)

These limitations may not constrain the measurement of subjective probability, which gets only incidental attention in the present work (a shift of focus may be in order). Bets involving small amounts of money might suffice to measure an individual's subjective probabilities for political, economic or personal events of real significance for his larger decisions. Knowledge of the ways in which these probabilities form and change, and actual measurements of the uncertainties affecting typical decision makers, would have very direct application in economic science.

The major limitation to this work is not in the particular design or even the experimental approach but in the nature of the hypotheses the authors have chosen to test. These have been drawn entirely from earlier discussion, almost none of which was empirically oriented. It is true that testing must begin somewhere, but a complex, integrated theory (corresponding to sets of twelve or thirteen axioms) seems far from the best starting point for a pioneer empirical study. The authors might well have begun by investigating a much broader range of simpler, more exploratory hypotheses, not necessarily inspired by normative arguments. Whereas they tend to suggest that their basic hypothesis is uniquely plausible, covering reasonable behavior on all occasions of uncertainty, I believe that there are important classes of uncertain situations in which normal people will systematically *violate* these axioms, and in which

other hypotheses will better describe their behavior. Be that as it may, future experimenters need not feel so constrained by the economic literature to date; human behavior under uncertainty is undoubtedly more various than has been imagined in that discussion.   •

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Distributed Lags and Demand Analysis for Agricultural and Other Commodities. By MARC NERLOVE. Department of Agriculture, Agricultural Handbook No. 141. Washington: Supt. Docs., 1958. Pp. 121. 60¢.

It is a classroom platitude that the ultimate effect of a price change upon the quantity demanded may differ from the impact effect. It is even recognized that the effects of the change may well be spread out over many intervening periods. But it is by no means obvious how this is to be handled empirically: what is the specific pattern of lags appropriate to an econometric investigation of demand—or of supply, or of total consumption? Convenience rather than conviction leads most econometricians to toss a one- or two-period lag into the hopper.

Irving Fisher apparently was the first to propose the general concept of a *distributed lag,* that is, that the relation between cause x and effect y takes the form:

$$(1) \qquad y_t = a + b_0 x_t + b_1 x_{t-1} + b_2 x_{t-2} + b_3 x_{t-3} + \ldots$$

Clearly, this is quite general. Empirical relations of this type have been estimated, Tinbergen's study of foreign trade elasticities being a familiar case in point. But precisely in the generality lies the rub. The number of parameters to be estimated is in principle infinite. Even when a cut-off point is taken, the number of lagged x's to be included may be large. Given the relative brevity of most economic time series and the almost inevitable intercorrelation among the successive x's, empirical estimates of the parameters of the general form (1) will not be reliable.

L. M. Koyck offered an ingenious device to alleviate these difficulties in his *Distributed Lags and Investment Analysis* (Amsterdam, 1954). It appears plausible to maintain that the impact of past prices upon today's demand diminishes systematically as we go back in time. Specifically, Koyck supposes that the coefficients of (1) taper off geometrically, so that his model is:

$$(2) \qquad y_t = a + b x_t + \alpha b x_{t-1} + \alpha^2 b x_{t-2} + \alpha^3 b x_{t-3} + \cdots,$$

with α being a coefficient which lies between zero and one. The number of parameters to be estimated has been reduced to just three. Furthermore, a little algebra reduces (2) to:

$$(3) \qquad y_t = a(1 - \alpha) + b x_t + \alpha y_{t-1},$$

which involves only two explanatory variables. Koyck's proposal does lead to a manageable form. However, technical problems of interdependent error terms arise so that simple least-squares estimation of (3) is ruled out. More serious is the natural objection that Koyck's specification is, after all, rather arbitrary.

In the present monograph, Marc Nerlove systematically exploits the notion that closer attention might be paid to the factors which underlie distributed lag relationships. Rather than adopting such a relationship from scratch, he considers plausible structural models which might generate distributed lags. Two models are distinguished. In one, the "rigidity" model, technological and institutional factors are basic; in the other, the "expectational" model, the source of the delayed reaction is found in uncertainty. For the rigidity model, suppose that the current price, x_t, of a commodity is expected to persist, and that should it indeed persist indefinitely, demand would eventually reach the the equilibrium level, $y_t{}^*$,

$$(4) \qquad\qquad y_t{}^* = a + bx_t.$$

However, because of such factors as durability, the adjustment to a price change is not instantaneous, so that y_t is not equal to $y_t{}^*$. Instead, discrepancies from equilibrium are only gradually eliminated; specifically,

$$(5) \qquad\qquad y_t - y_{t-1} = \beta \, (y_t{}^* - y_{t-1}),$$

with β, the speed-of-adjustment coefficient, lying between zero and one. For the expectational model, suppose first that consumers are somewhat farsighted, so that today's demand depends upon the average price expected to prevail from now on, $x_t{}^*$,

$$(6) \qquad\qquad y_t = a + b \, x_t{}^*.$$

Further, price expectations are revised in accord with deviations from current experience; specifically,

$$(7) \qquad\qquad x_t{}^* - x_{t-1}{}^* = \gamma \, (x_t - x_{t-1}{}^*),$$

where γ, the coefficient of expectations, lies between zero and one, and is analogous to J. R. Hicks' elasticity of expectations.

Now both the rigidity model, (4) and (5), and the expectational model, (6) and (7), imply that the relation between current demand and lagged prices takes the form of a distributed lag; in fact, just the particular form proposed by Koyck. Thus, Nerlove provides a theoretical underpinning for the convenient assumption of geometrically decreasing coefficients. (Koyck himself did offer the rigidity interpretation of his specification.)

The payoff to Nerlove's dichotomization comes when additional variables —income, other prices—are introduced. It is seen that adaptations of classical regression techniques are then required, and just which adaptation is most convenient and efficient is shown to depend upon which model underlies the distributed lag. A particular choice is between directly estimating a form like (2) and estimating a reduced form like (3); here, alternative methods of reduction and of estimation are spelled out. A persistent problem is that of interdependence of error terms, and here the new approach at least clarifies the issues. This systematic exploration constitutes the body of Nerlove's pioneering work.

It may be recognized that the demand for automobiles might fit into the rigidity model, while the Friedman permanent-income hypothesis is related to the expectational model. The final pages of this monograph discuss these

two natural applications of the analysis. Results of an empirical test of the Friedman hypothesis are included; these, rather surprisingly, make up the only numerical material in the work. My impression is that more such material was in order, if only to illustrate the somewhat elaborate calculations prescribed.

The field of specification and estimation of dynamic relations is a wide-open area of econometrics. Some key issues of identification and nonrandom error terms remain unsettled by Nerlove's investigation. Objection may certainly be made to his assumption that only the demand, and not the adjustment or expectational, equations are subject to error. Nonetheless, fertile new ground has been broken by Nerlove in tracking down the economic causes and statistical consequences of distributed lags.

<div align="right">Arthur S. Goldberger</div>

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Probleme einer dynamischen Theorie der Konsumfunktion. By Eva Bössmann. Berlin: Duncker & Humblot, 1957. Pp. 110. DM 10,60.

Theories of the consumption function are coming to be as numerous as economists. In spite of the implication in the title, however, this book by Eva Bössmann does not present a new dynamic theory of the consumption function. Rather, the book examines a more basic question: what can the static microeconomic theory of consumer choice contribute to making the consumption function a dynamic tool of analysis? The conclusion reached is a negative one—it is impossible to force a Walrasian type microeconomic model to correspond to a macroeconomic theory whose first concern is a simple solution of economic problems. Yet macroeconomic theory does depend on the microeconomic system. The theory is therefore at an impasse.

Miss Bössmann first briefly outlines the development of the concept of the consumption function from Keynes onward. She then discusses, in careful detail, the problems of aggregation and of passing from the microeconomic theory of consumer behavior to an aggregate consumption function.

Basic to this entire discussion is the view that logically one must utilize the theory of consumer behavior to explain the consumption function because the consumption function as a behavior relation rests upon the decisions of individual consuming units. In following this procedure, Miss Bössmann encounters two familiar problems, the preference system of the individual consumer and the rationality principle. The preference system is the yardstick used in rational choice. It decides the ordering of possible decisions according to some relevant criteria such as the maximization of utility or an "uncertainty preference functional." This assumes a constant preference system, even when uncertainty coefficients are introduced since they will be assumed to be constant. Choice is only rational if the decisions are fulfilled over time and are not frequently disappointed. There are further complications if interdependence of consumer preferences is assumed. These considerations, Miss Bössmann argues, severely limit the usefulness of the microeconomic theory of consumer choice and accordingly, its usefulness in interpreting group behavior when passing from a micro- to a macroeconomic consumption function.

Some economists contest this view of the role of the theory of consumer choice. Milton Friedman, for example, has argued, (1) that uncertainty does not establish a presumption as to the shape of the individual consumption function, and (2) that a group behavior equation may be applicable to all members of the group (*A Theory of the Consumption Function*, Princeton 1957, pp. 14-19). Miss Bössmann, on the other hand, is quite pessimistic, thinking that it is probably the theory of "decision processes" that will produce a more adequate behavior principle.

Miss Bössmann further argues that, due to problems of aggregation, the particular form of a consumption function chosen is often a matter of expediency. The worth of a theory depends on what the model is trying to explain. It is on this basis that one decides what quantities to take as variables and what to take as parameters. Miss Bössmann therefore accepts the view that one can work with an empirical aggregate consumption function even though empirically and theoretically one cannot derive a complete macroeconomic theory. This line of reasoning opens vistas which, unfortunately, she does not choose to explore.

Expediency and common sense are principles which we as economists employ every day. Ultimately, I believe, if we are to use our theoretical and empirical tools of analysis to solve economic problems we must put aside such theorems as the impossibility of making interpersonal comparisons and the inconsistency of collective preferences, and proceed on common-sense grounds to solve economic problems in spite of these theorems. A theory of economics begins with the assumption that the economic system is not a mere aggregate of separate individuals. It seeks to understand the economic system as a whole, the separate heterogeneous individuals possessing a functional unity.

Miss Bössmann is, on the whole, to be complimented for presenting a cogent and well-reasoned critique of the literature on the consumption function. Yet she does, on occasion, commit some elementary howlers. For example, in her discussion of the role that the holding of money plays in the theory of consumer behavior, she states that the quantity theory has limited usefulness since it assumes constant velocity. Surely a cursory reading of the literature shows that while quantity theorists regard velocity as possessing regularity, they certainly do not view it as constant.

As in the case of all too many German publications, the usefulness of this book is limited by the lack of an index.

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Die theoretischen Grundlagen David Riccrdos im Lichte des Briefwechsels. By ELEONORE LIPSCHITZ. Berlin: Duncker & Humblot, 1957. Pp. 214. DM 18.

The wealth of materials unearthed by the Cambridge edition of Ricardo's works and correspondence has led to a resurgence of Ricardo interpretation. As may be gathered from G. J. Stigler's recent article in this *Review* ("Ricardo and the 93% Labor Theory of Value," June 1958, pp. 357-67), some issues

believed to be settled a generation ago are ready for a comeback. There is, however, some reasonable doubt whether the monumental edition will sub- stantially clarify the zigzag track of Ricardo's theoretical evolution or rather confirm the pessimistic view that the classical master logician never freed his system from basic contradictions and, in some cases, even failed to reach a definite stand.

Miss Lipschitz's book represents one of those skirmishes which generally precede an over-all attack. Though some parts of her discussion shed new light on Ricardo's contribution, the study as a whole is incomplete and calls for supplementation and correction. Also its scope is more restricted than might be expected from the pretentious title. Significant aspects of the Ricardian conception such as his theories on rent, on comparative costs, on money, and on credit have been neglected. The author concentrates on two of his fundamental notions: Ricardo's theory of value and his views on the possibility of a general overproduction. The first topic is discussed in great detail. The rest of the volume has been reserved for German translations of important letters and papers rediscovered by the Cambridge editors.

Despite its limitations, however, the book is a valuable and timely addi- tion to the history of classical economics. Plunging resolutely into the new materials, the author describes in a vivid way Ricardo's life-long struggle with his perennial issue whether, under highly abstract conditions, the quan- tity of labor should be regarded as the cause of the change of relative values and, furthermore, as its measure or standard. In her search for an answer, few stones have been left untouched. The text of the various editions of the *Principles* has been compared. His extensive correspondence with James Mill, Trower, and Malthus is examined in order to disclose a thread of continuous scientific evolution. Furthermore, two important papers on "Absolute Value and Exchange Value," which could be regarded as Ricardo's testament on this issue, are scrutinized. These two notes, one of them a rough draft and the other a revised version, were written in August and September 1823, only a few weeks before his death on September 11. Still we should question whether those last words of the ingenious thinker invalidate previous state- ments. They were found among the Mill-Ricardo papers. Probably James Mill had some reason to withhold them from publication. A skeptical atti- tude seems appropriate on the ground that Ricardo did not live to finish the second corrected version.

As previously mentioned by Piero Sraffa, the two notes indicate that, at the end of his life, Ricardo revived an idea which, in his earlier writings, appeared only in occasional hints and allusions: namely, the notion of a real and absolute value underlying and contrasted with exchangeable value or relative value (*The Works and Correspondence of David Ricardo*, ed. by Piero Sraffa with the collaboration of M. H. Dobb, Cambridge 1951, 4, p. 359). We should add, however, that Ricardo took pains to emphasize the hypothetical and tentative character of this standard. If all commodities re- quired the same period of production, say one year, the quantity of labor employed on them would be "the nearest approximation to a perfect meas- ure" (*ibid.*, p. 364). Also the companion proposition that the quantity of

labor was the cause of value, was no more than an approximation: "Though it is by far the greatest cause it is not strictly the only one" (*ibid.*, p. 367). In this way the strait-jacket of his doctrine was loosened. The two principles, that the relative quantities of labor required in production in general measured relative value and that these relative quantities represented the dominant determinant of its changes, allow for exceptions. Following Stigler's terminology they should be described as an "empirical" theory of value in contradistinction to an "analytical" theory. Whether in other respects Miss Lipschitz's interpretation should be approved, for instance her view on Ricardo's ethical orientation and his opposition to capitalism (pp. 97-98, 120-21), may be left for later discussion.

Though the author is to be commended for careful treatment of various intricate questions, some technical deficiencies of her book should be mentioned. Obviously she omitted to check all her references. It so happens that a quotation from Karl Diehl's commentary published in 1921 is given as derived from a letter of Ricardo (p. 52). The names of some writers have been mixed up. The editor of the German edition of Ricardo's *Principles*, Heinrich Waentig, is called "Ottomar Waentig"—a confusion with the original German translator, Ottomar Thiele (p. 50, note 43). In the preliminaries of the appendix Miss Lipschitz reproduces almost verbatim several paragraphs written by Sraffa without indicating her source (pp. 125-26). The exposition is often redundant and repetitious. Instead of being analyzed systematically Ricardo's theorems are often discussed in the chronological order of the correspondence.

<div style="text-align: right">FRITZ KARL MANN</div>

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Economic History; Economic Development; National Economies

Théorie générale du progrès économique. I, *Les mesures des progrès économiques et l'idée d'économie progressive.* II, *Les composants:* 1. *La creation.* 2. *La propagation:* A. *Modèles microéconomiques.* By FRANÇOIS PERROUX. Paris: Institut de Science Economique Appliquée, 1956, 1957. Pp. 47; 89; 109. 1,000 fr. each.

Professor Perroux, director of the Institute of Applied Economics in Paris, is one of France's most distinguished economists, and one of the world's most prolific. (An addendum to the second *cahier* in the present series lists 193 of his books and articles.) His views on economic progress and economic growth (which he sharply differentiates—see below) should therefore be received with considerable interest in this country.

The three *cahiers* under review are parts of a larger study on the title subject. They deal, respectively, with: (1) indexes of economic growth, and the concept of a "progressive economy," (2) innovation, as a component of economic progress, and (3) the transmission (*propagation*) of innovation, as a second component of economic progress. The third *cahier* is concerned

with microeconomic models, i.e., innovation by the firm, and its transmission to other firms and industries under various market conditions. The next two *cahiers* in the planned series are to deal, respectively, with macroeconomic models, and the "signification" of economic progress.

In the first *cahier*, Perroux's point of departure is a criticism of the indicators of growth that are usually adopted as measures of economic progress, e.g., national product, per capita income and consumption, sectoral distribution of the labor force. His objections are of two sorts: the indicators do not accurately measure what they propose to measure; and what they propose to measure is relatively insignificant, i.e., is not what they *ought* to measure. On the first point, Perroux criticizes the standard growth indicators for their attempt to compare widely heterogeneous assortments of goods and services, for their lack of allowance for cyclical fluctuations, differences in the distribution of income (or consumption), and changes in international terms of trade. On the second point, he criticizes the standard indicators because, even if they were accurate, they would tell us nothing about what he feels they should be primarily concerned with: the extent to which a growing economy is a *progressive* economy.

> If we knew only what these indicators tell us, what would we know about economic progress? The answer is: very little. The reason is not only that any statistic requires interpretation. In addition, the concepts and the relationships underlying any statistics must derive from a theory, and the theory which presently underlies growth statistics is not satisfactory. . . . The theory is unsatisfactory because it is concerned with aggregate results and neglects the *process* by which the results are realized (p. 33, *cahier* no. 1)

From a normative standpoint, what the statistics should try to measure is not economic growth (*les progrès*), but economic progress (*le progrès*), by which Perroux means growth accompanied by an "optimum distribution of the attendant social costs," (i.e., *un amenagement optimum des charges . . . ou coûts sociaux*—pp. 34-35, *cahier* no. 1).

Why are we interested in economic growth? One answer, traditional in economics, is because it is presumed to involve commensurate increases in something we think of as "welfare." Perroux' point, essentially, is that the usual growth indicators, even if statistically acceptable, would not tell us anything about changes in welfare, because they neglect a number of social costs which may significantly reduce welfare: e.g., costs in human lives, and in individual freedom of choice.

I find several troublesome things about this argument. The first is the obvious one that the sorts of costs Perroux has in mind are not likely to be taken care of by any changes in growth statistics. Of course, we could (and should) try to get measures not only of *average* consumption, or income, per head in international or intertemporal comparisons of growth, but of the variances that are involved as well. If we make some convenient assumptions about diminishing marginal utility of income, perhaps this would help us appraise changes in welfare. But I doubt that this is really likely to help us

to arrive at judgments concerning the relative equitability in the sharing of the kinds of costs Perroux has in mind.

The other objection is a normative one. True, the usual indicators, no matter how much they were improved, would not tell us much about economic "progress." But for some purposes, it isn't clear that "progress," in Perroux' sense, is what we ought to be interested in. If what we're concerned with is, say, the relative military strengths of different countries, or the effect of economic growth on political stability, it is by no means clear that the allowances we need to make—even if we *could* make them—in the usual growth measures, would be those which take account of the social costs which concern Perroux.

Believing that the statistical deficiencies arise from an underlying deficiency of theory, Perroux is concerned with formulating a more adequate theory of economic progress. To do so, he suggests that:

> The enormous number of variables and interdependencies which affect economic progress can be divided into three groups designated by the terms innovation [*creation*], transmission [*propagation*], and signification (p. 37, *cahier* no. 1)

By "innovation," Perroux means changes in production techniques which result in lower costs or new products. By "transmission," he means the *horizontal* spreading of innovations to other firms in the innovating industry, or the *vertical* spread to other industries. I must admit I don't know precisely what he means by *signification*. Though this is to be dealt with in a later part of the study, Perroux refers to it at several points in the first three *cahiers*, as well. When he does, he uses terms like "social consensus," "ideology," and "economic relatedness" to describe the intangible "signification" that characterizes a "progressive" economy. If I follow him correctly, he has something in mind between the Augustinian *City of God* and the Frommian *Sane Society*. It is perhaps supererogatory to say that this hint of what is to come in a later *cahier* is more than slightly mystical, and less than very enlightening.

In his second *cahier*, Perroux takes up the first of his three "components" of economic progress, innovation. His aim is to contest the Schumpeterian and Wicksellian view that innovation derives from the small unit (Schumpeter's "innovating entrepreneur"), and that it is an individualistic process. According to Perroux, this view misconstrues the innovating process, which is *collective and cooperative* in its methods, its agents and its results" (p. 1, *cahier* no. 2).

Perroux regards innovation as typically a result of the cooperative efforts of large economic groups, rather than individuals, acting in an oligopolistic rather than perfectly competitive environment characterized by state intervention, rather than *laissez faire*. To support his contention, he presents extended essays on British economic development from 1760-1854 (the "first industrial revolution") and U.S. development since 1945 (the "second industrial revolution"). If the evidence he marshals is less than conclusive, it is nevertheless both interesting and provocative.

Related to his dissent from the individualistic view of innovation is a

criticism of the Schumpeter-Kondratieff interpretation of long cycle growth. His argument (pp. 33 ff., *cahier* no. 2) is, first, that in Britain during the 19th century, exogenous forces (e.g., war and monetary inflation) were actually more closely correlated with the inflationary phase of the long cycle than was the bunching of innovation, as contended by the S-K model; and, second, that consumption didn't rise relative to investment in the deflationary phase of the cycle, as the theory asserted. My impression is that Perroux's criticisms here are sounder than his inference that the weaknesses of the long cycle hypothesis derive from Schumpeter's individualistic view of innovation, or that they provide support for the collective view of innovation offered by Perroux.

Discussing automation and atomic energy in the U.S. economy, Perroux's comments on the "socialization" of innovation are especially suggestive. Their debt to Schumpeter is greater than one might infer from Perroux's inclination to differ with him wherever he can.

The third *cahier* presents some models of interfirm and interindustry relations as they affect the appearance and transmission of innovation. The simplified models consider innovation and its transmission under conditions of perfect competition, pure monopoly, duopoly and oligopoly. Perroux's argument is that a purely competitive model "yields the *worst consequences* with respect to the likelihood of innovation" (p. 21, *cahier* no. 3; italics in original). By contrast, pure monopoly—if accompanied by freedom of entry—and imperfect competition, under certain circumstances, are more likely to encourage innovation! Perroux's reasons revolve around the *means* and the *incentives* to innovate. In pure competition, the means are lacking because firms are small, and funds and facilities for research (i.e., in Perroux's terms, for developing and assembling relevant economic information) are limited. The incentives are lacking because the innovating firm cannot anticipate any protracted period of supernormal profits as a result of an innovation.

On the other hand, Perroux contends, in pure monopoly and oligopoly, the means for acquiring the informational inputs relevant to innovation are likely to be available as a result of previous supernormal profits. Moreover, the incentives to innovate are also likely to be stronger. In part, this is because of the oligopolist's opportunity to maintain his supernormal profits for a longer period by negotiation, collusion, and bargaining with other oligopolists, ("*effective* competition is a regulated game . . . ." p. 18, *cahier* no. 2). In part, the incentive to innovate arises from the oligopolist's fear of his present rivals, and the monopolist's fear of the *potential* rival. To preclude entry of the *potential* rival, and to strengthen the firm's bargaining relationship with his actual rivals, the monopolist and oligopolist, respectively, have keen incentives to innovate.

As concerns the *transmission* of innovation, pure competition emerges with equally low scores, because the terms of the model preclude transmission either via new-product creation, or price reductions by an innovating firm. (On the latter point, Perroux seems to be inconsistent in arguing that an innovating firm, under pure competition, cannot retain windfall profits for any period of time, even though it is presumed that its innovation has no

effect on market price. Actually, it would seem that if its innovation does not affect price, it *does* retain the innovational profits, and consequently acquires an incentive to innovate.) On the other hand, certain types of imperfect competition score more highly as transmitters of innovation, both within the industry, and to other industries. Again, the "potential rival" (as seller, or as monopsonistic buyer) plays an important role in stimulating transmission by an innovating oligopolist, as do the accepted "rules of the game," and the fear of government intervention if the results of innovation do not result in lower prices to other industries and to consumers.

Perroux admits that some forms of imperfect competition may stifle innovation and its transmission, e.g., cartels, monopolistic competition (i.e., *polypole hétérogène*). And he is critical of Galbraith's optimism concerning the role of "countervailing power" as a guarantor of innovational transmission. Nonetheless, he is strongly inclined to the conclusion that innovation and its effective transmission depend on a combination of monopolistic and competitive forces, and still more strongly inclined to the view that the forces that are involved extend, beyond market price, to considerations like the "rules of the game" that are accepted in interfirm bargaining relationships, and the role of the state as innovator or guarantor of its transmission.

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Economic Backwardness and Economic Growth. By HARVEY LEIBENSTEIN. New York: John Wiley; London: Chapman & Hall, 1957. Pp. xiv, 295. $6.95.

Professor Leibenstein has written an interesting book on the pure theory of economic development. Its central theme is the now-familiar one that a "critical minimum effort," i.e., a particularly powerful stimulant or series of stimulants (regardless of character), is needed to send an underdeveloped economy off along the path of sustained growth in per capita income. Backward economies are regarded as possessing the characteristics of "quasi-stable" equilibria; they are stable in the small, in the sense that small stimulants may induce change in some of the variables of the system, but the crucial one, per capita income—the index of growth—will tend to recede to its initial values. There are too many (well-known) economic and cultural obstacles to surmount.

To hurdle these barriers to sustained growth, powerful stimuli are needed, and because the equilibria of backward areas are not stable in the large, these stimuli can be expected to set the growth process going. At certain critical minimum income levels, we can ordinarily expect "human investment" to occur, that is investment in various types of human productive capacities; higher levels of saving can and ordinarily will evolve, making possible and economically feasible the broad range of investment outlays required in the initial stages of development; the onset of attitudes conducive to a decline in fertility rates will take place, as well as the breakdown of attitudes resistant to change; and so forth.

Of equal importance, a large enough stimulus will induce a rapid income growth at the outset such as to create an atmosphere of growth. On the one hand, this atmosphere is needed to engender attitudes that in themselves contribute to any sustained growth process, like the willingness to engage in new forms of enterprise and work. On the other hand, an atmosphere of growth is needed to prevent the development of attitudes hostile to growth, like the desire to erect various protective devices for entrepreneurs and workers alike, as well as to discourage familiar types of trading and speculative activities (in land, inventories, and other types of wealth) that tend to divert energies, skills and resources away from growth-promoting enterprises. A similar (if somewhat strained) argument is set forth relating the growth in use of knowledge and skills to anticipated rates of per capita income growth.

The point of all this is that a synchronization in the expansion of all resources is necessary if sustained growth is to be achieved. To bring this about, a minimum rate of growth, which only a large stimulus can produce, is needed to overcome the inertia and fears of the various population segments, minimize failures and losses, and thereby avoid or limit setbacks leading to pessimistic anticipations of the future and hence debilitation of the growth process.

In the course of this discussion, there is an interesting mixture of old and new ideas about the determinants of entrepreneurship, knowledge, and skills, and their respective growth propensities. If there is fault in this argument, it may lie in its failure to distinguish adequately a stage in which many of the prerequisites of sustained growth must be first established from the "take-off" stage itself.[1] The picture drawn portrays a greater element of abruptness in the development process than may actually exist.

Complementing this theory of the critical minimum effort is a theory of population growth that makes the latter first an increasing and then a decreasing function of per capita income. Because of this relation, Leibenstein rightly argues, common efforts to determine required growth rates in national income by referring to current rates of population growth are wrong, since they overlook the rising rate of population growth that is induced in the early stages of development by higher per capita income. It is part of the critical-minimum-effort thesis that the initial stimulants must be large enough to sustain per-capita income growth until this growth and its concomitants propogate motivations conducive to fertility decline.

Preceding this extended discussion of the critical-minimum-effort principle are chapters devoted to the basis of per capita output as an index of development, the meaning and nature of the quasi-stable equilibria marking backward areas, the static and "dynamic" characteristics of economic backwardness, a (not very convincing) theory of underemployment in densely populated areas, and a chapter upholding the Clark-Fisher thesis about development and occupational distribution, but from a novel viewpoint related to increased specialization as a function of rising per capita output.

Following the discussion of the theory of the critical minimum effort is a chapter devoted to computations of different population growth rates, data on

[1] Cf. W. W. Rostow, "The Take-Off into Self-Sustained Growth," *Econ. Jour.*, Mar. 1956, *66*, 25-48.

capital-output and (real) saving ratios as well as income growth rates, all
designed to give some idea of the magnitudes of these crucial variables. In
virtually all cases we are confronted with disheartening contrasts between
growth requirements and existing possibilities, such as a 5 per cent or more
required growth rate in national income, a (net) saving rate of 11-26 per cent,
etc. One wonders how such vigorously expanding economies as Mexico,
Burma, and others do it on a saving ratio of less than 10 per cent, or how
many currently advanced economies managed to develop at average growth
rates well below 5 per cent.

A concluding chapter is devoted to a critical examination of a number of
current investment criteria for economic development. The nub of Leiben-
stein's criticisms is that these criteria have in common the failure to take
account of certain long-run, feedback effects of different investment policies
on the rate of population growth, future saving habits, the expansion of en-
trepreneurship, labor skills, etc. However, much of what he says seems to be
consistent, particularly, with the "social marginal productivity doctrine,"
broadly interpreted, and what he offers in place of this principle, at least,
seems to be of little help in the implementation of actual investment policies.

Throughout the book there is one set of related assumptions, basic to much
of the argument, that bears questioning. One of these is the common assump-
tion that a rising ratio of labor to capital, because of a faster growing popula-
tion, will be accompanied by diminishing marginal product of labor. How-
ever, there is a multitude of production functions (linear and homogeneous
and nonlinear and nonhomogeneous) that show constant or increasing
marginal returns to labor under the posited conditions. It is necessary to specify
and justify the shape of the production functions assumed in arguments about
diminishing returns. There is no general and necessary reason why labor's
marginal product need fall. Or at worst, it may fall asymptotically.

Moreover, in the world of reality, there are many underpopulated areas,
like much of South America, where population and labor-force growth is
a positive blessing, where labor must be considered to be operating under
increasing (or constant) returns. In aiming at a general theory of economic
development, Leibenstein loses much of his generality by concentrating his
analysis primarily on problems of densely populated areas. This emphasis on
the latter areas also suggests the author's belief that the low per capita in-
comes found there originate to an important degree in relatively limited sup-
plies of land and natural resources; this is a very common, if not correspond-
ingly illuminating, view. But countries like Japan and Holland, with high
population densities and limited natural resources, on the one hand, and
countries like Brazil and China with low population densities and relatively
abundant natural resources on the other, testify that the crucial factor,
common to all underdeveloped areas, lies elsewhere. "Elsewhere" is the wide
disparities in economic organization and efficiency—in failures of the under-
developed areas to adopt their technologies to their scarce resources. Popula-
tion pressure is neither a necessary nor sufficient condition for underdevelop-
ment.

Accompanying the assumption of diminishing returns to labor is the further

and utterly erroneous one that diminishing returns must inevitably lead to diminishing per capita income. This confusing association of these two phenomena is not uncommon in recent growth literature and has been responsible in part for many spurious policy suggestions for proper allocation of inputs. The behavior of per capita income clearly depends on the relative rates of growth in output and population, and there does not appear to be anything general that can be said about this. Underdeveloped areas may well possess some of the characteristics of stable equilibria, as Leibenstein says, but it is doubtful that the reasons lie in either or both of the assumptions just discussed (although it must be noted that he does discuss other reasons).

As a corollary of the assumption of diminishing marginal product of labor as the labor-capital ratio rises, Leibenstein argues not only that a reduction in this ratio will raise labor's marginal product, but that his theory implies maximization of capital per head. However, this alleged implication does not follow at all. Even in principle, the behavior of labor's marginal product with respect to reductions in the labor-capital ratio depends, again, on the postulated shape of the production function(s) assumed. Nor is our experience with these relations uniform. At times, output per worker (at least) has increased faster, and at times more slowly, than capital per worker. On a cross-section basis, too, there is no exact correspondence between capital per worker and output per worker in different industries. This last suggests what is most probable, *viz.*, that output is not the same function of capital stock in all projects.

Leibenstein bases much of this part of his argument on feedback effects mentioned earlier. But these are not always fully convincing, and until they can be placed on a firmer basis, there is danger that such policy prescriptions, ignoring present and known relative resource scarcities and differences in production functions, will slow down the rate of per capita income growth, or conversely, make growth more costly and apparently more formidable than it is.[2]

There are other points that raise doubt in a reviewer's mind, but these are relatively minor, and do nothing to impair the conviction that the author lends to the critical-minimum-effort thesis. Prospective readers are warned, however, to be prepared for a difficult time of it, perhaps unnecessarily so. For the book is encumbered by a great deal of effort at abstract analysis that seems labored in that much of it contributes only marginally to our understanding of the development process. Characteristic of this is the existence of 58 diagrams, all original, but many questionable as aids to the discussion. Nonetheless, the book is recommended reading for students of economic, development.

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[2] For further criticism of the prescription of maximization of the capital-labor ratio, see F. Bator, "On Capital Productivity, Input Allocation and Growth." *Quart. Jour. Econ.,* Feb. 1957, *71*, 95-105; and O. Eckstein, "Investment Criteria for Economic Development and the Theory of Intertemporal Welfare Economics," *ibid.*, pp. 65 ff.

Prosperity through Competition. By LUDWIG ERHARD. New York: Praeger,
    1958. Pp. xii, 260. $5.00.

This book written by the Vice-Chancellor and Minister for Economic
Affairs of the German Federal Republic in collaboration with Wolfram Langer
was published originally in Germany under the title *Wohlstand für Alle* (Pros-
perity for All). The English version was translated and edited by Edith
Temple Roberts and John B. Wood. The reason for the change in title is not
given. Could it be because the word "competition" is likely to evoke a more
sympathetic response from the competitively-minded American public as
compared with the German reading public?

In the words of the author this volume "makes no claim to be theoretically
systematic, but is meant rather to give to the German people some sort of
account of progress so far, . . ." (p. 247). That it does, but again not in a
systematic way. The reporting of German economic progress is interspersed
with a great deal of economic, political and social theorizing on a popular,
political-platform level. The book is really a combination of straight economic
reporting, a vigorous presentation of the importance of the principles of the
soziale Marktwirtschaft (social market economy) in bringing about German
economic recovery and an equally vigorous presentation of the importance of
the Minister for Economic Affairs firstly in establishing the market economy
and secondly in defending the principles of free economy against the vigorous
and persistent onslaughts on the part of social democratic and other pro-
ponents of planned economy and economic *dirigisme.*

The story of German economic recovery has been told and retold and the
book contributes little new. One also looks in vain for some new light on the
nature of the widely used term "social market economy." The statement that
it is merely an application of the principles evolved by the West and aims
at "overcoming the age-old antithesis of an unbridled liberalism and a soulless
State control" and at "finding a sound middle way between out-and-out free-
dom and totalitarianism" (p. x) is generally acceptable as far as it goes, but
it is not much help in charting the actual course of economic development.
There is no doubt that the dismantling of national socialist controls, the
stabilization of currency, and the reintroduction of a relatively free market
in 1948, invigorated by Marshall Plan aid and the influx of millions of skilled
workers from the regions to the East contributed greatly to economic recov-
ery—a story eloquently stated and restated in the volume. The author, how-
ever, goes too far when he states that the course of German recovery has
rendered obsolete old laws of the cyclical character of economic development.
According to the author, seven years are said "to cover the period in which
recovery, boom, decline and crisis developed, followed by a convalescence
during which healing powers gathered strength to start the next cycle. It is
now almost nine years since I became responsible for German economic policy,
during which time this rigid rhythm has been broken, thus achieving a steady
upward trend of the economy, linked with full employment and a rising
volume of goods for sale" (p. 2). The recession in the United States and else-
where and the gathering clouds on the German economic horizon render such
a statement decidedly premature.

In presenting the statistical data to the American public the translators should have used the term billion, the American equivalent of the German milliard. In a number of instances milliards are quoted instead of millions (pp. 32, 34, 43, 94).

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Economic Survey of Europe in 1957. U.N. Economic Commission for Europe. New York: Columbia University Press, 1958. Pp. xii, 381. $4.00.

In its usually lucid manner, the Economic Commission for Europe again presents an informative and interesting report. Featured are studies of the international trade patterns of European countries, input-output analysis for Western Europe, review of manpower and employment trends in Eastern Europe, and a survey of recent economic developments on the continent.

In Western Europe, an increase in three channels of demand, consumption, investment and exports, has accounted for the phenomenal expansion of industrial production since 1953. The undesirable by-product of this upsurge has been a spread of inflation throughout the area with unfavorable effects on the balance of payments of most OEEC countries. The year 1957 therefore found most of these countries formulating anti-inflationary policies in an attempt to protect their price level stability and their international reserves. With the notable exception of France, most countries were willing to tolerate unemployment and a slowdown in the rate of economic growth in order to guard their balance-of-payments position. The necessity of choosing between these objectives was made acute by the limitation on the available policy instruments. For various reasons, great reliance was placed on monetary measures almost to the exclusion of fiscal and direct controls.

This year's analysis of economic conditions has benefited from the use of input-output models for all OEEC countries combined and for seven individual member countries. The models draw attention "to certain differences in the economic structure of several western European countries which are relevant to the formulation of policies for maintaining equilibrium in their economies" (Ch. 3, p. 1). Demand for resources in the combined OEEC model is classified as follows: public consumption, five categories of private consumption (in the models of individual countries these are lumped together), three categories of gross investment, and exports. Resources are shown as originating in: agriculture, eight industry groupings, services, imports, and indirect taxes minus subsidies. Relationships, such as the import content of various components of aggregate demand or the burden of indirect taxation borne by components of demand, can thus be studied.

While the interrelations embodied in the average coefficients are revealing, policy formulation can benefit more from marginal coefficients. These are estimated for the period between 1950 and 1955 and are shown alongside their average counterparts for 1950 (Tables 7 and 16 of Ch. 3). For all OEEC members combined, important differences between the marginal and average coefficients appear only in the consumption category where between 1950 and 1955 the shares of industrial output and of import rose while that of

services declined. For individual countries differences between the average and marginal coefficients are more frequent and pronounced. The predictive value of these differences is seriously impaired by the difficulty of tracing their exact causes.

Developments in the planned economies of Eastern Europe were dominated in 1957 by an increase in consumer income from higher wages, pensions and farm prices, and a slowdown in the rate of economic growth. The increased availability of consumer goods, however, did not match the rise in consumer income, and because official prices were held down shortages appeared in many sectors. A rise in savings somewhat relieved the pressure on the consumer-goods market.

More important than current developments are the changes in the process of planning and controls now taking place in Eastern Europe. These include: decentralization of economic decision-making and transfer of authority (primarily in the Soviet Union) from central ministries to regional boards, replacement of rigid five-year plans by more general directives for 10-15 years, increased emphasis on international specialization and on integration of economic plans within Eastern Europe, and finally, a greater attempt to achieve economic efficiency even to the extent of relying on market forces to accomplish it. These of course are general trends with considerable variations among countries. Yugoslavia (and to a lesser extent—Poland) went farthest in allowing production patterns and resource allocation to be affected by market forces. It also relies more than other countries in the region on financial and fiscal measures, rather than on output controls, for inducing conformity with central plans.

Included in the report is a discussion of demographic and other factors influencing labor supply in Eastern Europe. Of particular interest is the description of employment and manpower planning, designed to assure enterprises the manpower needed to achieve output goals. To this end a combination of social pressures, administrative measures, and incentives has been employed. The labor demand of an individual enterprise is controlled through its total wage bill. With wage flexibility on the one hand, and some flexibility of labor supply on the other, the plant manager is left with considerable leeway to vary his labor force. In addition, long-term assessment of labor resources and needs is undertaken centrally.

The labor markets in Poland, Hungary, Rumania, Bulgaria and Albania went through three stages in the postwar period. Up until 1951, they were characterized by a growing demand for reconstruction and industrialization which was met by abundant labor supply. During the second stage, 1951-55, the further expansion of demand began to result in labor shortages. Since 1955, the demand for labor has been on the decline because of the slowdown in investment activity and reduced availability of raw materials. This decline has coincided with an increase in the supply of labor, brought about mainly by the reduction in the armed forces and repatriation of refugees from Russia, and has resulted in both overt and disguised unemployment. An increase in the rate of economic growth would not necessarily be the best solution to the problem since in a planned economy investments have no genuine multiplier effect.

The volume of employment, "does not depend on the volume of effective demand, but is rather a function of available resources and adminstrative deci-
sions as to their utilization" (Ch. 2, p. 43). Thus the present policy of improving the consumer's lot is not in conflict with the other requirements of the labor market.

By contrast Eastern Germany and Czechoslovakia have suffered continuously from shortages of labor, while in the Soviet Union the available size of the labor force becomes a factor limiting the growth of the economy. This suggests that international cooperation in the planning of labor needs may be fruitful.

Newly available data on the pattern of Eastern European trade shows a high degree of concentration within the region. The Soviet Union has become a net importer of manufactured goods and a net exporter of fuels and raw materials, with a reverse situation in the rest of Eastern Europe. Future trade developments depend on the willingness of the Soviet Union to continue in its present role. But with further industrialization of Eastern Europe, expansion of trade with the raw-material-producing nations of the West would appear to be increasingly advantageous.

MORDECHAI E. KREININ

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Economic Developments in the Middle East 1956-57. New York: United
Nations, Department of Ecomonic and Social Affairs, 1958. Pp. ix, 163.

This is the sixth *Supplement* to the *World Economic Surveys* prepared each year by the U. N. Bureau of Economic Affairs. Following the theme of the *World Economic Survey* of 1957, the Mideast *Supplement* deals with economic forces that have been responsible for inflation in the Middle East.

Due to the lack of sufficient statistical information the Supplement addresses itself to the economic trends in only eight of the Middle East economies. An over-all picture of economic development in the area is presented at the beginning. This summary is followed by separate reports on the availability and use of resources in each individual country against the background of the country's special economic features. More than 30 tables dealing with major phases of economic activity in the region are grouped in a statistical appendix and briefly discussed at the end of the reports.

The most discernible single trend in the Middle East economies reported in the Supplement is a rise in real gross national product and income during the 1950-1957 period. The growth of income has been the result of expanding investments in agricultural mechanization, improvement in transportation services, and a faster rate of growth in industry and other services. In almost all of the region, gross investment has steadily increased and in most cases actually surpassed the rate of increase in income. The percentage of gross investment out of national product in 1957 stood at a low of 10 per cent for Egypt, a moderate 15 per cent for Iran, Lebanon, and Syria, and a high of about 25 per cent for Iraq and Israel. Due to substantial differences in these countries in the availability of resources, aggregate investment, population pressure and other economic forces, the increase in domestic product has not been uni-

form everywhere. The rate of growth has been greatest in Israel, Syria and Lebanon (8-12 per cent annual average) and lowest for Egypt and Jordan, with Turkey, Iran and Iraq occupying a middle position (estimated 6 per cent annual average). The absolute growth of output, however, has been accompanied by varying rates of increase of population in the region resulting in a much smaller rise in per capita real output and consumption.

The prime mover of development activity in almost all countries has been the government. The share of the public sector in the national economies of the Middle East has ranged from 15 per cent in the case of Lebanon to about 50 per cent for Israel. Public expenditures in the entire region have increased several times since 1950. The increase in government outlays has been lowest for Egypt (150 per cent) and highest for Israel (900 per cent). Outlays for development, defense and welfare activities (education and health) have received the lion's share of government expenditures in all 8 countries. Except for Lebanon and Syria, where investment by the government is still a fraction of total investment, the development effort and the resulting rise in the national product in the rest of the region have been fostered mainly through public investments.

For all countries under review, expanding aggregate demand has outstripped domestic resources. As a result mostly of a rapid rise in government expenditures, wholesale prices and costs of living have moved steadily upward. This pressure on the price levels has been accentuated by rapid increases in population and rising industrial wage rates. While the price rise has been steady and steep for Israel and Turkey (12-15 per cent annual average) it has been much milder for Egypt, Iran and Iraq (2-5 per cent). In almost all countries, costs of living have increased faster than wholesale prices indicating in most instances the relative inelasticity of supply of domestic products (particularly food and construction materials), and the relatively higher dependence of wholesale prices on world market prices. For the region in general, and particularly in the case of countries with a more or less stable wholesale price index (Jordan, Lebanon and Syria) the pressure of rapidly rising aggregate demand has been alleviated by a large and consistently growing import surplus.

The excess of annual imports over exports (excluding oil) has been as high as 60-80 per cent of total imports for Iran, Jordan, Iraq and Israel. The deficits in the balance of payments have been financed by various means: in Iran and Iraq by increasing oil revenues and domestic expenditures by oil companies plus sizable amounts of U.S. military and economic aid; Jordan, by foreign grants and loans, mostly from the United States and Britain; Egypt, by use of its large sterling balances accumulated during the second world war; Syria and Lebanon, by acquiring foreign private donations, private capital, and some foreign aid; Israel, by means of German reparation payments, private donation, U. S. aid, and foreign short- and long-term capital; Turkey, by suppliers' credits, U. S. grants and long-term loans.

What the future holds for the region is moderately bright in the case of Iran, Iraq, Lebanon and perhaps Syria. The expected rise in world oil consumption, ensures Iran, Iraq, and Syria of increasing oil royalties. The threat of inflation, however, although not yet imminent in these countries is not re-

mote since both the consumer demand and the pressure of private investors on domestic resources make themselves increasingly felt. Egypt, Israel, and Turkey, on the other hand, are expected to find the going rough in the next few years. Egypt's sterling balances are being rapidly depleted and unless new sources are found for financing imports development activities will have to be curtailed. With reparation payments being exhausted and the rate of private capital imports slackening, Israel also will find it difficult to maintain its present high standard of living for an increasing population. The consequence of increasing public debts and the "irreducible" size of essential imports make the future also grim for Turkey. Without a substantial increase in exports and/or external aid it is doubtful whether Turkey's development activities can be maintained without jeopardizing further the stability of Turkish currency.

The Supplement does a good job of assembling under one cover extensive and widely scattered information regarding the economies of the region. But on the whole, it suffers as much, if not more, from the same shortcomings as its predecessors. For the most part, the text provides a broad description of some general problems faced by the region and very little analysis of specfic conditions and solutions. A good portion of the nontabular information is little more than a verbal account of the accompanying statistical tables. Despite the experience acquired during the last few years in collecting and interpreting data, the quantity and quality of statistical information are improved but very little.

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Economic Opinion and Policy in Ceylon. By HENRY M. OLIVER, JR. Durham: Duke University Press; London: Cambridge University Press, 1957. Pp. xiii, 145. $3.50.

This small volume indicates effective use of a Fulbright year abroad. It is a case study, historically oriented, of nationalism at work. Oliver suggests a more descriptive title might be: "A Partial History of Nationalist Economics, Economic Ideology, and Development Policy in Ceylon, 1916-1956, with interpretations and reflections by the author."

It does not investigate such standard topics as taxation, monetary policy, and labor issues. But the author's choice of emphasis in studying the Ceylon economy is very understandable. More often than not economists come back from underdeveloped areas with the conviction that political and social developments will have more influence on the economic future of their region than economics narrowly defined.

In what kind of economic world has Ceylon been living, as the Ceylon nationalist sees it? First, the British colonial government has done wrong by Ceylon in several ways. In the nineteenth century the government took title to wide areas of uncultivated hill land in central Ceylon, and sold them to British planters. This action seems to Ceylonese critics legalized theft, since the lands were something like Europe's medieval commons—sources of pasture and fuel, and potentially areas for expansion of village cultivation. Another charge is that taxation took income away from Ceylon, and gave it to foreigners—that

is, to British civil servants—in salaries and pensions far higher than those
received by most educated Ceylonese. A further complaint is that too few Cey-
lonese were able to get into government work. Finally, a textile quota of 1934,
discriminating against cheap Japanese imports, aroused indignation. The quota
looked like "a wealthy country's decision to thrust part of the burden of its
own unemployment upon desperately poor Ceylon" (p. 16).

Second, the British planters and traders seem to the nationalists to have been
exploiters of Ceylon's economy, not contributors to it. They are—to a lessen-
ing extent as political independence has come and Ceylonese have advanced in
the business world—accused of "monopoly." Monopoly seems often to mean
only that all the trading firms used to be, and many still are, British. But some-
times the term has indicated restrictive agreements. And Ceylonese have felt
themselves barred from jobs: British managers have held the best paying ex-
ecutive jobs, and Indian laborers work on the tea and rubber estates. Here the
threat is to Ceylonese culture and national existence, as well as to Ceylonese
livelihood.

A dash of Marxist theory readily transforms these criticisms into a picture
of British "colonial capitalism" systematically exploiting the land. Britain, and
the West in general, have gotten raw materials cheaply in Ceylon, sold manu-
factures dearly, and invested profitably. They discourage and seek to block
Ceylonese enterprise and national economic development—for example, by ty-
ing government aid to agricultural programs, and by having foreign capital
enter Ceylon in order to choke off domestic industrial growth.

Through what policies will Ceylon achieve economic advance? In general,
Oliver finds Ceylon opinion growing more interventionist and equalitarian as
time has gone on (pp. 35-36). The following are sample views of the past three
decades. In the 1920's, Japan was considered a model, and Japanese experience
earned frequent enthusiastic references. The government's obligation was to
supply Ceylonese investors with necessary funds, whose lack was the chief
block to Ceylon's economic growth. Legislators also demanded government
support of research, training of management, and protection from foreign
competition (pp. 93-94). A Banking Commission report of 1934 urged new
industries to cater mainly to the home market, and to insulate the
economy from the vagaries of foreign markets. In the late '30's, an influ-
ential adviser urged cottage industries as the normal and desirable next step in
Ceylon's progress. In 1946 an Executive Committee of Labour, Industry, and
Commerce urged the development of manufacturing industry, and recom-
mended that "basic" industries should be state monopolies (pp. 98-99); but
the able Minister of Industries Vaithianathan later argued that "successful
state enterprise requires harsh compulsions," and that "in a democracy, state-
owned and state-managed industries do not prosper" (p. 103). Economist
Gamini Corea in 1950 urged opening up unused lands as an obvious way of
cutting food imports and so freeing funds for import of capital goods. Econ-
omist Das Gupta of the Central Bank and University stresses the island's
lack of enterpreneurship as a major obstacle to growth.

The political left has furnished the most consistent support for rapid indus-
trialization. N. K. Sarker was in 1951 confident that "a resolute government

could achieve rapid development and that industrialization was the most effec-
tive road to higher incomes" (p. 108). Trotskyist leaders Colvin de Silva and
N. M. Perera reasoned that the farmer must sell his surplus to an industrial
population; and communist Pieter Keuneman approved of Sarker's argument
that surplus agricultural labor could supply the real resources needed for in-
dustrialization.

In its campaign platform of 1956 the present governing coalition urged na-
tionalization of all essential industries, "including foreign-owned plantations,
transport, banking, and insurance," and administration by the state of all key
industries. But the new government has been very cautious about implement-
ing its program.

What has been the effect of nationalist sentiment and of government poli-
cies on Ceylonese incomes? Oliver is cautious, but clearly feels that on net
balance the effect has been injurious. "Nationalist sentiment has raised income
only through its favorable effects on Ceylonese ambition and efforts. Restric-
tionist policies which nationalism has demanded probably have on balance
lowered Ceylonese incomes somewhat, and foreign investors' fears of an un-
certain future have been distinctly unfavorable for the Island's economic ad-
vance. The Government's industrial ventures have not to date amassed an im-
pressive record, but questions of social costs and possible infant industry
gains are especially tricky. Agricultural ventures must have added substan-
tially to national income, but a somewhat different allocation of the resources
employed may well have added more. . . . Heavy welfare expenditures have
slowed capital formation" (p. 129).

A fervent nationalist might or might not accept these evaluations. But even
if he did, he might still judge Ceylon's policies a success. He would place a
high social value on replacing foreign managers and employees with Ceylonese,
and on the progress made toward insulating Ceylon's economy from depend-
ence on imports and on export markets.

Oliver's book is carefully written, well documented, and moderate in state-
ment. It gives, within its range of coverage, a responsible economic evaluation
of Ceylon's policies.

<div style="text-align: right">THEODORE MORGAN</div>

University of Wisconsin

*Studies in Economic Development, with Special Reference to Conditions in
the Underdeveloped Areas of Western Asia and India.* By ALFRED BONNÉ.
London: Routledge & Kegan Paul, 1957. Pp. x, 294. 32 s.

Dr. Bonné writes out of his long experience in the Middle East where he is
professor at the Hebrew University, Jerusalem. He personally believes that
there is an "urgent necessity to generate economic progress by well co-ordi-
nated measures of national and international policies, without relying on
'infectious trends' of economic activities and without infringing upon the sover-
eignty of national governments" (p. 7). His advocacy of sponsored develop-
ment is the keynote of the first chapter. This is followed by international com-
parisons of population, income, and consumption, grouped under the heading,
"Measure and Challenge of Inequality."

In the remaining three-fourths of the book, entitled "The Move towards Adjustment," the subjects considered are: the role of incentives, the industrial and agricultural sectors, land tenure problems, and development finance. Bonné calls the theme of these chapters an assessment of the "prospects" for development. A theory of the initiation and maintenance of development applicable to present-day underdeveloped countries is outlined in the last chapter. There is an appendix on resistances to economic change imposed by social and political institutions.

The book's main task of considering the "prospects" for development is never clearly defined. Nevertheless I have understood the author to be inferring the direction and pace of development in the Middle East and India from published empirical materials. The treatment suffers in some cases because data is unavailable or apparently unknown to the author, and in other cases because he moves so rapidly from one complex multiple-country problem to another that space is inadequate for proper presentation or consideration of data. Consequently the main chapters are loosely organized collections of information about economic and policy trends interspersed with theoretical propositions and even policy proposals with little connecting analysis. The author appears to know more about the noneconomic aspects of the subject than most economists, but this strength is obscured by the faults just mentioned. Economists will find in this work an excellent example of the perils of attempting too much in a field where the empirical materials and theory are as underdeveloped as the countries themselves.

The principal value of this book is its exposition, in the final pages, of a theory of development held implicitly by many economists and policy-makers in the region. It begins with a conventional definition of development as an increase in output and consumption per capita, an increase in the proportion of the population employed, and an "improvement" in income distribution. Stagnant communities are transformed into developing economies by "implanting" new incentive mechanisms, techniques, institutional arrangements and public policies. The "implanting" agent is the government. Private carriers of economic change are ruled out for contemporary underdeveloped countries. Governments are compelled to assume this role by nationalistic forces which demand economic reorganization. Even governments that are closely controlled by interests which will be upset by development will initiate some economic expansion within the definition of economic development.

As an economist, I cannot say whether this expectation about the goal-choices of governments is justified. As an individual, however, I am skeptical. In the cases of India and Pakistan, for example, the adoption of genuine development goals—not the mere aggrandizement of special interests—has been associated with broadening the base of political power. Where that has lagged, as in Pakistan, so has the adoption of development goals. Even where appropriate goals are given, it has yet to be shown that the crucial economic changes are initiated exclusively or even primarily through the agency of government. Bonné should be encouraged to elaborate his theory and, if possible, to offer evidence of its consistency with experience.

FREDERIC C. SHORTER

Princeton University

Fomento—The Economic Development of Puerto Rico. By WILLIAM H. STEAD.
    Washington: National Planning Assoc., 1958. Pp. 150. $2.00.

One of the most interesting economic development experiments is in prog-
ress in Puerto Rico. The present volume, which is a study of the techniques
used in that experiment, is one of a series of works sponsored by the National
Planning Association in the general field of economic development.

Mr. Stead notes the change in the nature of the Puerto Rican development
effort since the second world war, the shift from a program based largely on
government investment to one of enticing private investors, expecially from
the United States mainland, to carry the principal burden of industrialization.
The success of the program is shown by the fact that by the end of 1957 a net
of 448 new industrial plants had been established in the island.

The author points out that the Puerto Rican government since the war has
sought to use its funds as a "catalyst." It has provided sources of external
economies which previously did not exist, but which manufacturing firms need,
e.g., transport, electric power and housing facilities, as well as education and
health services. The government has also undertaken the construction of a
number of factory buildings which it has then leased or sold to private enter-
prisers. It has loaned money to some firms. Finally, it has provided a wide
range of services, such as the training of workers, technicians and managerial
personnel, market surveys, and aid in establishing necessary business connec-
tions for new enterprises.

The fundamental purpose of Stead's study is to draw conclusions concern-
ing the applicability of the Puerto Rican formula of development for other
countries in a similar situation. He feels that Puerto Rico's access to the Uni-
ted States market, participation in the legal and judicial system of the United
States, and its opportunity to share the economic and political climate of this
country are the unique factors in the Puerto Rican experience. However, he
concludes that these are largely offset by the fact that Puerto Rico has fewer
resources available for development than most underdeveloped countries.

Stead feels that there are nine aspects of the Puerto Rican situation which
are "transferable" to other countries. These include the policy of putting prin-
cipal emphasis on private investment and using government funds mainly to
promote such investment; the provision of profitable conditions, including tax
relief, to encourage private investment; "intensive and well-managed promotion
programs . . . to secure new industries"; the provision of land and sometimes
buildings to interested entrepreneurs; an "excellent service program" to help
industries get started; the use of government corporations instead of govern-
ment departments to handle the state's share of the development program;
the policy of providing "supporting activities" such as roads, power, ports,
public utilities and so on; "effective central planning"; and "able and honest
government administration."

This reviewer believes that Stead has tended to underestimate the role
which access to the United States market has played in the Puerto Rican de-
velopment effort. Without this factor, most of the others cited would certainly
not have been so effective. Most of the industrial plants built in Puerto Rico
have been to serve the continental market. Only very recently has the program

evolved sufficiently to permit relatively important projects for serving Puerto Rico's own needs. However, Stead has certainly written the most succinct and in many ways the most interesting study of the widely publicized Puerto Rican development program.

ROBERT J. ALEXANDER

Rutgers University

Statistical Methods; Econometrics; Social Accounting

Input-Output Analyse. By HEINZ PLATT. Meisenheim/Glan: Anton Hain Kg, 1957. Pp. 121. DM 16; paper, DM 14.

This little book gives a compact but readable account of the present state of input-output analysis. It summarizes essentially the American literature, although a number of German works are also considered. Incidentally, the author with the longest list of relevant publications listed in the bibliography, is not Leontief but Hans Peter, presumably the person under whose supervision this book was written as a thesis.

After some methodological reflections customary in continental books of this kind, the author quickly gets down to a survey of the principal types of Leontief systems—closed, open, static, and evolutionary (Part I). Part II examines the theoretical foundations of input-output analysis. The author considers Leontief models first as an extension of Walrasian equilibrium analysis with production characterized by fixed technological coefficients. He proceeds to a closer examination of the concept of fixed input coefficients and of the assumptions on which they may be justified. This turns in particular on the famous substitution theorem of Samuelson which says that the optimal technological choices are made by the market once and for all independently of the production program, provided that there is no joint production and that labor is the only scarce factor. The author seems to have missed the essential point, namely, the invariance of the technological coefficients implied by the theorem. His own interpretation, that the Samuelson theorem merely makes an *ex post* statement, would lend rather poor support to using the same technological coefficients *ex ante* for predictions based on a new bill of goods.

The third part of the book, "Applications: Their Basis, Possibilities and Limitations," does not live up entirely to the ambitious wording of the title. It would have benefited by a substantive discussion of the empirical findings of input-output analysis and by a critique of the estimation method from a more properly econometric point of view.

This book does not pretend to make an original contribution or even to arrive at an independent evaluation of input-output analysis. Its principal merit is that of providing an interesting, clear, and fairly complete exposition of the technique of input-output analysis.

MARTIN J. BECKMANN

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Economic Systems; Planning and Reform; Cooperation

Wirtschaftssysteme des Westens (Economic Systems of the West). Vol. I,
   Belgique, Denmark, Great Britain, Japan, Niederlande, Oesterreich, Span-
   ien, Schweden. Edited by RUDOLPH FREI. Basel: Kyklos-Verlag; Tü-
   bingen: J. C. B. Mohr (Paul Siebeck), 1957. Pp. 247, DM 22.-; paper
   DM 18.-.

The general approach and methods of this symposium are similar to those
heralded, a century ago, by the historical school of economics and further re-
fined by the adherents of economic institutionalism about three decades ago. A
common denominator is the assumption that old and worn out economic
theories cannot be successfully replaced with new ones without an elaborate
empirical exploration of pertinent economic institutions, behavior and develop-
ments. Even the wording used in the editor's preface to this volume is obvi-
ously reminiscent of those earlier periods of economic thought.

Are the classical and neoclassical theories of monopoly, oligopoly, imperfect
and monopolistic competition adequate to deal with the "new competition"
based on "countervailing power" in the recent struggle between new "giants"
of sellers and buyers? Have relevant American developments been duplicated
by similar phenomena in other industrialized countries of the West? These
two problems are the starting points of the symposium, and their formulation
reveals the influence of ideas of J. K. Galbraith. In 1955, Calvin B. Hoover
stated the problems in the form of 15 questions relating to shifts toward a
more or less "directed economy."[1] In particular, his questions pertained to the
nationalization, cartelization and other forms of concentration of industries, to
governmental price, subsidy, wage, saving, investment, employment and na-
tional self-sufficiency policies, and to changes in the forms of competition, in
the control of corporations by stockholders, in the inequality of the income
distribution, in the size of social security benefits and in the proportion of na-
tional income flowing through governmental budgets.

On the basis of Hoover's formulation of the questions, and with the support
of The Twentieth Century Fund and The List Society, a questionnaire was
sent to economists of 16 countries with the request that they submit their an-
swers in the form of articles; half of the resulting papers constitute this
volume. It is planned to publish the other half later; these will deal with Can-
ada, Finland, France, Western Germany, Italy, Norway, Portugal and Switzer-
land.

The eight articles of the first volume—four in English, three in German and
one in French—reveal considerable variety with respect to the authors' adher-
ence to the questionnaire. The two extreme cases are the articles on Denmark
(by Bjarke Fog) and Austria (Wilhelm Weber). While the former strictly
follows the questionnaire to the extent of repeating all of its 15 questions, the
latter disregards and replaces them with a different approach to the examina-
tion of the classical free-competition model. Ranging between these two ex-
tremes, the articles on Belgium (Valéry Janssens), the Netherlands (C. Goed-

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[1] "The Relevance of the Competitive Laissez-Faire Economic Model to Modern Capi-
talistic National Economics," *Kyklos,* Vol. 8, 1955, pp. 40 ff.

hart, J. F. Haccou, P. Hennipman, J. H. van Stuyvenberg) and Spain (Higinio
Paris Eguilaz) excel by offering useful statistical and descriptive materials
grouped according to the economic structure of the countries concerned. The
articles on Great Britain (G. C. Allen), Sweden (Hugo Hegeland) and Japan
(Gen-ichi Abe and collaborators) also belong to this intermediate range and
display some analysis of national-income, behavioristic and historical relations,
respectively.

The contributors' task was to concentrate on developments from 1939 to
1956. Their efforts result in a substantial amount of useful information with
regard to particular data, events and institutions. At the same time, they are
cautious in making definite statements on over-all trends of development.

Nevertheless, three groups of countries can be distinguished with respect to
trends. The articles in the first group emphasize that efforts have been made to
maintain the pattern of free competition. Belgium, Denmark, Austria and the
Netherlands, approximately in this sequence, have successfully participated in
such efforts, although governmental economic interference has been more or
less significant and partially growing in them. The second group comprises
Great Britain and Sweden. They display fairly developed systems of central-
ized economic guidance and only weak attempts to defend private-enterprise
freedom. Spain and Japan belong to the third group, stressing even more com-
prehensive governmental planning on the basis of nationalist traditions. Inci-
dentally, articles on the still substantially agrarian Spanish and the Far-East-
ern Japanese economies appear out of place in this symposium on industrial-
ized countries of the West.

The total impression of the reader will be that the economic policies scru-
tinized reveal more and heavier shifts toward governmental economic planning
than toward freedom of private enterprise. Yet, it must be understood that
conclusions should be carefully restricted not only as to the eight countries and
two decades but also in view of other limitations of the questionnaire con-
cerned. Its scope includes essential endogenous economic relations of mater-
ial production, distribution and consumption, and even some political aspects.
However, a fundamental analysis of the relevant ethical, sociological and other
exogenous ideological bases could obviously lead to different conclusions.

THEO SURÁNYI-UNGER

Syracuse University

Tsenoobrazovánie v promyshlennosti SSSR (Price Formation in the Industry
of the USSR.) By D. D. KONDRASHEV. Moscow: Gosfinizdat, 1956.
Pp. 175.

This little book is a sample of the attention which Soviet planners are now
giving to the role of prices in their economy. The postwar period has seen
increased experimentation with price changes, and in just the past year
Soviet journals have entertained a fairly open debate on the "law of value"
and the nature of the price system which this law implies for the Soviet sys-
tem. Kondrashev's book, like the discussion in general, has done little to
clarify the issues of price formation, but it contains much material on Soviet
thought and practice in an obscure area.

Kondrashev's discussion of retail pricing is quite good. He explains clearly

that the main task of price setting here is to establish stability in the consumer goods market. Given a total sum to be collected from the population through consumer-goods prices, the proper approach is to forget about costs and to set individual prices in accordance with demand. Kondrashev fails to understand that the resulting heterogeneity in cost-price ratios implies something about long-run output adjustments (incidentally, Marxist concepts are neatly adapted to demonstrating the desirability of such adjustments in Akademiia Nauk SSSR, *Voprosy ekonomiki, planirovaniia i statistiki,* Moscow 1957, pp. 405-19), but his straightforward explanation of the relation of consumer-goods pricing to the diversion problem is a great improvement over the usual obscurantism with which this subject is discussed by Soviet authors.

In dealing with producers-goods pricing, Kondrashev is much more clumsy. The western conception of prices as indices of cost and value which permit the integration of partial optimizations with an over-all maximum is outside the ken of Soviet economists. Their Marxist legacy almost precludes it, and the Soviet institutional setup is not one in which prices could practically serve this conception. Kondrashev shares this debility and so his critique of Soviet price formation is correspondingly weak. He develops answers for only the simplest problems of price setting, as for instance in the case of raw cotton pricing. The base price for cotton is supplemented with bonuses for increased yields. These bonuses are very large, amounting in the highest yield category to twice the base price. At the same time the producers are docked for trash in the cotton, but only at the base price. So producers whose yields put them in the bonus brackets add a liberal increment of trash to their cotton, for which they are docked once and paid thrice. He sees that this price scheme is not conducive to economic rationality, but when confronted with more complex problems, he is helpless for want of a theoretical apparatus. For instance alcohol is produced in the Soviet Union from a variety of raw materials—grain, potatoes, molasses, all of which can be utilized elsewhere in the economy. Because of peculiarities in pricing the raw materials the cost of alcohol varies greatly depending on the raw material used. Alcohol is sold to a variety of users—as a solvent, as a raw material in the chemical industry, and for human consumption. The price varies according to the buyer—high for vodka, but cheap for the chemical industry, so that the price of tires will not be "too high in relation to other parts of the automobile." So the profit or loss of an enterprise depends mostly on what raw material it uses and where its output is sold. Using the simple concepts of marginal cost and marginal productivity, any student of bourgeois economics could suggest a satisfactory set of prices in this problem, but the Marxist professor struggles helplessly.

But no one expects price analysis to be the forte of a Marxist economist, and the main interest of the book lies elsewhere. For one thing, Kondrashev tells us a great deal about the administrative details of price fixing, and the history of price policy in general and in particular industries.

The book is also interesting for what it betrays about the preoccupations of those who control price formation. Kondrashev's main objective for a "good" price system seems to be to cover costs and also earn a small profit for

all producers. This objective is not drawn from any understanding of the relation of prices and rationality, but is rather to serve the narrower expedient of *khozraschet*, to make enterprises dependent on their revenues from sales, and to create a presumption that any enterprise making a loss is poorly managed. Somehow this is supposed to strengthen the hand of the controllers in forcing the managers to do their best. The mechanism of this effect is not explicitly explained, but there is some interesting discussion of the administrative difficulties of controlling a subsidy program. Even when a subsidy is given in the form of a fixed amount per unit of output (which in some ways is no different from raising the price) managers apparently find it relatively easy to get subsidies larger than planned. So they lose interest in efficiency and compensate wastefulness at the public trough.

<div align="right">ROBERT W. CAMPBELL</div>

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The Development of the Soviet Budgetary System. By R. W. DAVIES. New York and Cambridge, Eng.: Cambridge University Press, 1958. Pp. xxi, 373. $8.50.

The book under review consists of three parts: (1) Revolution and Civil War, 1917-20; (2) The Mixed Economy, 1921-29; (3) The Planned Economy, 1930-41.

Undoubtedly, this is the only work on the Soviet budget in Western literature that analyzes the subject so exhaustively. Two valuable characteristics of the book are noteworthy. To the author, the Soviet budget is not limited to income and expenditures. Dr. Davies evaluates the Soviet budget as a state financial plan, and on this basis he presents a picture, on the one hand, of the relationships between the budget and the individual branches of industry, and on the other hand, the relationship between the budget and the state plan throughout the Soviet period. The second valuable characteristic of the book is its objectivity. The author does not present an evaluation of the Soviet budgetary system and of the budgetary policies. He presents all the necessary data, and rightly leaves the evaluation to the reader. This objective picture would be particularly useful at the governmental level, where an attitude of wishful thinking often determines the analysis and interpretation of the Soviet economy.

Davies correctly stresses that the absolute centralization of the Soviet financial system was the logical result of the policy of forced industrialization. However, this statement is only correct for the past. Recently the system has been altered substantially, and it would now be incorrect to examine the centralization of Soviet financial and planning systems solely as an integral part of industrialization. On pages 329-32 Davies lists the measures of the Soviet government, starting with 1953, whose adoption considerably weakened the policy of centralization of Soviet finances. At present the decentralization of finances and planning, in connection with the recent reorganization of the industrial administration has progressed considerably, although the Soviet government has not rejected the policy of forced industrialization. In other words, the Soviet government has entered into a stage of economic development in which the basically unchanged goals of

industrialization may be reached without extreme centralization in the fields of budgetary and financial planning—so important during the initial stages of industrialization. In this connection it is important and appropriate to stress that for a long time we underestimated the flexibility and potentiality of the Soviet economy.

In the beginning of the concluding chapter, "The Budgetary System in Perspective," speaking of the Soviet budget during the second world war, Davies had a wonderful opportunity to demonstrate the military character of the Soviet budget during peacetime. This may be clearly illustrated by the fact that during the war, while the budgets of the United States and Great Britain increased many-fold, the Soviet budget did not even double.

It is unfortunate that Davies did not have the opportunity to cover the more recent developments of the Soviet economy and budgetary system with the same perception and detail as for the earlier period. Nevertheless the reader will find here all that is necessary for understanding the distinctive features of Soviet finances and budget under the conditions of a planned economy in the various stages of its development. Anyone interested in the Soviet economy can not afford to by-pass this valuable book.

T. Sosnovy

Library of Congress, Washington, D.C.

Business Fluctuations

First Report. By the Council on Prices, Productivity and Incomes. London: Her Majesty's Stationery Office, 1958. Pp. iv, 75. 2 S.

This Council, appointed in August 1957, to report "from time to time" on the economic state of Great Britain, is composed of Lord Cohen (chairman), Sir Harold Howitt, and Sir Dennis Robertson, who is reputedly the drafts- man of this first report. It begins with a brief factual review of the postwar years. In general the British economy has performed well, showing an in- crease of goods produced in all but two years; but the rise of prices at 4 to 5 per cent per annum since 1946 is wholly without precedent in peacetime during the past century. The extra sum of money paid out in 1956 com- pared to 1946 because of higher prices went one-half to wages and salaries, a fifth each to profits and to import prices, and one-eighth to indirect taxes. Wages rose more rapidly than salaries. From 1938 to 1956 the total wage and salary bill rose by one-half; the total amount paid out in dividends fell by one-fourth. Production rose at 3 per cent annually.

The phenomenal rise of prices and money incomes over the decade 1946- 1957 has been caused by an abnormally high level of demand, and this in turn is ascribable to the effort in the early postwar years to restore levels of consumption and to improve the social services; with Korea, to build up armaments; and latterly, to execute large schemes of capital development. Imports have not contributed to inflation since 1951 nor has the increase of money wage rates seemed to be the crucial factor.

Answering next the question as to how the value of money *should* be- have, the report argues that inflation should be stopped, not merely mod- erated. Special circumstances, such as a rise in the cost of imported food or

materials, may necessitate breaching the rule. But Britain is too much dependent on export markets to be able to tolerate an upward drift of prices and costs. Indeed, there is much to be said for a policy of *falling* prices with technical progress.   •

As a means of damping down demand, monetary policy can "play an essential though not an omnipotent part." The Council expresses the conviction that monetary policy should and can eliminate the last and most expensive units of output without producing a drastic check to production. And, despite the fact that continuous growth of investment is a desirable objective, there are certain limits which cannot be exceeded without inflation. In some quarters it is argued that, if private investment is to be curtailed, it should be done in a "selective" manner; but the practical objections to direct controls are very great.

The application of measures to restrain the intensity of total demand implies, with regard to labor, the following: reduction of overtime, some withdrawal of married women from the labor market, and some increase of unemployment. It implies also some decline in wage demands in the long-run interest of employment and standards of living. Part of the benefit of increased productivity should go to lower prices. If wages are always raised in every case of increased productivity to the full extent of the gain, the combined pull of these higher incomes on other prices and the push from the imitation of the higher wages will produce general inflation. In conclusion, the report expresses scepticism concerning the efficacy of price controls, cost-of-living subsidies, and the limitation of dividends to curb inflation.

The report of Lord Cohen's Committee is a model of lucid argument, perceptiveness and relevance, and economy of word and number. Including the statistical appendices, it is 75 pages in length; but its quality puts it in a small list of classics such as the Bullion, Macmillan, and Douglas reports. Congressional Committees, Federal Reserve, and Monetary Commissions in the United States might note that reports running to hundreds and thousands of pages are not always valuable in proportion to length.

The virulent attack on the report and its authors by representatives of the Labour Party is difficult to understand in view of the express disowning of the theory that the inflation can be ascribed directly and simply to a wage push. The "three wise men" of the Council may safely ignore these strictures and proceed in subsequent years to further demonstrations of their skill in the fine art of applied economics.

<div style="text-align: right">HOWARD S. ELLIS</div>

University of Bombay

Money, Credit and Banking; Monetary Policy; Consumer Finance; Mortgage Credit

Changes in Monetary Policy and Their Consequences. By OTTO VEIT. Frankfurt on Main: Fritz Knapp, 1957. Pp. viii, 215. DM 13,20.

This largely empirical study is concerned with changes in monetary policy which have resulted from the recognition of the limits of interest-rate policy

as a curb on the demand for credit, and the consequent shift to control of the supply of credit in the inflationary decade from 1946 to 1956.

The work has three major divisions. Part I summarizes those basic changes in the economies of most western countries which bear upon this shift from what the author calls "pure discount rate policy" which aims at the ultimate debtor, to "liquidity policy" which is directed at the banking intermediaries. The list is familiar: the increased use of deposit money, the use of monetary policy to insulate the domestic economy from undesirable external trends related to the abandonment of the automatic gold standard, and the greatly increased share of government in the national income of most countries. While these changes make strong central-bank action imperative, the reduced importance of interest qua cost makes it difficult to pass on to ultimate borrowers the effect of central-bank action. Veit recognizes the interest-sensitivity of construction and inventory investment, but he concludes that the fiscal neutralization of interest costs under current tax structures makes for a highly inelastic total demand curve on balance. The seeming paradox emerges that, while the effect of interest costs on the demand for credit has diminished and has thus caused a shift of emphasis to control of the credit supply, the central bank achieves such control by acting on the liquidity of the commercial banks, and here the chief instrument is none other than the interest rate. The other general tools of credit control are used in essence to make the official discount rates directly or indirectly effective.

Part II provides an analytical account of the success of central-bank policy in West Germany, the United Kingdom, the United States, Switzerland and Sweden in controlling the size of the money supply, while Part III details the performance of the German banking system in the period under study. Critics of the failure of American monetary policy to more fully curb inflation in this decade may derive some small comfort from the account of European policy. In both West Germany and Switzerland, the domestic inflation has been fed since the early 'fifties by a persistent balance-of-payments surplus and a high level of government-supported construction activity. Because of a shortage of marketable securities, the German Bank deutscher Länder was able to engage in effective open-market operations to support its restrictive discount-rate policy only toward the middle of 1955 and had to resort to the use of "rediscount quotas" and other broadside measures to shrink the size of the money supply. The Swiss national bank in this same period was restricted to the use of discount-rate changes, but since the official discount rate remained unchanged from 1936 to 1956 (partially to prevent an increase in the level of mortgage rates), this tool was more theoretical than real. The Swiss federal government has instead sterilized its budget surpluses periodically and has acted with the national bank to sterilize part of the gold influx. (Cf. J. Amberg, *Die Politik der Schweizerischen National bank*, Winterthur 1954, for the peculiarities of the Swiss situation.)

In Britain and Sweden the cheap-money situation reflected official policy favoring full employment and was altered only slowly. The British shift to tighter money began in 1951 but was not fully effective until 1955 al-

though reinforced by forced funding, the use of a high minimum liquidity ratio, selective credit controls and the traditional advantage which the Bank of England has in its exercise of moral suasion. Sweden kept interest rates low and the money market liquid via open-market purchases and simultaneously sought to neutralize this liquidity by strict reserve requirements and drastic profit taxation. The abandonment of this contradictory and unsuccessful course in 1954 gave somewhat more scope to discount-rate policy.

Veit concludes that where persistent efforts were made, central banks were successful in applying pressure to bank liquidity and in thus containing the inflation of the money supply within narrower limits. What is not considered in this analysis are the broader problems confronting money management—what can be called the internal and external limits of monetary policy. The former refers to the inability of central banks with the use of present tools to get at such complicating factors in the monetary situation as the behavior of velocity and the operation of the nonbank financial intermediaries. The author's concentration on M to the exclusion of V makes his analysis of the period under review incomplete at best. What may be called the external limits refers to the inability of monetary policy to control the total economic situation without the cooperation of intelligent fiscal policy. Relevant papers presented at the last meeting of the American Economic Association (published in this *Review* in May 1958) would furnish a useful addendum on these points.

Despite these limitations, the book provides a skillful, compact summary of recent monetary history in the countries analyzed. This is a work which might well serve as supplementary reading in any money and banking course, providing as it does a more sophisticated and detailed account of foreign monetary developments than is usual in most texts for such courses. There is no index and the translation is at times quite literal, but the presentation is on the whole clear and straightforward.

<div style="text-align: right">ANNA B. DUTKA</div>

New York City

The Monetary and Fiscal Policy of India. By K. C. CHACKO. Bombay, India: Vora & Co., 1957. Pp. xiv, 386. $4.50.

The presence of highly developed monetary institutions and almost universal scarcity of credit is a familiar paradox to students of underdeveloped economies. This situation is explained historically by concentration of western banking facilities catering to export requirements in the exchange sector and neglect of developmental possibilities in the broader subsistence sector of such economies. Nevertheless, such an environment affords unique opportunity for fruitful study of the relevance of western-oriented monetary policies to problems of developing economies. This the author of this volume has attempted to do for India, which in many respects has become a model for assessment of current developmental schemes. Unfortunately, while assembling much pertinent information on Indian monetary and banking institutions, the book fails to provide much in the way of interpretative insights.

A basic difficulty is that the title promises too much. Approximately three-fourths of the volume is devoted to a painstaking historical and legal description of the Indian money market, Reserve Bank of India, commercial banking structure, internal and external aspects of the monetary system, cooperative banks and rural credit problems, wartime finance, and financial aspects of the two five-year plans. While much of this is undoubtedly necessary background material, it seems disproportionate in view of the author's avowed purpose of investigating the type of monetary policy most appropriate to the Indian planning goals of maximum production and equitable distribution. Indeed, most of what he has to say on this question (fiscal policy occupies a minor role in the discussion throughout) is confined to part of a single chapter.

Essentially, his prescription is for the monetary authorities to assure a sufficient flow of cheap money for productive purposes as determined by the Planning Commission. The Reserve Bank, which is the key to his entire program, is called upon to adopt a more flexible bank (discount) rate policy, although flexibility seems desirable only in a downward direction. The author is doubtful as to whether an increase in the existing $3\frac{1}{2}$ per cent rate would actually help check inflation. He advocates selective credit controls as a means of resolving the conflict between monetary requirements of growth and the attendant short-run inflationary dangers. However, the analysis suffers from his failure explicitly to distinguish between these two situations.

Throughout the volume, the author seems to leap to hasty conclusions, without support of adequate or logical analysis. Thus, Britain's commitment to domestic full employment indicates her inability to maintain future exchange-rate stability and calls for India's withdrawal from the sterling bloc. Deficit financing need not be inflationary in India since the marginal productivity of capital is higher than in industrially developed countries (the sole evidence cited being the 18 per cent actual increase in national income during the first plan as compared with an expected 11 per cent increase!). Moreover, the reader is often left with the assertion of some cited authority as the ultimate word, with no further discussion by the author. It is a pity that he has not been able to utilize more effectively the high-quality statistical and documentary materials that obviously exist in his area.

SHELLEY M. MARK

University of Hawaii

Money and Banking. By the Committee on Money and Banking. New York: Pitman Publishing Corp., 1957. Pp. xix, 588. $6.00.

This book, a unique experiment in textbook writing, has given writing experience and encouragement to many economists. Fifty-nine teachers of Money and Banking were selected to plan and write the text cooperatively. The authors, who with the exception of two, had previously published little or nothing, were primarily drawn from privately endowed colleges and teachers colleges. By means of questionnaires directed to the 59 co-authors

the scope of the book was determined. Each chapter was outlined and written by a committee of two or three. Committee assignments were according to personal preference. There was mutual criticism of the text within each committee and between committees. An editorial staff checked for duplications or significant omissions.

The text has more correlation between chapters and unity within chapters than one would expect from a textbook written by 59 authors. In general, it shows careful planning of content, thorough coverage of subject matter and good scholarship. Its method enables each small section to have the benefit of more research and study than is likely when one author is responsible for all parts. There is, however, more unevenness of quality.

The most serious general criticism of the book is its failure to be a good teaching device. Too often it merely talks about a topic instead of trying to teach the relevant material. The frequent introduction of technical terms and economic ideas without sufficient explanation of their meaning might cause the student to become lost in a labyrinth of words. This criticism applies especially to parts of Chapters 1, 2, 5, 16, 17, 18 and 22. The wealth of detail also makes it difficult for the student to grasp essentials. However, the authors are to be commended for the inclusion of some pertinent detail which is ordinarily omitted from money and banking textbooks, and for their use of money and banking history to vivify the subject. Such details as bank routing symbols, early note and deposit insurance plans, trust functions of banks, the Aldrich-Vreeland Act, proposed monetary panaceas, and details about the Federal Deposit Insurance Corporation and Savings and Loan Associations help to relate the subject to some general experiences and interests of the student.

Some of the weaknesses of the book are: (1) failure to present the table of purchasing power of the dollar on a single base rather than giving the indices from 1914 to 1947 on a 1926 base and those from 1947 to 1956 on a 1947-49 base (pp. 38-39), (2) failure to include the most recent 1952 revisions in describing the method of determining wholesale prices (p. 314), (3) misusing the term fiscal for monetary (p. 114), (4) an incorrect statement that a "balance of payments" can be "favorable" or "unfavorable" (p. 536), and (5) a questionable statement that "the volume of loans to member banks should not exceed the total amount of excess reserves of all member banks" (p. 186).

The best parts are the discussions of secondary reserves, bank loans, bank investments, credit expansion, monetary control, sources of disequilibrium, functions of Federal Reserve banks, branch banking, Treasury influences on the money supply, and miscellaneous financial institutions. Most of the study questions are very good, but some require considerably more information than the student can obtain from the chapter or his experience.

The book is somewhat extensive for a text of a one-semester course, and deletions cannot be made very satisfactorily. However, it lends itself well to supplementary reading for a one-semester course.

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Public Finance; Fiscal Policy

Sales Taxation. By JOHN F. DUE. Urbana: University of Illinois Press, 1957. Pp. ix, 396. $5.75.

Professor Due, long known as a specialist on sales taxation, has now written a highly useful general treatise on the subject. The book includes sections on incidence and other economic and equity implications, experience with sales taxation in a number of countries, and the optimum form of sales tax. A selected list of references to other books and articles is appended to each chapter.

Due accepts, with some qualifications, the opinion that the main burden of the sales tax falls on consumers in proportion to their expenditures on taxed items. He concedes that monetary adjustments will be necessary to permit the tax to force up the price level but feels that these adjustments must be expected because failure to allow them "would be contradictory to the general philosophy of sales taxation, that the tax be a consumer-borne levy" and would cause unemployment (p. 14).

The major part of the book, covering about 300 pages, is a survey of sales taxes in Western Europe, Canada, the United States, Australia, and New Zealand with less detailed descriptions of sales taxes in Asian and Latin American countries. The U.S.S.R. and other Soviet-bloc countries are not covered. The author's first-hand study of sales taxes in North America and Western Europe enables him to comment on informal practices and attitudes of officials and taxpayers as well as to outline provisions of law and regulations.

The principal broad sales taxes studied by Due include: (1) multiple-stage or turnover taxes in Germany, Austria, Luxembourg, the Netherlands, Belgium, and Italy; (2) value-added tax in France; (3) manufacturers' sales taxes in Canada and Finland; (4) wholesale taxes in Switzerland, Australia, New Zealand, and the United Kingdom; (5) retail taxes in Norway, the Canadian provinces and municipalities, and states and municipalities of the United States. These taxes vary greatly in the extent of exemptions and rate differentiation. The Norwegian retail sales tax, for example, is uniform in rate, provides few exemptions, and applies to many services which are usually not reached by American and Canadian retail sales taxes. The British purchase tax, on the other hand, applies only to specified classes of commodities, at highly differentiated rates, and thus resembles a system of excises about as closely as it does a "general" sales tax.

Although the turnover tax is sometimes represented as a simple measure, Due shows that the turnover taxes in actual use in Europe are rather complex, because it has been considered necessary to try to mitigate discriminatory features of these taxes, particularly the incentive given to business integration. Other objections to turnover taxes include the arbitrary differences in taxation attributable solely to differences in the number of "turnovers" characteristic of various goods and complications relating to exports and imports. The appeal of the multiple-stage taxes is that they yield maximum revenue at any given rate, which is thought to reduce political opposition

and incentives for evasion. The value-added tax eliminates multiple taxation while continuing the practice of spreading liability over all (or several) stages of production but in Due's opinion cffers no fundamental advantage over a single-stage tax. Most American students probably will agree with Due that a single-stage tax is preferable to the multiple-stage taxes and that, where a retail tax can be satisfactorily administered, it is superior to a manufacturers' tax or a wholesale tax. An economic advantage of the retail tax is the avoidance of pyramiding; another characteristic—which some consider an advantage and others a disadvantage—is that the amount of a retail tax is evident to consumers (and voters). The main objection to the retail tax is the large number of taxpaying firms, many of which do not maintain adequate accounting records. Although experience shows that this difficulty can be overcome in a favorable environment, it may be decisive where retailing is carried on to a great extent by small family enterprises, especially if many taxpayers are uncooperative or illiterate.

On these topics and many other points, Due's presentation is careful and systematic, giving attention to considerations of economic policy as well as administrative convenience and efficiency, but without preoccupation with either abstruse theoretical points or minutiae of law and administration. The book is a valuable addition to tax literature and should be read carefully by anyone having responsibility for drawing up a new sales tax or revising an old one.

RICHARD GOODE

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The Income-Tax Burden on Stockholders. By DANIEL M. HOLLAND. National Bureau of Economic Research Fiscal Study No. 5. Princeton: Princeton University Press, 1958. Pp. xv, 241. $5.00.

This volume consists mainly of an elaborate and painstaking series of calculations designed to measure "the differential burden on stockholders caused by the existence of two income taxes on corporate earnings—an essentially proportional tax at the corporate level when earned, and a progressive tax at the personal level when received as dividends." The principal contribution of the book consists of its statistical findings since, as the author states, most of the underlying concepts have already been developed in the literature.

The statistical findings are difficult to summarize meaningfully because of the many and varied assumptions on which they rest. The main emphasis of the author, however, is placed on a statistical comparison based on the following assumptions: (1) that the corporate tax rests entirely on profits; (2) that earnings, both distributed and undistributed, should be considered as allocable to the individual taxpayers in the various income brackets who actually own the stock of United States corporations, in proportion to their holdings as measured by dividend receipts, and consequently that the taxes paid by these corporations should also be considered a part of the tax burden of the individual stockholders; and that taxable income is an acceptable measure of income.

On the basis of these assumptions the author provides quantitative estimates in answer to the following two questions: (1) How heavy has been

the extra tax burden on stockholders' pro rata share of net corporate earnings? (2) How heavy has been the extra burden measured in terms of effective rates on all stockholders' income, compared with that for other taxpayers? In other words, a comparison is made between the existing tax structure and the results which hypothetically would have been obtained if all corporations were taxed as partnerships. (The author carefully avoids taking a policy position on either the practicality or desirability of the latter procedure.)

In summary form, some of the most important findings, covering the years 1940-1952, are:

1. Not all stockholders were overtaxed. Despite the double taxation of distributed corporate income (on the author's assumptions) some stockholders were undertaxed. The reason, of course, is that a sizable portion of corporate profits are not distributed and for upper-bracket stockholders the corporate tax rate is lower than the personal rate.

2. Overtaxation was heaviest at the bottom of the income scale and turned into undertaxation at the top of the income range.

3. The progressivity of the corporate tax is uneven and indiscriminate at best. Stockholders with essentially similar taxable incomes were found subject to widely varying effective rates of tax because some had invested heavily and others lightly in corporate stocks. Even for "average" stockholders in each income class, the progressivity effect of the corporate tax was found to be equivocal, depending in part on the definition of progressivity used.

4. Only a slight degree of relief is afforded most stockholders by the dividend exclusion and tax credit incorporated in the Internal Revenue Code of 1954. These reforms, judged in terms of the stated objectives of the Administration of reducing the double taxation of distributed earnings, "operate in somewhat capricious fashion."

For readers who prefer modifications of the basic assumptions listed above the author makes similar calculations based on a wide range of alternative assumptions too varied to report here. In general, he concludes that reasonable variations in his assumptions would alter his findings in magnitude but not in general pattern.

By way of evaluation, little need be said. The analysis is meticulously carried through, well expressed, and, as in any book surviving the National Bureau screening process, all findings are qualified with great circumspection. Qualitatively, the major findings were predictable without detailed statistical analysis so that the main value of the work consists in the added precision which its quantitative estimates give to effects already generally recognized to exist. The only place where the author comes close to expressing a policy judgment is in his discussion of the dividend exclusion and credit introduced by the Eisenhower Administration in 1954. After quoting the stated objectives of this reform from the President's Budget Message for fiscal year 1955, the author proceeds to point out incisively how far both the original proposal of the Administration and the law as finally enacted are in conflict with these objectives, at least in so far as the equity aspects of the question are concerned.

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International Economics

Ocherki ekonomicheskikh otnoshenii SSSR s Kitaem. (Survey of the Economic Relations of the USSR with China.) By M. I. SLADKOVSKII. Moscow: Gosudarstvennoe Izdatel'stvo Ministerstva Vneshnei Torgovli, 1957. Pp. 455. Rbl. 17.80.

This book, by a senior official of the Soviet Ministry of Foreign Trade long concerned with Chinese affairs, is an important addition to the existing literature on the history of Sino-Russian economic relations and particularly to the information available on Soviet assistance to communist China since 1950. Based on Russian, Chinese, and English-language sources, it traces the history of Sino-Russian economic relations from the 17th century to 1955 and includes hitherto unpublished material for the 1939-55 period from the files of the Soviet Ministry of Foreign Trade. The last hundred pages of the book include a historical table on Sino-Russian trade, a chronology of events, a bibliography, and an 80-page collection of the texts of pertinent treaties and agreements between China and Russia dating from 1689. (Unfortunately lacking, however, is a map on which to locate the many place names cited in the book.)

Sladkovskii emphasizes primarily developments in the level and composition of trade and the economic and political factors responsible. Only secondary attention is given to Russian investments in China, and the treatment of such topics as Russian participation in the Chinese Eastern Railway is relatively brief. Much statistical and descriptive material is presented not only for the more remote historical periods but also for the period after the second world war. Thanks to the recent Soviet statistical "thaw," Sladkovskii has been able to include data on Soviet trade with communist China published for the first time in this book; the corresponding Chinese statistics have not yet been released by communist China.

The generally scholarly tone of the book is, however, marred by Sladkovskii's efforts to justify Soviet foreign political and economic policy, through the omission of or cursory discussion of Soviet actions detrimental to China and through attacks on the opponents of Soviet policy toward China. Although he readily criticizes Czarist policies harmful to China which have since been repudiated by the Soviet regime, he ignores or glosses over policies or acts of the Soviet regime after 1917 which were disadvantageous to China, such as Soviet domination of Mongolia and Soviet seizure of Japanese assets in Manchuria as war booty. Sladkovskii likewise fails to discuss Soviet promises to the nationalist Chinese regime which were subsequently broken. Using the conventional Soviet slogans and labels, he denounces the "Kuomintang clique" and "reactionary imperialist and monopolistic circles" in the United States.

The most interesting and useful portion of the book for most Western readers will be the 63-page account of Sino-Soviet economic relations since 1950. Sladkovskii presents annual data for 1950 through 1955 on the value and commodity composition of Sino-Soviet trade and a detailed, although nonstatistical, discussion of the fields and characteristics of Soviet assistance

to China, including an illustrative list of projects built or to be built with Soviet aid. Lacking, however, are details on the schedules of deliveries and repayments under Soviet loans to China and a full treatment of the Sino-Soviet balance of payments. Although detailed trade figures are given (in Soviet foreign-trade rubles, convertible to dollars at the official rate of 4 to 1), no such data are given on invisible items, capital transactions, and unilateral transfers. Sladkovskii confirms that Sino-Soviet trade is conducted at prices which initially are "determined on the basis of world market prices" but which "remain unchanged for a long period of time"; however, he does not reveal precisely how these prices are set nor how frequently they are adjusted.

Sladkovskii's data, together with subsequently published statistics for 1956,[1] show that total Sino-Soviet trade rose from $576 million in 1950 to $1,497 million in 1956. Over this period, Sino-Soviet trade increased from about one-fourth to about one-half of total communist Chinese trade, while it remained about one-fifth of total Soviet trade. Although Sladkovskii does not discuss military aid by the USSR to communist China, some inferences regarding the size and trend of this aid and other strategic exports may be drawn from the behavior of the unspecified residual in the commodity breakdown of Soviet exports to communist China. This residual of unidentified exports accounts for $287 million of the 1950 total of $388 million, rises to $386 million of the 1954 total of $759 million, and then declines to $271 million of the 1956 total of $733 million. Of the specified exports, the dominant category is machinery and equipment, with a share in total Soviet exports to communist China of 11 per cent in 1950 and 42 per cent in 1956. The principal communist Chinese exports to the USSR are agricultural products, nonferrous metals, and textiles.

Only a minor part of Soviet economic assistance to communist China has been provided on a gift basis. With the exception of a small amount of uncompensated technical assistance, the USSR has furnished commodity exports and technical services to communist China either in return for current payment in Chinese goods or on credit. Two Soviet loans to communist China have been announced: a credit of $300 million in 1950 (drawn upon during 1950-54, with repayments scheduled for 1954-63) and a credit of $130 million in 1954 (drawing and repayment terms undisclosed). During 1950-54, while communist China was drawing upon these loans, and before substantial repayments began, communist China had an import surplus on merchandise account with the USSR of $140-$200 million. In the absence of further loans (or grants), the import surplus declined to $99 million in 1955 and disappeared in 1956, when communist China had an export surplus of $31 million in her trade with the USSR.

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[1] M. I. Sladkovskii, "Successes of Soviet-Chinese Trade," *Vneshniaia torgovlya* (Foreign Trade), Oct. 1957, 2-6. This article supplements Sladkovskii's book with data for 1956 as well as minor revisions of its statistics for 1950, 1952, and 1953.

The main objective of this book is to investigate from the theoretical point
of view the export of capital to the underdeveloped countries. Pointing out
that no unified theory of capital movements exists, since none could "cope
with the actual problems of the postwar period full of economic anomalies"
(p. 2), the author is quite optimistic that such a theory can, should, and
will be formulated. He even foretells likely theoretical developments such as
re-emphasis of long-range considerations in a true "rediscovery of classical
economics" (p. 13). On the other hand, the author does not limit himself
solely to theoretical analysis and he considers the various practical prob-
lems connected with international investment as well.

In a sense, this study represents a prodigious effort to summarize much
of the earlier literature and to familiarize the reader with current thought
on the subject. Guth succeeds in his endeavor since he is well versed in the
literature, particularly that published in the United States, the United King-
dom, and Germany. The volume opens with a very useful introduction. We
are told that the term "export of capital," as it appears in the book, denotes
only the transfer of purchasing power from the lending to the borrowing
underdeveloped country, because the author is little concerned with the
actual form of the transfer. The central problem is stated on page 7 as fol-
lows: To what extent does the export of capital to underdeveloped countries
serve in the long run the mutual interests of the countries providing and re-
ceiving developmental capital? The author devotes eight chapters to finding an
appropriate answer. Of these chapters, the first four deal essentially with the
capital-exporting countries.

Pointing out quite correctly that there was practically no international
capital market after the great depression, Guth identifies the decline of pri-
vate foreign investment as the most pronounced recent tendency in interna-
tional investment patterns. Writing in the true liberal tradition, he regrets
this tendency as being quite detrimental to the harmony of interests of all
countries concerned. At a later point (p. 152) he adds the rather pessimistic
note that owing to the inability of the capital-exporting countries "to mobi-
lize private capital for the purpose of financing several programs of basic de-
velopment in the underdeveloped countries . . . many wishes of the latter
will remain unfulfilled."

Of the remaining chapters, which are chiefly concerned with capital-import-
ing countries, Chapter 5 ("The Role of Foreign Investment in Economic
Development") and 6 ("Uses of Foreign Capital") cover much ground
which is commonly found in textbooks on economic development of under-
developed countries. Two statements, however, deserve special mention be-
cause they definitely reflect the author's own viewpoints: (a) foreign invest-
ment should serve only as a spark-plug in developmental programs and
should in no case replace domestic capital formation (p. 84); and (b) capi-
tal imports should be geared to maintaining a certain "minimum velocity of
development" which is necessary to propel and maintain the cumulative

process of development (pp. 88-89). Chapter 7, "The Long-Range Goals of Economic Development," surveys the controversy centering around the issue: Should economic development in underdeveloped countries result in improvements of production of raw materials or should industrialization be the chief goal? The reviewer does not find the discussion provocative at all, inasmuch as Guth restricts himself to complete objectivity while presenting the well-known arguments of Wilhelm Röpke on the one hand and of Raúl Prebisch and H. W. Singer on the other. The final chapter scrutinizes the determinants of the rate of growth and outlines the functions of capital imports in the short and long run: expansion of production, financing of a current-account deficit in the short run, establishing an equilibrium in the balance-of-payments in the long run, and increasing productivity and thereby improving the standard of living. Here the author is on firm ground and this chapter is by far the most interesting part of the book. It seems to the reviewer, though, that Guth could have devoted more space to the discussion of productivity and particularly to the argument that the productivity of labor in the developing countries has been rising at a much faster rate than is generally recognized, as Donald MacDougall and others have pointed out. The addition of supporting figures would have been desirable. In fact, no statistical table appears here or elsewhere in the text. These comments should not be construed, however, as criticism of this well-written chapter.

In a 12-page concluding summary the question posed in the introduction is now answered—in the affirmative: The contribution of capital exports is positive. They are likely to lessen, in the long run, the discrepancies in the standards of living between the developed and developing countries, primarily because they permit a better utilization of the factors of production. But much hinges on the world political situation and the general feelings of mutual trust and security. The author warns the reader, however, that the role of capital exports should not be overstated and he reiterates again his conviction that "the more participation there is of creative entrepreneurs and the less of the anonymous government in capital-exporting, the sooner it will be possible to improve, on the basis of personal relationships, the spirit of cooperation between nations" (p. 154).

Guth has performed a useful service in presenting an interesting and succinct survey of the literature and in bringing together theory and practice. The book is unquestionably a useful addition to the literature on the economic development of underdeveloped countries.   •

                                              STEPHEN SPIEGELGLAS
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The Theory and Procedure of Exchange Control in Germany: A Study of Monopolistic Exploitation in International Markets. By FRANK C. CHILD. International Scholars Forum: 10. The Hague: Martinus Nijhoff, 1958. Pp. viii, 241. 19 Guilders.

This expanded version of a doctoral dissertation is an interesting experiment in applying the tools of modern welfare economics and national income analysis to the already well-explored terrain of the German exchange control

experience of the nineteen-thirties. Despite the formidable analytical and empirical obstacles involved, the attempt is, on the whole, a successful one. The work begins with a concise historical description of German exchange controls before the advent of the Nazis, which is quite useful in bringing out the very thin line of demarcation which exists between defensive and aggressive controls. This is followed by the theoretical core of the book in which the author develops an analytical framework for treating the "gains from trade" in the context of marginal monopoly-monopsony theory. Although this approach suffers from the familiar criticism of cardinality as applied to consumers' surplus, it does offer a rough substitute for the standard indifference curve approach, which has the distinct advantage of being an attractive pedagogical device and of enabling one to handle empirical data in monetary form. The remainder of the book consists primarily of a qualitative application of this analytical framework to the actual German experience, with the final chapter also including a brave attempt by Child at quantification.

In spite of the shortcomings inherent in both the analytical tools and the data (which, by the way, are treated scrupulously and imaginatively by the author), this reviewer found that an extremely persuasive case was made for the point of view that the benefits which Germany itself received from her exchange control system were far more substantial than had been indicated in Ellis' classical study of *Exchange Control in Central Europe*. What is even more challenging is Child's contention that "contrary to opinion at the time, German trade redounded with some net benefits to the exploited nations" (p. 3). Ironically enough, the major reason for this conclusion is not based on the elaborate analytical welfare apparatus constructed by the author, but stems instead very simply from the increases in national income levels in the various countries subjected to Germany's trading practices. These practices artificially expanded German imports in a way made possible, for the exporting countries, by the excess capacity characteristic of the depressed economic conditions of the 'thirties. This is a finding which, in the light of recent developments in the world economy, has far more than mere historical interest; the current Soviet trade offensive has found an excellent breeding ground in similar conditions of excess capacity and low prices in primary producing countries.

The major thesis of the book, in the author's own words, is that "exchange control is a useful and flexible instrument of commercial policy and that appropriate regulations will, for any one nation increase or even maximize the gains from external trade" (p. 2). On the lofty plane of pure theory this conclusion is undoubtedly a valid one and could even be considered tautological. But any attempt to extend it to the more mundane level of practical policy must inevitably involve one in the obvious pitfalls of overgeneralizing on the basis of the unique factors in the German experience (e.g., international political power and bargaining strength, relatively efficient bureaucratic administration, and the effectiveness of a totalitarian regime in preventing conflicting domestic political pressures from pushing the degree of control well beyond the optimum). Child's discussion, it is true, does cover each of these factors, but his interest in them is restricted to the narrow framework of technical

welfare economics (e.g., the costs of compliance as an offset to the gain from trade, the assuming away of the welfare aspects of a change in the domestic income distribution). The chances of exchange control regulations meeting his criteria of "appropriateness" and "flexibility" would thus appear to be considerably smaller than in the Germany of the 'thirties for virtually every other nation in the world. Nevertheless, Child's book does make a useful contribution to the growing literature on actual exchange control experience and also constitutes a stimulating exercise in applied theory.

EUGENE R. SCHLESINGER

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The Bank for International Settlements. By HENRY H. SCHLOSS. Amsterdam: North-Holland Publishing Co., 1958. Pp. xi, 184. $4.00.

The bulk of this book is taken up by a review of the origins of the Bank for International Settlements; its structure and mechanical functions; its short-lived operations in facilitating the payment of reparations under the Young Plan; its modest contributions toward mitigating the financial crises of the early 'thirties; and the embarrassing position of the Bank during the second world war when, attempting to carry on its routine duties in a divided Europe, it incurred charges of collaboration with Germany. The slightness of the subject matter, however, prevents the author from adding anything by way of information or interpretation which is not already contained in Auboin's compact monograph on the BIS.

The subtitle, "An Experiment in Central Bank Cooperation," indicates what is intended to be the main focus of this study. Cooperation as carried on under the wing of the BIS has two forms: (a) short-term operations between European central banks and (b) meetings, consultations, and exchanges of information among officials of the central banks. Since all aspects of these activities are apparently considered confidential, it is impossible to learn what, if any, economic significance they may have. To go beyond what is presented in this well-written book, we will doubtless have to wait for some member of the inner circle to publish his "Confessions of a Central-Bank Cooperator."

GERALD SIRKIN

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International Economics. By THOMAS C. SCHELLING. Boston: Allyn and Bacon, 1958. Pp. xvi, 532. $6.75.

The primary objective of all undergraduate textbooks in international economics has normally been an understanding of policy issues. Text discussion of international economic policy may be likened to a balloon, steadily expanding as helium is pumped in by the pressure of world events and by the growing involvement of the United States in the world economy.

Traditionally the balloon has been secured—moored to the foundations of knowledge—by two strings of varying strength: history and theory. In this reviewer's opinion, the best textbooks have been moored by a single thick rope: in the cases of Condliffe and Ellsworth II the rope of history; in the

cases of Ellsworth I, Haberler, Harrod, Kindleberger, and Marsh the rope of theory. No text has really been ideal, secured firmly by *both* ropes. Professor Schelling's new book is moored by a thread, of theory. In the balloon is the most interesting, the most extensive (the last section labeled policy consumes almost exactly half the book and several earlier chapters are heavily policy-oriented) and probably the best discussion of current policy issues available in text form. But it is knowledge that is likely to float away, if not during a reading of the book certainly very soon afterwards as the world economy changes, and policy issues change along with it. The story of the little red balloon, which follows the small French boy wherever he goes, although it is not grounded to anything, is, after all, a fable.

Schelling states that "The policy orientation of this book is an attempt to broaden the subject of international economics." The subject is broadened, in that considerably more attention than usual is given foreign assistance programs, cost-sharing problems in intergovernmental arrangements, and strategic trade controls and economic warfare. A keen mind as well as extensive experience in Washington are trained in interesting fashion on these vital current problems, with appropriate stress on the political. The style in these chapters and throughout is choppy (with an average of two subheads a page), "discussion-oriented," and may well give the student a feeling that he is engaging in razor-sharp repartee with the author, Schelling usually being one step ahead of him.

But in other ways, in this reviewer's opinion, Schelling's policy-orientation *narrows* rather than broadens the scope of international economics. For one thing, there is lacking a feeling for the broad sweep of history, while at the same time there is little use made of general equilibrium international trade theory and little regard for long-run considerations in the analysis of current problems. For another, and here most modern texts are equally at fault, there is no attention paid to the history of the subject; nor, except for the use of tables, is the student instructed in the proper use of evidence in support of analysis.

We are introduced at the start to money transactions and the balance of payments. Trade is conducted in money terms, and therefore we must analyze it in money terms, even if when we get to "underlying economic conditions" in Part II this results in an awkward, truncated discussion of the real basis for trade. The doctrine of comparative costs follows from differences in money prices—through a confusing series of approximations—and implies differences in factor endowments. Almost as an afterthought the student is given a glimpse of the real distinguishing feature of international trade, i.e., factor immobility across national boundaries, and then this feature is given a role distinctly subsidiary to the *financial* separateness of countries and governments. The rich body of literature which concerns possibilities of interchange of factors and goods, of differing technologies and the combination of factors into goods—in short of general equilibrium trade theory—is lost in the shuffle. Similarly, the third section devoted to the theory of balance-of-payments adjustment not only neglects shifts in relative prices and handles in-

come effects entirely in the form of net shifts in exports minus imports, but also is developed almost solely in terms of one country. As a result, any concept of an equilibrium system of interacting national economies (which even in terms of income effects alone would come through to the student if a reflection ratio had been incorporated in Schelling's multiplier formulas) is again lacking.

These weaknesses in theory show up, most clearly in such policy discussions as that concerning economic integration. Foreign assistance and economic warfare are clearly topics that are not amenable to precise, or even loose theoretical formulation (except possibly at an advanced level in terms of game theory). But surely issues with respect to European integration involve almost all of traditional trade and payments theory; as Meade, Scitovsky, and others have shown, applications are possible in almost every facet of the problem. But because the theoretical groundwork has not been adequately laid, little of this can be developed in Schelling's chapter, in which there is no attempt even to draw the basic distinction between customs union and common market, deriving from absence or existence of freedom of movement for factors of production.

Other teachers may find that they are sympathetic with Schelling's approach and disagree strongly with this reviewer. Certainly parts of the book —especially the last six chapters on foreign assistance, cost-sharing problems, and economic warfare—will prove interesting to any reader and useful as supplementary reading in all courses (graduate or undergraduate) dealing with international economic problems.

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Business Finance; Investment and Security Markets; Insurance

The American Stockholder. By J. A. LIVINGSTON. Philadelphia: J. B. Lippincott Co., 1958. Pp. 290. $4.95.

The object of this volume is to appraise the status of the stockholder in our society. Following a brief introduction, there is a compact and well-organized presentation of the facts concerning stockholders and share ownership. The three positive rights of the stockholder are then considered in successive chapters. They are (1) the right to discharge the officers and directors —the management—by electing new directors; (2) the right to sue officers and directors for misuse of powers, gross mismanagement, fraud, or dishonesty; and (3) the right to sell his stock. Two chapters on management-stockholder relationships are among the best in the book. Other subjects discussed include the New York Central proxy fight (The Battle for the Century), the Ford stock sale, the role of institutional investors, the New York Stock Exchange and its operations (The Stock Exchange in Sir Galahad's Clothing), and the regulatory activities of governmental agencies. The final chapter is concerned with executive compensation, the tax-sheltered elite, and the "moral obligation" of institutional investors.

In his effort to place share ownership in proper perspective, Livingston endeavors to correct certain misconceptions. In a straightforward presentation "people's capitalism" and similar conceptions are criticized on the ground that they are idealized and overdrawn versions. Most readers will agree with this appraisal. The diffusion of share ownership is appreciably less than the term "people's capitalism" implies. To the sophisticated observer this and similar slogans involve an excessive resort to hyperbole. "Corporate democracy" is also a misleading phrase, though it is extremely useful to "professional" stockholders—Lewis D. Gilbert, Wilma Soss, et al. "Plutocracy" is the correct term, according to the author, as long as voting is on the basis of the number of shares owned.

The author concludes that only one positive right—the right to sell stock —can be exercised effectively by all classes of stockholders. In practice only substantial professional investors,—including banks, investment companies, insurance companies, and well-to-do individuals, have the power to remove officers and directors. In the absence of a nucleus of some kind, it is virtually impossible for the scattered holders of a majority of the stock of a large corporation to exercise a degree of control. Nor is the right to sue meaningful for most stockholders. "Lawsuits, like proxy fights, are primarily the large stockholder's game. Or the lawyer's game." The thesis is advanced that the large institutional investors should play a more active role. Effective appraisal of management can come only from this source. Institutional investors should examine salary, pension, deferred compensation, and stock option plans critically. They are in position to enhance the moral tone of business and finance.

There have been notable improvements in the position of the stockholder and in the operation of the securities markets over the last quarter of a century. They are attributable mainly to the efforts of the Securities and Exchange Commission and other regulatory bodies, according to Livingston. Perhaps no informed person would disagree. Nevertheless, in the absence of governmental controls and regulations instituted since 1930, the status of the stockholder would doubtless be much better than in the 1920's. Some reforms in the securities markets and more enlightened corporate practices would have been essential to restore a measure of investor confidence, if no external controls had been imposed. It would of course be idle to speculate on the precise nature of the changes that might have occurred if important institutional innovations had not been introduced.

This volume is intended mainly for a business or lay audience. The non-specialist will find a great deal of information that will enhance his understanding. The specialist in finance will enjoy this book, though he may find little that is new. Its nearest counterpart in the literature is Ripley's *Main Street and Wall Street*, published in 1927 when confidence in financial and business leadership was at a predepression peak. In some respects this work complements Ripley's famous volume very effectively. It differs markedly in both content and style from *The Modern Corporation and Private Property* by Berle and Means and Brandeis' *Other People's Money*.

The American Stockholder is unusually well written, at times with a tinge

of iconoclasm. The bulk of the contents is descriptive rather than analytical. The points the author wishes to make stand out with the requisite clarity. When adverse comments seem called for, the criticisms are direct and never exceed the bounds of good taste. To borrow from Ripley, Livingston is never "promiscuously accusative." He is at his best when one senses a carefully restrained indignation.

LEWIS H. KIMMEL

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The Over-the-Counter Securities Markets. By IRWIN FRIEND, G. WRIGHT
    HOFFMAN, WILLIS J. WINN and others. New York: McGraw-Hill, 1958.
    Pp. vii, 485. $12.50.

The first point that must be made in reviewing the encyclopedic *Over-the-Counter Securities Markets* is that it is the only complete survey of this tremendous field now available. As such it must receive acknowledgment as a scholarly, definitive work on the subject. It provides the general and professional reader with a description and, what is more important, a measurement of the varied market sectors which make up the over-the-counter security market.

The difficulty of comprehensively covering a market in which 40-50,000 securities are traded can not be underestimated. The Securities Research Unit of the Wharton School, which undertook this massive task, has accomplished a superior result in measuring and analyzing the over-the-counter market from every aspect imaginable. Through an intensive statistical survey, in 1949 with a follow-up in 1951-52, and with the active cooperation of the securities business itself the activities of the unorganized security markets have been completely measured. The book covers the varied activities of both the securities dealers and the securities buyers in all over-the-counter markets running from government debt to stocks of small corporations. In sampling the market, the authors have weighted the relative importance of the participants in the market running from huge metropolitan banks acting as underwriters in the municipal bond field to the one-man investment firm that handles only a few securities at one time. The difficulties of classifying and describing all these transactions is wryly admitted by one of the authors who writes that "no two transactions are exactly alike in all respects." Certainly one impression that one gains from this study is that the unorganized security markets are markets of negotiation and that a firm's success depends ultimately on the shrewdness and risk-taking ability of the relatively small number of professionals engaged in initiating and maintaining markets. Perhaps the most interesting point developed concerns the typical spread charges on unlisted common stocks. It has been the stated policy of the National Association of Security Dealers to limit the profit of the dealer to 5 per cent on trades for a customer. The average customer cost on the New York Stock Exchange for a sample of transactions in 1953 was 3.3 per cent as opposed to 4.6 per cent on the over-the-counter market. Only in transactions involving more than $10,000 did the over-the-counter market match the cost figures of the Exchange and on

transactions up to $2,500 the customer cost was significantly above 5 per cent.

Another significant factor brought out by this study is the increasing importance of the over-the-counter market for handling large blocks of stocks. As institutions have become increasingly important in the market for securities, the organized exchanges have been unable to handle large blocks of securities without undue influence on the market price. The evidence is clear that large transactions made over the counter involve less time for completion and fewer vagaries in price movements. It is the opinion of the Securities Research Unit that measures taken by exchanges to counteract the loss of business to the unorganized market have been for the most part stop-gap and generally ineffective. Considering that the securities markets are becoming thinner and that block trading is becoming more important due to the development of pension trusts, investment companies, and the like, the over-the-counter market is increasingly important in maintaining competitive capital markets.

A few exceptions, however, may be taken to this otherwise interesting and excellent book. This reader felt that there was an excessive number of tables some of which were not fully integrated with the text. Another fault for some readers may be the lack of actual examples taken from the day-by-day action of the market. More case materials describing typical transactions would have been helpful to the average reader.

R. R. DINCE

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Industrial Organization; Government and Business; Industry Studies

The Sources of Invention. By JOHN JEWKES, DAVID SAWERS and RICHARD STILLERMAN. London: Macmillan; New York: St. Martin's Press, 1958. Pp. xii, 428. $6.75.

Professor John Jewkes of Oxford, and two research assistants, have undertaken to analyze the sources of invention in the nineteenth and twentieth centuries. Before proceeding, they introduce two caveats: (1) it is very difficult to apply economic analysis to the innovative process; (2) the road to understanding is blocked by the growing complexity of science and technology.

The authors obtained their material from an extensive review of the literature on invention—much of it noneconomic—confining the study primarily to Great Britain and the United States and supplementing their readings by interviews with "scientists, technologists, patent lawyers, historians, inventors, administrators and businessmen." The persons interviewed are not named or quoted because many of them "would prefer anonymity" or "would probably not agree with the conclusions."

Fifty case histories, including those of the ball-point pen, the cotton picker, the jet engine, penicillin, radio and the zip fastener, are described in part II (150 pages). The cases are labeled as summaries. They discuss briefly the

history of each invention but rarely include financial figures. Nonetheless, many of these condensations should be useful as class reading for courses concerned with innovation.

The major portion of the book consists of a series of essays on invention, ending with a chapter entitled "Conclusions and Speculations." There is no attempt, even in the discussion of "modern views on invention," to relate the material to a conceptual scheme of economic development. The absence of a theoretical structure is frankly stated in the beginning, when Jewkes says that "it may yet prove impossible" (p. 3) to apply economic analysis to industrial innovation. Although Schumpeter tried to do so, his efforts are not mentioned.

One of Jewkes's most valuable chapters is "Inventors and Invention in the Nineteenth Century." He appears most at home with this material and writes about it confidently. On the other hand, in the chapters on corporation research and development in the twentieth century the tone shifts. The authors recognize many of the great contributions of corporate research. They also analyze some of the reasons for the increased cost of modern development, including the desire to reduce competitive risk by speeding up the time schedule of development. But the conclusions are pessimistic: "If, through high and progressive taxation, large fortunes tend to disappear, then one of the props of the individual inventor will have disappeared, and it appears doubtful whether it can be adequately replaced by public or private financial institutions designed for the same purpose" (p. 260).

There are no specific references to the strength and limitation of the venture-capital corporation, although Jewkes is much impressed by the ingenuity of the United States as compared to that of Great Britain in experimenting with new types of institutions. Nor is there adequate treatment of modern firms whose leaders have a passionate interest in encouraging creative research. Jewkes explains that there are some firms which try—difficult though it may be—to provide freedom for at least a few men of promise. Yet he concludes that "the industrial laboratory does not appear to be a particularly favorable environment for inducing invention" (p. 132).

> "It is opportune to ask whether Western societies are fully conscious of what they have been doing to themselves recently; . . . whether there is truth in the idea that, although the institution is a powerful force in accumulating, preserving, discriminating, rejecting, it will normally be weak in its power to originate and will, therefore, carry within itself the seeds of stagnation. . . . (p. 243) If present trends continue for any length of time, it is not improbable that . . . emphasis will be laid on the need for, indeed, the duty of, research workers to submit themselves to team work" (p. 245).

The real hope on this score would appear to lie in a countervailing trend in the United States which Jewkes does not mention—a trend toward greater support of the individual. This trend is manifest in grants from research foundations and in liberal financial help for individuals to start new corporations with a research conception. Although the economic impact of these

forces is yet to be assessed, it is heartening that they exist. For Jewkes makes
a most convincing case for the great significance to the inventive process of
providing for autonomous research as well as for team work.

W. RUPERT MACLAURIN

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Anti-Trust Laws: A Comparative Symposium. University of Toronto, Faculty
    of Law, Comparative Law Series, Vol. 3. Edited by W. FRIEDMANN.
    (Toronto: Carswell Co. Ltd., 1956. Pp. vi, 635. $12.00.

The occasion for this volume is the fact that specific antimonopoly legisla-
tion, formerly almost exclusively confined to the United States and Canada,
has been adopted since the second world war in a substantial number of
countries which had previously relied upon the courts to deal with undesirable
restrictive trade practices within the framework of the common law or of civil
codes. The volume contains essays dealing with the antimonopoly laws of
Canada, France, Germany, Japan, the Netherlands, Norway, Sweden, the
United Kingdom, and the United States; an essay on international cartels and
combines; and a comparative analysis of antimonopoly laws by the editor.
There are also reproduced the texts of the South African Regulation of
Monopolistic Conditions Act of 1955, the United Kingdom Restrictive Trade
Practices Bill of 1956, the Report of the Ad Eoc Committee on Restrictive
Business Practices to the United Nations Economic and Social Council, and
the section on Agreements and Concentrations of the Treaty establishing the
European Coal and Steel Community.

With two exceptions the essays were written by lawyers. One of the two
essays on Canadian legislation was written by economists and the essay on
the United Kingdom legislation was written jointly by a lawyer and an econ-
omist. The essays give a full account of the history, provisions, and interpreta-
tion of the various antimonopoly laws along with a brief evaluation in each
case, but except for the essay by Bladen and Stykolt dealing with the Ca-
nadian legislation there is no attempt at criticism based upon economic
analysis. As the editor points out in the preface, "This is, essentially, a law-
yer's book. But in no other branch of law are the links between law and
economics so close as in the field of restrictive trade practices" (p. v). Hence
the present volume admirably complements that edited by E. H. Chamberlin
under the title *Monopoly and Competition and Their Regulation,* a symposium
participated in by economists from various countries under the sponsorship of
the International Economic Association.

Two essays in the present volume which, in the reviewer's opinion, are
particularly useful are those dealing with the efforts of the military occupation
governments to bring about deconcentration and decartelization of industry
in Japan and in Germany. The treatment of the German program is especially
extensive, consisting of an essay by two German lawyers and another by an
American lawyer connected with the program. There is also reproduced the
text of the Draft Law Against Restraints of Competition, introduced in the
West German Parliament in 1952 and described by the editor as the "first

legislative document issuing from a country outside the North American continent that incorporates a radical anti-trust philosophy" (p. 526).

It is noteworthy that the Japanese antitrust law of 1947, which in its original form is said to have been more severe than the corresponding American legislation, has been materially weakened by subsequent amendments, and the German law, even with weakening amendments, had not been enacted as of 1956. The editor concludes that "There is much evidence that both Germany and Japan are reverting increasingly to pre-allied patterns of organization and economic thinking" (p. 557). Likewise, although the other countries whose policies are analyzed in these essays have recognized through their postwar legislation the desirability of a more generally competitive regime they have not seen fit to adopt the American and Canadian position that restriction of competition is undesirable in principle. Rather they have continued to distinguish between "good" and "bad" restrictions on competition and to regulate competition through such devices as publicity, registration of agreements, investigation, and administrative courts. In the reviewer's opinion the present volume serves a very useful purpose in facilitating study of the merits and limitations of these policies by those concerned with monopoly problems and government-business relations generally.

                                                    ROBERT W. HARBESON
University of Illinois

Soviet Transportation Policy. By HOLLAND HUNTER. Cambridge: Harvard
    Univ. Press, 1957. Pp. xxiii, 416. $8.50.

This study, No. 28 of the Russian Research Center series at Harvard, deals with Soviet policies toward transportation as an arm of industrial development during the 1928-55 period, in the belief, as the author states, that these policies "provide significant lessons for many other countries now on the eve of rapid industrial growth." In this, as in other Russian studies, many statistical problems presented themselves, but the author's expectation that Soviet transport data were more available than had originally been thought seems to have been borne out, and an appendix of 100 pages offers a considerable stockpile of scattered transport data to which the reader can readily refer in checking textual statements. Are these reliable? The author believes that these data and other similar Russian statistics, while often carelessly or deliberately misused by Soviet writers, are not invented, and have been largely confirmed by the 1956 Soviet statistical handbook.

Part I reviews the geographical and historical factors which have shaped the Russian transport system. Soviet policy, seeking to use transport to develop the nation's resources, has had three principal locational objectives: (1) removal of unevenness of regional development, considered by Marxist writers to be characteristic of capitalistic economies; (2) advancement of backward regions; and (3) avoidance of capitalistic encirclement. However, economic factors and political objectives have in many cases conflicted with these original principles. The Soviets have repeatedly trumpeted programs to bring forward the underdeveloped eastern portions of the country, since European

Russia held three-quarters of all industry, but no fundamental shift of the economy to the east has occurred. Blocking this shift were the climatic and topographic deficiencies of the eastern regions, and the desire of the government to industrialize as rapidly as possible, which led the Soviets expediently to introduce projects in the west where greater economies of large-scale production could be achieved. The Nazi invasion produced a transfer of activity toward the east, and reconstruction has not completely restored the balance, though it has come close.

Considerations of transport economics have also impaired the attainment of political objectives. The Soviets have followed a policy of minimizing transport costs by locating plants near sources of raw materials and by intensive development of selected routes, usually those already carrying heavy traffic. These criteria have favored the development of industry and rail routes in the west.

A major part of the book is devoted to a discussion of the performance of Soviet transportation, chiefly railroads which carry 84 per cent of freight traffic and 90 per cent of intercity passenger travel. The Soviets relied upon underutilized capacity in the rail system to take care of traffic increases during the 1920's, but the lag in investment in new line and equipment led to a stagnation of rail transport in the 1931-34 period, a situation overcome only by an energetic program of capital improvements and training of rail labor. In the second world war the Soviet transport system belied the gloomy predictions of many analysts, and stood up well. The reasons for this performance include not only the patriotism of personnel and the effectiveness of centralized controls, to which the Russians give credit, but the fact that the Nazi invasion and occupation reduced civilian demands on the carriers more than they did rail capacity (rolling stock was evacuated to the east), thus affording a margin for handling military traffic.

Soviet railroads recovered prewar levels of traffic in 1948 and since that time have accommodated substantial increments in demand largely through the intensive use of equipment and the concentration of traffic on main routes which have been rebuilt. In 1955 Russian freight traffic soared 118 per cent above the 1948 level, passenger traffic was 88 per cent higher, yet road operated increased only 5 per cent, locomotives 9 per cent, and freight cars 34 per cent. The author terms this a "remarkable achievement," considering that the railroads had strained to handle the unexpectedly large flow of traffic in 1948.

A comparison of Soviet and American rail operations reveals the "intensive" use of the Russian system. The Russians work their equipment harder, securing a more rapid loading and unloading of cars by shippers and a shorter turnaround time. Though freight trains are lighter and slower in Russia, they run more frequently, and are concentrated on main lines, so that traffic density, measured in ton-miles per mile of road, averages 2.8 times as high in Russia as in America. This comparison reflects the size of the American rail plant (220,000 miles as against 75,000 miles in the U.S.S.R.), and the greater relative shortage of capital in relation to labor in Russia.

At the same time Russian railroads have lagged in quality of rolling stock,

roadway and signaling equipment, and these items must be improved if the Soviets are to increase rail freight traffic by 174 per cent as programmed in the sixth five-year plan. This direction of transport effort differs markedly from that of the United States where the proportion of intercity freight carried by the railroads has declined in recent years from 75 to 50 per cent, and the roads face financial crisis. Hunter's analysis strongly suggests that American railroads ought to adjust plant more to traffic demand, eliminating weak traffic lines and concentrating service on routes of greatest operating efficiency, under a general plan of consolidation.

Only one-sixth of freight is not carried by rail: waterways have 12 per cent, pipelines 1 per cent, and trucks 3 per cent. Why have trucks not figured more prominently in the Russian transport scene? Because, says the author, the experience of other countries reveals that trucking came about as a by-product of extensive investment in highways made to accommodate private passenger cars. Lacking any similar pull from consumers in Russia, the Soviets preferred to hold highway investment down and to emphasize the rail system, especially for freight. In the next few years, however, the author expects the Soviets to make greater use of trucks and buses on highways, as well as of pipelines to service new oil fields in the interior. Possibly, too, the patient Russian traveler will find better passenger equipment and faster train schedules in the new five-year plan.

In a planned economy with total control over economic organization, a high degree of coordination of the various forms of transportation could be anticipated. However, the author finds the Russian experience does not bear this out. He observes that the several carriers compete for freight, that joint shipments by rail and water are few, and that rail officials in the U.S.S.R. complain that too much short-haul traffic falls on the rails rather than on trucks. This evidence leads the author to conclude (p. 159) that:

> . . . the resource allocation problems of a modern industrial economy cut across ideological boundaries. There is no evidence that the Soviet transportation system is any more "unified" than the American transportation system. In a purely technical sense, the reverse appears more accurate. In each economy, difficult technical problems are confronted, and political slogans do not aid appreciably in solving them.

In a summary evaluation the author states that the policy of devoting capital to industry while holding back new investment in the rail system has been generally successful, with the exception of the short-lived crisis of the early 1930's, and that the transport sector will not retard industrial expansion in the next decade. He gives these reasons: the availability of ample resources for capital formation, expected increases in productivity of rail labor and capital, and a sufficient (though declining) share of transport investment in the Soviet total.

This is a well-documented and impressive study. The author has carefully culled relevant transport data from the Soviet statistical woodpile and interpreted them carefully yet meaningfully. Gaps occur, particularly as to intercity travel, where data on bus and airline traffic are completely lacking.

The reviewer could detect only a few minor flaws in interpretation of rail statistics, such as the failure to note that larger freight cars helped the Soviets handle a volume of freight in 1955 three times the prewar average, though the number of cars increased only 50 per cent. In comparing average train weights in the United•States and Russia, the fact is overlooked that much lightweight traffic in America has been diverted to trucks, leaving heavy bulk items for rail haul, while Russian trains carry all classes of freight.

A more serious question may be raised with regard to Hunter's views of Soviet planning. He recognizes that political slogans, directed often towards uneconomic objectives, have the function of dramatizing a plan and rallying popular support for carying it out, if not *in toto*, at least with respect to the portions which prove feasible. However, he expresses surprise that five-year plans are not hard-and-fast blueprints, that projects are delayed, unlisted projects sometimes started, and goals under- or overrealized. That Soviet planners and ministers are pragmatic rather than prescient should surprise no one; were they not, the economy would founder. Nor do year-by-year errors in estimating the volume of traffic rule out the value of planning transportation investment and performance over the longer period. Despite much side-tracking and back-tracking on transportation plans, it is not accurate to state that the Russian transport accomplishments were "completely unplanned" (p. 117). Hunter's own analysis indicates that rapid industrialization has occurred with minimum capital inputs for transport, that the effort to keep traffic from rising too rapidly as new industries were located has succeeded, and that the confidence that intensified use of the rail plant could cope with traffic increases has been justified. While Soviet ideology, ritualism and bureaucracy have impeded rational analysis and execution of economic projects, neither the "remarkable achievement" of the Soviet railroads nor the failure to develop industry and a rail net in eastern Russia before adequate capital was available for the purpose can be deemed poor planning.

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Government and Business: A Study in Economic Evolution. By HOWARD R. SMITH. New York: Ronald Press, 1958. Pp. viii, 802. $7.50.

This book is not directly competitive with Mund's *Government and Business*, Dimock's *Business and Government*, or Wilcox's *Public Policies Toward Business*. Although much of the subject matter overlaps that of these texts, Smith's *Government and Business* emphasizes the evolutionary aspect. In the words of the author: "This . . . is the record—an account of the step-by-step process through which government acquired the multitudinous responsibilities it now performs in connection with economic activities in the United States" (p. 687). Essentially it is a history of the mutual interaction of economic, political, and social change with legislation of an economic character, with individuals as well as institutions playing their parts.   •

The story begins with the forces behind the Revolution and ends with the economic backdrop of the 1956 election. Between 40 pages of introduction and

"An Overview" of 80 pages are 70 pages of "Before Laissez Faire" (from the constitutional beginnings to the Civil War), 80 pages of "Laissez Faire Resplendent" (from the Civil War to the turn of the century), 175 pages of "Laissez Faire Threatened" (from the turn of the century through the 1920's), 170 pages of "Laissez Faire Replaced" (The great depression and the second world war), and 110 pages of "A Mixed Economy" (the postwar period). Many of the chapter titles are meaningless out of context, e.g., "Behind the Mask" and "End of the Honeymoon."

A conflict context is presented. The public interest is identified with the compromising and tempering of stands reflecting the democratic process. ". . . *the public interest in a democracy is not wanted because it is the public interest, it is rather the public interest because it is wanted*" (p. 20). Only the majority can know what is "right." Not even an economist with his tools of economic analysis can say what is "right." Although there may be inadequate direct consumer representation in the legislative process, Smith argues that the people are a silent partner in all government decisions. Thus there is no need to worry over activities of producer interests. In fact, "If the groups which do form are producer, well and good . . ." (p. 13).

Perhaps because of this thesis, microeconomic analysis, such as pertains to economies of scale and industrial structure, is largely absent. Macroeconomic analysis is more relevant, and Keynesian economics and "economic maturity" in relation to New Deal policy are thoroughly discussed in the chapter on "Recession and Stagnation."

Smith's chronological approach precludes treatment on a completely separate basis of each of the various problem areas, such as antitrust policy or the regulation of public utilities. The several most important economic problems and legislative acts of a particular time period are handled together within the circumstances of those times. The range of government-business relations covered is all-encompassing. Hardly a piece of federal legislation of any importance with respect to economic activity passed in more than a century and a half is not mentioned. And the reasons why some plans or proposals were not adopted are given. As a result some acts are only summarized in the briefest fashion, and descriptive material has to be highly selective.

Two of his findings are: Economic and social reform in America has progressed in distinct cycles, with each liberal period exceeding in intensity the one before. And the country has never retreated significantly after accomplishing basic reforms. The question which he finally asks is whether the resulting mixed economy with its social theory of property will destroy the flexibility and creativity which has made the United States the productive economic organization that it is.

Given his approach, Smith does a workmanlike and exceedingly thoughtful job. He cut out a complex piece of work for himself and has woven together the many strands, not all just economic, into an integrated whole. But also, given the hundreds of possibilities, it is unlikely that a reader will not wish to dispute a few points.

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Land Economics; Agricultural Economics; Economic Geography; Housing

Conservation in the Production of Petroleum—A Study in Industrial Control.
   By ERICH W. ZIMMERMANN. American Petroleum Institute, Petroleum
   Monogr. Ser., Vol. 2. New Haven: Yale University Press, 1957. Pp. xxii,
   417. $6.00.

This is the second of a series of monographs sponsored by the American
Petroleum Institute, and devoted to exploring the economic intricacies of the
petroleum industry of the United States. It compares very favorably with the
first monograph (Ralph Cassady, Jr., *Price Making and Price Behavior in the
Petroleum Industry,* 1954), in that it has a more solid informational back-
ground and comes closer to the essential analytical issues involved.

However, it must be emphasized that this work is in no sense a general
treatise on the economic or other problems of petroleum conservation, as its
title might suggest. It is effectively a discussion, within a severely limited
frame of reference, of the relative merits of the American program of curtail-
ment and proration of crude petroleum outputs, with some side comments on
connected governmental regulations and industry policies. In this discussion,
the results of the American program are judged mostly in relation to those of
"no program at all." The general conclusion is that our domestic program
has probably enhanced conservation perceptibly—largely through deterring
an unduly rapid antagonistic withdrawal of oil by competing producers in
flush fields—but has significant deficiencies.

The discussion of deficiencies, however, is more or less tacked on at the end,
and suffers from the fact that the relative merits of alternative modes of
regulation or organization of petroleum production are never thoroughly dis-
cussed or compared with those of the present regulatory system.

Given these restrictions on the scope of the work, the expected topics are
developed in acceptable order: definition of resources and conservation; the
physical characteristics of petroleum deposits and relevant aspects of American
property law; history and description of state and federal conservation laws
and policies; special problems of natural gas; evaluation of existing American
oil and gas conservation programs; weaknesses of the programs; future energy
availability. A good deal of essential information is imparted, and a reasoned
defense is presented—satisfactory at least in broad qualitative terms—of the
existing control system as superior to none at all from a conservation stand-
point. The book as a whole, however, is disappointing in a number of ways,
of which the following stand out.

First, it is excessively long in the light of its substantive content, very
discursive in style, and lacking in rigor when engaged in analysis and in
compression when engaged in description. Second, the basic definition of
(ideal) conservation embodies an inelegant and seriously inexact approxima-
tion to the correct definition. Zimmermann generally appears to view the
conservation goal mainly as one of maximizing the aggregate physical re-
covery of oil from any pool over time (without explicit regard for the relation
of cost to recovery, for the price or value pattern through time, or for a time

discount rate), and secondarily as the protection of the property rights of producers. His confusion cn this point is underlined when he argues that accepted formulations for conserving by maximizing the discounted present private or social net value of a resource deposit are inapplicable to petroleum, largely because underground petroleum has "a will of its own" in coming out of the ground. (It is obvious that the present-value-maximum formulations survive nicely if this "independent will" of petroleum is appropriately recognized by postulating certain intertemporal relations between present and deferred costs and yields.) The lack of a precise and generally correct standard for ideal conservation does not seriously affect the evaluations of actual conservation performance, however, since these evaluations are made only in most loose and general terms.

Third, the evaluation of the actual working of the American curtailment-and-proration schemes is seriously marred by the lack of any adequate or close consideration of the probable extent to which the predominance of "equitable" as opposed to engineering considerations in arriving at pool and well quotas under state proration laws has deprived curtailment of its potential (and asserted) advantages as a conservation measure. The subject is mentioned but not pursued; it has been treated extensively in other writings. Fourth, there is similarly no adequate consideration of the extent to which, under our system of controls, wastefully intensive and misplaced drilling of wells has been limited, of the actual industry performance in this regard relative to the ideal, or of the net damage to conservation done by this performance. This omission stems in part from the fact that well-spacing laws and their effects are really discussed for the first time in the "evaluation" chapter —the four earlier chapters on conservation laws having been devoted almost entirely to curtailment and proration activities—and that the ultimate belated discussion of well-spacing legislation and its effects is cursory in the extreme.

Finally, there is no real comparison of the relative conservation merits of our control system and one of unitization in the development of individual oil pools. Unitization is acknowledged as an engineering ideal, but shortly dismissed as usually impractical in this country for various technical and political reasons. Thus, although we are offered some judgments on how much better our controls are than none at all, there are substantially no judgments as to how much worse our controlled system is than one which secures unitization. On this point, voluminous comparative materials on unitized developments abroad have been almost entirely neglected.

Taking such deficiencies of the book into account, and considering the residue, we discover a not very thoroughly documented argument to the effect that curtailment of oil outputs, *cum* proration, in a setting of nonunitized and otherwise lightly regulated exploitation of oil resources, probably enhances ultimate aggregate yields of petroleum and very likely enhances and stabilizes oil-producers' incomes. This position deserves support, but the work which supports it is not an adequate treatise on conservation problems in the production of petroleum.

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Farm Prices—Myth and Reality. By W. W. Cochrane. Minneapolis: University of Minnesota Press, 1958. Pp. vii, 189. $4.00.

The ambitious objective of *Farm Prices* is to find a "new dimension" in farm policy and a solution of the farm problem as the author sees it: prices, incomes and surpluses. •

Cochrane maintains that there exists a "myth," widely held by the public, that if left alone or nearly alone, agriculture will gravitate towards a desirable price and income pattern and level (Ch. 1). In reality, however, prices and incomes fluctuate widely: variability and instability of these two factors are actually the *norm* in agriculture (Ch. 2). To throw a theoretical searchlight on this behavior, the aggregate demand for food (with its characteristically low price elasticity), the aggregate supply of food (with its characteristically very low elasticity) and the all-pervading role of technological advances in increasing food production are analyzed to explain the sharp fluctuations in prices and incomes (Ch. 3). In this context, it is stated that "it is wrong, as wrong as can be, to conclude that a falling farm price level will reduce total farm output" (p. 51). The cobweb analysis is used to interpret—or better, partly interpret—price variability of individual commodities (using potatoes, hogs and milk as examples); the meaning and usefulness of changes in relative prices is outlined (Ch. 4). Cochrane further predicts that the supply of food will outdistance demand during the next two decades. He dramatically describes how farmers—in the competitive market in which they operate—are running faster and faster on the treadmill "in the quest for higher incomes growing out of the adoption of new and more productive techniques" (p. 105) and are actually losing out in terms of their relative income position (Ch. 5). •

Up to this point, the author has traveled over territory familiar to friends of agriculture. In his search for a new policy dimension in the remaining three chapters, Cochrane first describes the role of value judgments in policy-making; exposes his own value scheme; and justifies the role of economists as policy advisers (Ch. 6). He recommends that more members of the profession "openly and unashamedly attempt to play the role of social scientists" (p. 127), meaning that they consider other than purely economic factors and concepts—though he overlooks that the training of agricultural economists today is anything but designed to instill in them the knowledge or the courage to grapple with broad policy implications. He examines the five major approaches to the price-income-surplus problem (free market, flexible price support, farm efficiency, fixed price support, domestic food consumption expansion) and concludes that they are "blind alleys": unable to afford access to their objectives and likely to raise more troubles (Ch. 7). While his criticisms are not justified for all "approaches" (even if taken individually), Cochrane correctly concludes that a highly commercialized and efficient agriculture will enhance the surplus food problem (p. 143). He is also pessimistic with respect to the possibility of sharply expanding food consumption through the demand-expansion approach and thereby solving the price-income problem (p. 150 ff.). Finally, Cochrane endorses production and marketing controls for the most important food commodities to bridle the "technological monster"

and production (Ch. 8) through "the annual determination of the quantity of a commodity that a given market will take at a price defined as fair to producers and consumers alike" (p. 168).

Production and marketing controls are an appealing proposal. It is indeed hard to convince oneself that Congress will not adopt the measures outlined by Cochrane, maybe as the line of least resistance. We have such controls now, and their proposed expansion is the crux of Cochrane's discussion. One could take issue with his endorsement of such a program on the grounds that his argument is built on false premises: namely that his quarrel is not with the public, but with a few prominent "spokesmen for agriculture" who cling to the self-equilibrating-market-myth and who are like those psychologists about whom Freud said that they ignored what nursemaids had known for centuries about the nature of child behavior. The second false premise might be that "society wants to get out of the business of supporting farm prices and incomes; this is abundantly clear" and that society will not choose to pay several billion treasury dollars a year for such supports (p. 164) Such assumption is contrary to fact. "Society" has been in this business for years, and our modern industrial economy seems bent, consciously and purposefully, on providing income security for all its sectors. Cochrane also fails to bring into the proper limelight the incidence and nature of the direct and indirect costs of controls. He does not explain, for instance, those social costs which would arise out of the bridling of that "technological monster" responsible for the farm problem. It is one thing to limit production—another to restrict innovations. Are we to prohibit research, cut funds of experiment stations and extension services, shelve new seeds and fertilizers? To withhold from discussion this vital topic leaves unanswered one possible source of "more troubles" which the author wants to avoid. But let us comment about farm prices on the author's own battleground. *Is supply control a new dimension?*

According to the Food and Agricultural Organization, chronic undernourishment affects "something like half the world's people; R. L. Meier in his exciting *Science and Economic Development* (1956) predicts a world food crisis within the next 25 years. The estimates might be exaggerated and Meier might be wrong; but the fact remains of hunger on a tremendous scale. If one tackles the American farm problem as a "social scientist" (not as an economist or a politician) as Cochrane tries to do, then the "new dimension" ought to take these facts into account; it ought to go beyond the limits of the requirements of the national market and the balancing of national demand and supply, and recognize that for the hungry the preoccupation with equating food supplies with effective demand is a waste of breath. It ought to assert that the Treasury costs of supporting a prosperous and expanding agriculture is one of the cheapest long-run investments the American public can make to over-all social welfare for no other reason than that this nation occupies a role of political, social and economic preponderance. It ought to devise new ways and means of giving away or selling food to the undernourished without causing resentment. Production controls are a pessimist's solution. They ignore the revolutionary contribution which, in theory and practice, U.S.

agriculture is making to the solution of the most urgent world problem: hunger. They imply that when the Malthusian doctrine is about to be shown wrong, well then—lets give it a twist of the arm and make it come true. In this solution, technology is like the water-carrying broom which floods the house when the apprentice has forgotten the formula. Now we must wait for the master who will turn the power of technology to good use—because from the broad, long-run viewpoint, the production- and technology- control program is not the magic word.

<div style="text-align: right">ERNEST FEDER</div>

University of Nebraska

Labor Economics

A Decade of Industrial Relations Research, 1946-1956. Edited by NEIL W. CHAMBERLAIN, FRANK C. PIERSON, THERESA WOLFSON. Industrial Relations Research Association publication No. 19. New York: Harper and Bros., 1958. Pp. viii, 205. $3.50.

This book constitutes the first part of a decade's survey of contemporary literature in the field of industrial relations which was decided upon by the Industrial Relations Research Association some years ago. A second volume is to follow.

In the present volume six areas are treated by seven authors: (1) Union Government and Union Leadership (Joel Seidman and Daisy Tagliacozzo); (2) Collective Bargaining (Joseph Shister); (3) Wage Determination in Theory and Practice (Melvin Reder); (4) The Economic Effects of Unionism (George H. Hildebrand); (5) Employee Benefit Plans (Robert Tilove); (6) The Labor Movement Abroad (Adolf Sturmthal). The basis for the selection of these areas was—according to the Preface—not any logical relation between them, but the fact that in each of them a considerable amount of research had been done. This fact in turn bespeaks their importance to the labor economist.

The several studies, each of them from 25 to 40 pages long, follow the same general plan: each essay discusses the issues and problems in the particular area and their treatment by various authors, and suggests topics which appear to call for further study. These essays are followed by bibliographies of from 60 to nearly 100 titles consisting mostly of books and articles published in the professional or semiprofessional journals. Also listed are papers delivered at conferences and, especially in the study of Employee Benefit Plans, the authentic source material in the various official publications of federal, state, insurance and other agencies having to do with these plans.

The conception of the essays varies somewhat from author to author. Some expound primarily the writings in the field. Others discuss and analyze independently the problems and issues involved, and refer to the various writings on them more by way of references to the subsequent bibliography. Still others strike a balance between these approaches. In some the bibliographical references are at times expanded into brief abstracts. A somewhat questionable feature—in this reviewer's opinion—is the expression of occasional

unsubstantiated value judgments like "the best of these . . ." which are likely
to create a perhaps unwarranted bias in the reader's mind.

Suggestions for further research call partly for empirical studies of particu-
lar fields, partly for more work on theory. Desiderata in the first class are:
more studies of the actual working of formal union machinery and of the
human factors, individual and group, in union leadership and policies; investi-
gations of the impact of health plans on medical care, of the effects of govern-
ment regulation of benefit plans and the need for new legislation; comparisons
of the apparent effects of unionism in various countries. More theoretical work
is desired with regard to the changing aspects of collective bargaining; and as
to wage determination, where the issue of the competitive hypothesis versus
the institutional approach needs clarification. Also there is need for a more
general theory of the labor movement to be based on industrialization rather
than capitalism as the challenge to labor.

Naturally all these studies are most useful in quickly informing academic
and business researchers, teachers, executives and labor leaders on the prob-
lems in the several areas, on practices and methods applied in industrial
relations, and on the literature concerning them.

Regarding the form of the publication, however, there is a question on this
reviewer's mind: Is the book form—slow in being put together, and static in
its very nature—best suited to the purpose? Or might it not be preferable to
make such bibliographical studies of particular areas a more or less regular
feature of the professional journals? If the essay parts were condensed, and the
bibliographical material expanded in the direction of concise abstracts, such
studies, one at a time, might be used periodically to supplement book reviews,
notes and lists of publications.

JOHN V. SPIELMANS

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The Maritime Story : A Study in Labor-Management Relations. By JOSEPH P.
GOLDBERG. Cambridge, Mass.: Harvard University Press, 1958. Pp. xv
+ 361. $6.50.

This well-written volume is one of a projected series of studies of the history
of American labor-management relations during the period of major trans-
formation therein beginning in the 'twenties. It is the first of a number which
will deal with particular industries, companies, and unions of special interest.
The author is a long-time specialist in labor-management relations, and is
currently with the Bureau of Labor Statistics. His writing exhibits a thorough
familiarity with the complexities of the union situation in the maritime area,
and he has succeeded in producing an account which is both absorbing and
analytical of the events, personalities, and trends during this period.

The study has a felicitous chronological organization. He begins with some
account of the relatively oppressed condition of American seamen, as com-
pared with shore workers, at the end of the nineteenth century—a time when
the American wooden sailing ships were being forced out of business by steel
and steam under foreign flags, and by lower labor costs abroad, and when
American steam vessels were making little headway against railroad rivalry

at home and inadequate aid in the offshore trades. The despicable labor record of that era, which was full of the use of brass knuckles, hazing, ship jumping, wild, crime-ridden waterfronts, improperly manned ships, and brutal, though traditional maritime law, is well outlined, but the author could have improved his presentation by a more complete discussion of the fundamental economic disadvantage under which the industry labored. He then turns his attention to the beginnings of collective bargaining on the West Coast, to the career of Andrew Furuseth, the Seamen's Act of 1915 and the controversies which surrounded it. Although this section is very illuminating, he largely ignores the problems created by high costs in one segment of the competitive world merchant fleet.

He then turns to the core of the study, the rise of union organization and the improvement of labor conditions at sea as a result. He discusses in turn the rise of unionism during the first world war, despite the opposition of the Shipping Board, its collapse in the 'twenties with the return of intense international competition, the resurgence in the latter 'thirties under the twin stimuli of government assistance to shipping under the Merchant Marine Act of 1936 and the labor policy of the New Deal, the vast expansion and excellent record of the maritime labor organizations in the second world war, and finally the achievement in the postwar period of a system of orderly relations characterized by union job control, employer acceptance, more responsible union leadership, and wages and working conditions comparable to those existing shoreside. He concludes with a useful chapter which analyzes the seamen's problem, summarizes present practices, and reviews the history. Throughout run several important themes: the struggle of labor against centrifugal forces, the distinctive role of the federal government through its ownership and control of vessels and its subsidies, the vast improvement in the status and rewards of seamen during the period, and finally the gradual emergence of the distinctive bargaining and hiring system which in his view has at last brought stability to a chaotic situation.

The key figures in the maritime labor movement—Andrew Furuseth, Joseph Curran, Harry Lundberg, and Harry Bridges—emerge with some clarity. Many will find these portraits of particular interest. With respect to the last named one might indeed like to have more explicit statements regarding communist affiliations, but the writer contents himself with stating the opinion of Curran and Lundberg that Bridges was under communist influence.

The major defect of the study is that it is almost entirely a history of unionism and of labor relations as seen by the unions. The industry leadership, though mentioned from time to time, is shadowy throughout, and its views are not very fully reported. Apart from the few general works on merchant shipping the writer has relied mainly on union papers and on government reports, especially those dealing with maritime labor problems. There was almost no use of management records, which might add some other judgments. In particular, problems of labor efficiency are neglected. Despite this weakness the author preserves a remarkable balance; the study is in no way to be classed as a propagandist labor work.

Another important defect is the absence of any satisfactory discussion of

international competition in shipping and its significance for this segment of American labor. Beyond the stubborn opposition of the shipowners to all improvements, unenlightened as it often was, one sees only faintly the pressures of efficient European and even Oriental labor, of more efficient foreign-built ships, and of falling berth and charter rates. It is true that the author very explicitly recognizes that the achievement of full shoreside parity for American seamen has depended mainly on government aid through differential subsidies, the fifty-fifty rule with respect to the shipment of foreign aid cargoes, and in other ways. But this recognition is hardly enough. Has the wage differential widened in recent years? What has been the impact of American collective bargaining on that abroad and vice versa? Are American ships increasing or decreasing in their labor efficiency as compared with foreign flags? How have maritime labor costs changed as compared with those in railroads and motor transport? To such questions one can find no answer in this otherwise very fine study. Nevertheless the work ably illuminates a heretofore dark corner of maritime history.

JOHN G. B. HUTCHINS

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The Worker Views His Union. By JOEL SEIDMAN, JACK LONDON, BERNARD KARSH, and DAISY L. TAGLIACOZZO. Chicago: University of Chicago Press, 1958. Pp. x, 300. $5.75.

Prior to the 1950's there was little published research on local unions. This volume is a comparative study of the attitudes of workers in six local unions toward their unions, employers, and jobs. While the authors have previously published much of this material in journal form, some of it has appeared in journals not regularly read by professional economists.

Each local was chosen to represent a broad area of the American labor movement: a coal-miners' local to represent workers in a one-industry area with strong union traditions and authority concentrated in the national officials; a plumbers' local to depict the craft-conscious building trades; a telephone-workers' local to represent white-collar workers; a knitting-mill local to exemplify workers' attitudes in a newly formed local after an organizational strike; and farm-equipment and steel-workers locals as illustrations of aggressive unionism in the mass-production industries. The authors are careful to note that these locals are all in the Midwest, that they would have preferred using more locals, but limited time and money prevented the extension of the study to cover additional unions.

This volume affords good evidence of the attitudes of members toward their union, but we can touch in this review on only a few of the findings. Readers may be particularly interested in the different attitudes of the leadership and the membership toward union participation in politics and factionalism. When asked some form of the question "Should unions be active in politics?" a clear majority of the rank-and-file members in all but the steel and farm-equipment locals answered in the negative; in the steel and farm-equipment locals only about one-half favored political action by their union. The leadership, on the other hand, generally favored union political activity.

In the steel and farm-equipment locals the workers opposed factionalism, and the leadership favored it. The workers tended to view the union as a collective bargaining institution and therefore wanted unity, while the leaders were of the view that factionalism preserved democracy in their unions. Readers will also be interested in the sevenfold classification of union members which the authors present, ranging from the "ideological unionist" at one extreme to the "unwilling unionist" at the other.

The authors explain their methodology and some of its limitations in an appendix. They note that a few hundred interviews in six locals is hardly a sufficient sample to depict the entire labor movement. However, there are two shortcomings in the methodology adopted which were not mentioned by the authors. Particular care was taken to select only those locals which were not under investigation and whose officials would be cooperative. Local officials who are uncooperative are likely to be so because they anticipate, with good reason, that the attitude of the membership toward the leaders is likely to be somewhat short of overwhelmingly favorable. It is true that interviews with members of unions whose officials are uncooperative would be a difficult thing. However, by restricting their sample to locals which were not under investigation and whose leaders were cooperative they were likely to get a sample more favorably biased toward the union than is the membership of the labor movement as a whole.

Your reviewer feels that the choice of telephone workers to be representative of the white-collar workers was unfortunate to begin with. Although telephone operators dress neatly and the work is clean, that is where the similarity to white-collar workers ends. They are subject to at least as tight a discipline as are factory workers; the operators cannot talk with one another while working, they work odd hours and split shifts, must obtain permission to leave their work, and are closely supervised. There are many unions containing locals whose members would better fit the category of "white-collar workers" than would a local composed primarily of telephone operators.

Furthermore, the value of this study would have been enhanced had more quantitative measures been provided. The authors frequently use such imprecise terms as "some," "substantial minority," "many" and "a number of."

These criticisms do not detract from the usefulness of this study. It is a valuable addition to the literature on local unions. As such, it will prove helpful to students of labor-management relations and to partisans on both sides of the fence.

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"These essays represent revised versions of faculty contributions to a Joint Seminar conducted at Duke University in the spring of 1957. The broad purpose . . . was to provide . . . an introduction to Commonwealth problems and to the possibilities for advanced research therein. The faculty participants—economists, historians, political scientists—contributed to the objectives of the Seminar through a series of papers in which

they brought to bear the research and analytical techniques of their respective disciplines." (From the preface.)

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NOTES

Members who wish to make suggestions for officers of the American Economic Association for 1959-60 are invited to place names with James Washington Bell, secretary of the Association, for transmission to the Nominating Committee when appointed by the incoming President.

FELLOWSHIPS FOR REGIONAL FACULTY RESEARCH SEMINARS IN ECONOMICS

The Ford Foundation announces the sponsorship of 6 Regional Research Seminars in Economics to be held in the summer of 1959 for an 8-week period. Participation in the seminars is open on a competitive basis to faculty members teaching economics or business subjects at liberal arts colleges or other institutions of higher learning that do not offer a doctorate in economics. It is the purpose of this program to enhance the effectiveness of teaching and to encourage research studies of significance.

Each seminar will be limited to 10 participants. The seminars will be devoted to a survey of important recent contributions to the literature, and an examination of the major research problems in the field. Participants will formulate research projects which it is anticipated will be continued over the following academic year. The seminar group will meet at least once in the following year to discuss progress in these research studies.

The regions, directors, and subject areas will be the following: I, New England States and New York State excluding New York City: Professor Charles Kindleberger, Massachusetts Institute of Technology, International Economics. II, Middle Atlantic, including New York City, New Jersey, Maryland, Virginia, West Virginia, District of Columbia, Delaware and Pennsylvania: Professor Paul Strayer, Princeton University, Public Finance. III, Southeast, including North Carolina, South Carolina, Georgia, Florida, Alabama, Mississippi, Tennessee, Louisiana, Arkansas: Professor Maurice Lee, University of North Carolina, Economic Fluctuations, Growth and Stability. IV, Middle West, including Ohio, Kentucky, Indiana, Illinois, Michigan, Wisconsin, Minnesota, Missouri and Iowa: Professor Walter Adams, Michigan State University, Government and Business. V, Northwest, including Washington, Oregon, Idaho, Montana, Wyoming, North and South Dakota, Nebraska, and Northern California: Professor Raymond Mikesell, University of Oregon, International Economics and National Policies. VI, Southwest, including central and southern California, Nevada, Utah, Arizona, Colorado, New Mexico, Kansas, Oklahoma and Texas: Professor Andreas Papandreou, University of California (Berkeley), Theory of the Firm.

Fellowships provide a stipend of $800 to compensate in part for loss of summer earnings, a $200 allowance for living expenses, and travel expenses. Preference will be given to applicants who have had a minimum of 3 years' teaching experience since attending graduate school, and who are under 50 years of age. Applications may be secured from the seminar director in the applicant's region. The deadline for submitting applications is January 15, 1959. Awards will be announced not later than March 1.

ANNOUNCEMENTS

A 5-year grant for graduate seminar workshops has been given the economics department of Columbia University by the Ford Foundation. For 1958-59 programs have been launched in public finance, expectational economics and economic development of industrial countries. Fellowships and assistantships will be available under this program.

Claremont Men's College has announced that its newly found Institute of Federalism will sponsor studies in the political economy of American Federalism. Specialists in admin-

istrative organization, economics, public finance, and poltical ethics and political history from its present faculty, augmented by several additial appointments, will conduct research in the political and economic effects cf federal grants-in-aid programs, the functional divisions of federal, state, and local government, the comparative costs and efficiency of federal and state services, and the extent and effect of centralization upon elections, candidates and platforms. •

The national AFL-CIO has established a speakers bureau for college and university functions. The bureau intends to have speakers available for seminars, forums and any type of meeting that would appear to be educationally desirable. Interested academic officials should write to Tilford E. Dudley, AFL-CIO Department of Public Relations, 815-16th St. N.W., Washington 6, D.C.

The U. S. Civil Service Commission is still accepting applications for economist positions in the following specialized fields: business, international trade and development, fiscal and financial, transportation, labor, and general economics. The entrance salaries now range from $5,985 to $12,770 a year. To qualify, applicants must have completed either a 4-year college course with major study in economics, or have had 4 years of successful progressive experience in economics. In addition, they must have had from 2 to 4 years of advanced economic experience, part of which must have been in one of the specialized fields. Further information and application forms may be obtained at many post offices throughout the country, or from the U.S. Civil Service Commission in Washington.

NEW PUBLICATIONS

The first issue of the *Journal of Law & Economics* will be published in October 1958. This annual journal will specialize in the examination of issues of public policy of joint interest to lawyers and economists. It is to be published by the University of Chicago Law School under the editorship of Aaron Director, professor of economics in the Law School.

A group of scholars interested in assessing the impact of technology on society have formed the Society for the History of Technology. The Society will sponsor meetings and will publish a quarterly journal, *Technology and Culture*, devoted to the study of the development of technology and its relations with society and culture. The Society expects to begin publication of *Technology and Culture* in the fall of 1959. Applications for charter membership ($10.00) in the Society for the History of Technology should be sent to the chairman of the executive committee, Professor Melvin Kranzberg, Room 315, Main Building, Case Institute of Technology, Cleveland 6, Ohic.

Deaths

Paul.E. Bryant died September 18, 1957.

Alexander H. Erlick of the University of Miami died in June 1958.

Rowland G. Freeman, Harvard Graduate School of Business Administration, died July 23, 1958.

Leon Goldenberg died July 24, 1958.

Edward Kilberg of the National Bureau of Economic Research was killed in an accident on August 15, 1958.

Roy C. Osgood died July 9, 1958.

Stefan Valavanis of Harvard University met with an accidental death in Greece, July 1958.

Visiting Foreign Scholars

Maurice Allais of the École Nationale Supérieure des Mines de Paris will be visiting professor at the Thomas Jefferson Center for Studies in Political Economy, University of Virginia, in the second semester.

Sven Danö of the University of Copenhagen is visiting research professor at the University of Illinois.   •

Gordon R. Fisher, of England, has been appointed research associate in the Research Seminar in Quantitative Economics, University of Michigan.

John Johnston, on leave from the University of Manchester, England, is visiting associate professor of economics at the University of Wisconsin.

Kameo Matusita of the Institute of Statistical Mathematics, Tokyo, will be in this country November 1 under a Rockefeller grant and will visit various universities throughout the country.

N. A. Mavlankar of Fergusson College, Poona, India, is visiting lecturer in the department of economics, Florida State University.

Alan Peacock, of the University of Edinburgh, is visiting professor at the Johns Hopkins University in the first semester of the current academic year.

Erich Schneider of Christian-Albrechts-Universität, Kiel, Germany is here for six months under a Rockefeller grant and will visit several universities throughout the country.

A. A. Walters of the University of Birmingham, England is visiting associate professor of economics at Northwestern University in the current academic year.

Appointments and Resignations

Walter Adams has returned from Europe and the Middle East where he made an evaluation of American university programs there under the auspices of the Carnegie Corporation.

Reijo G. Aho has been appointed instructor in the department of economics at Michigan State University.

Joseph Airov has been promoted to associate professor of economics in the School of Business Administration, Emory University.

Armen A. Alchian has been promoted to professor of economics at the University of California, Los Angeles.

William R. Allen has been promoted to associate professor of economics at the University of California, Los Angeles.

Clayton W. Anderson has been appointed instructor in accounting, School of Business, Northwestern University.

Robert E. Asher of the Brookings Institution is on leave for eight months to head a special mission in Tunisia for the Food and Agriculture Organization.

Ruben V. Austin has been appointed assistant to the dean of the College of Business and Public Service at Michigan State University. He is also coordinator of the Business Administration School in Sao Paulo, Brazil.

George L. Baker, Jr. has been appointed research associate in business administration in the Harvard Graduate School of Business Administration.

Stephen J. Barres has been appointed associate professor and major advisor of management in the College of Business Administration, University of Nevada.

Merrill D. Bartlett has been appointed instructor in business and economics at the University of Maine.

James W. Beck has been appointed director of the Bureau of Business and Economic Research and acting chairman of the department of economics at the University of North Dakota.

Jack F. Bennett has transferred to London as deputy European financial representative of the Standard Oil Company (New Jersey).

James Berna has been appointed assistant professor of economics at Georgetown University.

Bernard P. Bernsten has resigned from the Econometric Institute to accept a position as staff economist and secretary of the Committee on Business Statistics, United States Chamber of Commerce.

Arthur G. Billings has been appointed associate professor of economics at the University of Kansas City.

B. D. Bixley has been appointed instructor in the department of political economy of the University of Toronto.

Joseph B. Black, Jr. has been appointed faculty lecturer in finance in the School of Business, Indiana University.

Albert L. Blum, formerly of the National Industrial Conference Board, has been appointed assistant professor in the New York State School of Industrial and Labor Relations, Cornell University.

Morris Bornstein has been appointed assistant professor of economics at the University of Michigan.

W. Donald Bowles has been appointed assistant professor of economics at the American University.

Donald O. Bowlin has been appointed assistant professor in the department of economics at Pennsylvania State University.

Carl T. Brehm, Jr. has been promoted to assistant professor of economics at Michigan State University.

Henry Briefs is on leave for one year from Georgetown University to work with the Council of Economic Advisers.

Louis P. Bucklin has been appointed instructor in marketing in the School of Business, Northwestern University.

Arthur D. Butler has been promoted to associate professor of economics in the School of Business Administration, University of Buffalo.

William R. Campbell has resigned as faculty lecturer in management in the School of Business, Indiana University.

Wilfred V. Candler is a member of the economics faculty at the University of New England, Australia.

Rocco Carzo, Jr. has resigned from the School of Business, Indiana University, to accept a position at Pennsylvania State University.

Frank J. Charvat has been promoted to associate professor of business administration in the School of Business Administration, Emory University.

Hollis Chenery was promoted to professor of economics at Stanford University, effective September 1957.

Nanda K. Choudhry of the University of Wisconsin has been appointed visiting assistant professor at the University of Rochester.

James Collier has been promoted to assistant professor of economics at the University of North Dakota.

Edward J. Cook has been appointed chairman of the department of economics in the School of Business, Fordham University.

John D. Coupe has been appointed assistant professor of business and economics at the University of Maine.

F. R. Crocombe retired June 30, 1958 from the department of political economy, University of Toronto.

J. H. Dales has been promoted to associate professor in the department of political economy, University of Toronto.

Peter L. Danner has been appointed associate professor and chairman of the department of economics and business administration, St. Ambrose College, in the current academic year.

Martin Diamond has accepted an associate professorship at Claremont Men's College.

Robert Dinman of the University of Florida has been granted a leave of absence to accept a position at the University of Indonesia in Jakarta under the auspices of the Ford Foundation.

Joel B. Dirlam has returned to the University of Connecticut as associate professor of economics.

Edgar S. Dunn has been granted leave from the University of Florida to accept a position with the Organization for European Economic Cooperation in Paris.

Read R. Durtschi has resigned from the University of Utah to accept an appointment at Utah State University.

John M. Dutton has been promoted to research associate in business administration in the Harvard Graduate School of Business Administration.

Alfred L. Edwards has been promoted to assistant professor of economics at Michigan State University.

Theo H. Ellis of North Dakota Agricultural College has accepted a position as agriculturist with the Farm Economics Research Division, U. S. Department of Agriculture, at Polytechnic Institute, Auburn, Alabama.

Roland T. Ely has been appointed instructor in economics in University College of Rutgers—The State University.

Harry F. Evarts of Harvard Graduate School of Business Administration, has been appointed instructor in production management in the School of Business, Northwestern University.

Marvin L. Fair of Tulane University will serve as visiting professor of transportation at American University in the current academic year.

John M. Ferguson, formerly of the University of Pittsburgh, has been named chairman of the department of economics at Chapman College, Orange, California.

Max W. Fletcher has been appointed assistant professor of economics in the College of Business Administration, University of Idaho.

·Earl B. Franch of Bucknell University has been appointed assistant professor in the New York State School of Industrial and Labor Relations, Cornell University.

C. B. Franklin has been appointed instructor in the department of economics, Florida State University.

Joseph R. Fritzemeyer has been appointed faculty lecturer in accounting in the School of Business, Indiana University.

Samuel Frumer has been appointed faculty lecturer in accounting in the School of Business, Indiana University.

W. D. Gainer, on a year's leave from the University of Alberta, is research associate in the department of political economy, University of Toronto.

James A. Gentry has been appointed lecturer in business administration in the School of Business Administration, Emory University.

Claude S. George, associate professor of industrial management, has been appointed assistant dean of the School of Business Administration, University of North Carolina.

Cecil R. Glaves has resigned from the department of business and economics, Illinois Institute of Technology.

Edwin C. Gooding has been appointed instructor in economics at Lehigh University.

Oscar R. Goodman has been appointed visiting associate professor of finance in the School of Business, Northwestern University.

William E. Gordon has been appointed associate professor at Simpson' College.

Wytze Gorter, of the University of California, Los Angeles, is Fulbright lecturer in international economics at the University of Gröningen, Netherlands, this year.

Margaret Graves, formerly of the Graduate School of Business Administration, Harvard University, has been appointed instructor in the department of economics, University of Florida.

David G. Greene has been appointed instructor in the department of economics, Michigan State University.

Albert Griffin has been promoted to professor of business administration in the School of Business Administration, Emory University.

Gene K. Groff has been appointed faculty lecturer in management, School of Business, Indiana University.

Morton C. Grossman has been granted leave from the State College of Washington to serve as chief program economist and principal economic policy adviser in India, stationed in New Delhi, for the U. S. International Cooperation Administration.

William L. Haeberle has been appointed associate professor of management and director of management leadership program, School of Business, Indiana University.

Paul K. Hall has been appointed faculty lecturer in accounting, School of Business, Indiana University.

W. Lee Hansen of Johns Hopkins University has been appointed assistant professor of economics at the University of California, Los Angeles.

Kurt L. Hanslow of the legal staff of the United Automobile workers, AFL-CIO, is visiting associate professor in the New York State School of Industrial and Labor Relations, Cornell University.

George Hardbeck has been appointed associate professor at Louisiana Polytechnic Institute.

Joseph E. Haring has been appointed instructor in economics at Barnard College.

Neil Harl has been appointed research associate jointly with the Agricultural Law Center, State University of Iowa, the Agricultural Research Service, USDA, and the Agricultural Experiment Station of Iowa State College.

Robert E. Harris has been appointed assistant professor of business administration and finance in the School of Business Administration, The American University.

Albert G. Hart has been appointed chairman of the economics department at Columbia University.

Mitchell Harwitz has been appointed assistant professor of economics at Northwestern University.

John P. Henderson has been appointed economist in the Bureau of Business and Economic Research, and associate professor in the department of economics, Michigan State University.   •

Kenneth M. Henderson, formerly president of Ditto, Inc., has been appointed lecturer in business administration, School of Business, Northwestern University.

Clifford Hildreth has been appointed head of the department of economics, Michigan State University.

Randall Hinshaw is visiting professor of economics at Oberlin College this year.

Werner Z. Hirsch has been granted a year's leave of absence from Washington University to serve with Resources for the Future.

Jack Hirshleifer of the University of Chicago is visiting associate professor of economics at the University of California, Los Angeles.

Karel Holbik has been appointed assistant professor of economics and finance at Boston University.

Bob R. Holdren, formerly of Williams College, has been appointed assistant professor of economics in the department of economics at Iowa State College.

Stanley C. Hollander of the Wharton School has been associate professor in the College of Business and Public Service, Michigan State University, since January 1958.

Jerome J. Hollenhorst has been appointed instructor in economics in the department of economics at Iowa State College.

Donald R. Hughes has been promoted to research associate in business administration in the Harvard Graduate School of Business Administration.

Franklin Yen Hui Ho is assistant professor of business administration at the University of Portland.

Donald F. Istvan has resigned from the School of Business, Indiana University.

Myra Janco has been promoted to associate professor of marketing in the School of Business, Indiana University.

Leland H. Jenks of the California Institute of Technology will be visiting professor of economics at Columbia University in the 1959 spring semester.

James E. Jensen has resigned from the Federal Reserve Bank of Dallas to accept an appointment as associate professor of economics at Wisconsin State College, Stevens Point.

Mrs. Dell B. Johannesen has been appointed assistant professor of economics at the University of North Carolina.

Clyde C. Jones has been appointed assistant dean, College of Commerce and Business Administration, University of Illinois.

William A. Kamins has been promoted to research associate in business administration, Harvard Graduate School of Business Administration.

Laura L. Karadbil has been appointed instructor in accounting, School of Business Administration of The American University.

Gordon M. Kaufman has been promoted to research associate in business administration, Harvard Graduate School of Business Administration.

Robert M. Kaufman has resigned from the Antitrust Division of the Department of Justice and has been appointed legislative assistant to U. S. Senator Jacob K. Javits of New York.

Lloyd Keeton, formerly of Ferris Institute, has been named research associate at the University of Kentucky Bureau of Business Research.

William N. Kinnard, Jr. has been promoted to associate professor of finance and real estate and head of the business department, School of Business Administration, University of Connecticut.

Charles R. Klasson has been appointed faculty lecturer in management, School of Business, Indiana University.

George Kleiner has returned to the University of Illinois after spending a year in Japan as Fulbright lecturer.

Allen V. Kneese has resigned from the University of New Mexico to accept a position as research economist with the Federal Reserve Bank of Kansas City.

William E. Koenker is on a two-year leave from the University of North Dakota to be program economist with the International Cooperation Administration in Baghdad, Iraq.

Alexander Kondonasis has joined the staff of the University of Oklahoma as assistant professor.

Anthony Krzystofic has been appointed instructor in accounting in the School of Business Administration, University of Massachusetts.

Marian Krzyzoniak has been appointed research associate in the department of economics, University of Michigan, to work on the Merrill Foundation Tax Research Study.

John D. Lages, formerly of the University of Missouri, has been appointed instructor in economics at Iowa State College.

Thomas J. Leary has joined the faculty of Northeastern University as assistant professor of economics.

B. C. Lemke of Michigan State University will conduct a course in financial control for the Istituto Post-Universitario per lo Studio dell'Organizzazione Aziendale (IPSOA), in Turin, Italy, this year.

Eugene N. Lerner, formerly with the Federal Reserve Bank of Chicago, has been appointed assistant professor of finance in the School of Business, Northwestern University.

Richard K. Lewis has been appointed instructor in the School of Business, University of Kansas.

Ben A. Lindberg, formerly of the Harvard Graduate School of Business Administration, has been appointed professor of business administration and assistant dean of the School of Business Administration, The American University.

Charles Lockyer has been granted leave of absence from the University of Kentucky

Bureau of Business Research to serve as director of the Division of Accounts and Administrative Service, Kentucky Department of Highways.

Stanley Long of Northwestern University is teaching at Yale University in the current academic year.

David E. Lund has been appointed assistant professor of transportation in the School of Business, Northwestern University.

Richard M. Lyon has been appointed professor of business organization and management in the College of Commerce, University of Notre Dame. He is also lecturer in law and industrial relations at Lake Forest College.

Ivory L. Lyons has joined the faculty of Northeastern University as instructor in economics.

H. I. Macdonald has been appointed research associate in the department of political economy, University of Toronto.

Don A. Markwalder of Northwestern University has been appointed instructor in economics at the University of Toledo.

David D. Martin, on leave from Washington University, is visiting associate professor of business economics and public policy in the School of Business, Indiana University.

James W. Martin has resigned as Kentucky Commissioner of Highways to return to the University of Kentucky as director of the Bureau of Business Research. Upon completion of his duties with the state of Kentucky he was given the Award of the Governor's Medallion for outstanding service.

Raymond R. Mayer has resigned from the School of Business, University of Chicago, to become associate professor of management in the business and economics department, Illinois Institute of Technology, effective Jan. 1, 1959.

Howard McBride of the University of Illinois has been appointed assistant professor at the University of Cincinnati.

Ray H. McClary has been appointed faculty lecturer in accounting, School of Business, Indiana University.

Harold F. McClelland has been appointed instructor in economics, Claremont Men's College.

I. McDonald has been appointed lecturer in the department of political economy, University of Toronto.

Edgar L. McGowan has been promoted to associate professor of accounting in the School of Business Administration, University of South Carolina.

Lachlan McGregor of the University of Illinois has accepted an appointment in the economic history department, Northwestern University.

Bruce D. McSparrin, Jr. has been promoted to assistant professor of management in the School of Business, Indiana University.

Emanuel Melichar has been appointed instructor in economics at the University of Connecticut.

Edmund A. Mennis has been promoted to director of research, Wellington Fund, Philadelphia.

Kenneth Merrill has been appointed instructor in the School of Business, University of Kansas.

Emil A. Mesics has been appointed associate professor in the New York State School of Industrial and Labor Relations, Cornell University.

Albert Meyers has become professor emeritus, Georgetown University.

Jerry Miner has been appointed assistant professor in the economics department, Syracuse University.

William M. Morgenroth has been appointed instructor in industry, School of Business Administration, University of Pittsburgh.

Aurelius Morgner has resigned from Texas A. and M. College.

Edward J. Morrison has resigned from the School of Business, Indiana University.

Norman W. Mosher has been appointed instructor in economics at the University of Maryland.

Grady L. Mullennix has resigned from Michigan State University to accept an appointment as associate professor of business administration, Fresno State College, Fresno, California.

Glen Mumey has been appointed instructor in economics at the University of North Dakota.

Lawrence B. Myers has been assigned to the International Cooperation Administration in Burma since May 1957.

Marc Nerlove has been appointed visiting lecturer in political economy at the Johns Hopkins University for the fall term.

Herbert E. Newman has been appointed chairman of the department of economics at Dickinson College.

Edwin G. Nourse will be visiting distinguished professor of economics at the Pennsylvania State University in the spring semester 1959.

John F. O'Leary, Jr. has been appointed instructor in economics at Lehigh University.

John T. O'Neil has been promoted to professor of business administration, School of Business, Northwestern University.

Danilo Orescanin has been appointed faculty lecturer in business administration and administrative•assistant to the dean, School of Business, Indiana University.

David W. Ortlieb has resigned from the School of Business, Indiana University.

Robert W. Paterson, head of the Bureau of Business and Economic Research, has been promoted to professor of economics at the University of South Carolina.

Arnold A. Paulsen has been appointed assistant professor of economics and extension economist in farm management in the department of economics, Iowa State College.

H. Austin Peck has been promoted to professor of business and economics at the University of Maine.

Jørgen Pedersen has been appointed visiting professor of business, economics and government, School of Business, Indiana University.

Frances Perkins, former Secretary of Labor, is visiting lecturer in the New York State School of Industrial and Labor Relations, Cornell University in the current academic year.

Leroy L. Phaup, Jr. has been appointed associate professor of insurance in the School of Business Administration, University of South Carolina.

Clinton A. Phillips has resigned from the University of Tennessee to accept an appointment in the department of economics, Tulane University.

Janus Poppe has been appointed instructor in the department of economics, University of Maryland.

Reed M. Powell has been appointed visiting lecturer in business administration in the Harvard Graduate School of Business Administration.

Jan S. Prybyla, formerly of the College de l'Europe Libre, has been appointed assistant professor of economics at The Pennsylvania State University.

Olin S. Pugh has been promoted to associate professor of economics at the University of South Carolina.

LeRoy Qualls has been given leave from the University of Florida to serve with the State Budget Department, Tallahassee, Florida.

Clyde N. Randall has been appointed dean of the College of Business, University of Utah.

Robert A. Randolph has been appointed assistant professor at Springfield, Mass., YMCA College.

Arnold H. Raphaelson has been appointed assistant professor of business and economics at the University of Maine.

Jewell J. Rasmussen has been appointed head of the department of economics, at the University of Utah.

Emmett J. Rice has been promoted to assistant professor in the department of economics, Cornell University.

Ronald G. Ridker has been promoted to assistant professor of economics at Washington University.

J. Roberts has been appointed lecturer in the department of political economy, University of Toronto.

Merrill J. Roberts has been appointed professor of transportation in the School of Business Administration, University of Pittsburgh.

Robert A. Robertson, formerly of the University. of Arkansas, has been appointed assistant professor of economics at the University of New Mexico.

Stefan H. Robock is now the deputy director of the Business-Education Division, Committee for Economic Development, New York, N.Y.

R. Thayne Robson has been appointed acting assistant professor of economics at the University of California, Los Angeles.

James A. Rogers has accepted an assistant professorship at Claremont Men's College.

Robert R. Rogers has been appointed instructor in the department of business and economics, Illinois Institute of Technology.

Robert W. Rosen has been appointed interim assistant professor in the economics department, University of Florida.

Edward Rosenbaum of Wayne University has been appointed assistant professor in the School of Business Administration, The American University.

Stephen W. Rousseas, of the University of Michigan, is visiting lecturer in economics at Yale University.

Marvin Rozen, formerly of Stanford University, has accepted an appointment in the economics department, Pennsylvania State University.

P. H. Russell has been appointed lecturer in the department of political economy, University of Toronto.

Arnold Saggese has been appointed assistant professor of economics at Iona College, New York.

Robert L. Sandmeyer, formerly of Fort Hays Kansas State College, has been appointed instructor in economics in the department of economics, Iowa State College.

J. E. Sands has been appointed assistant professor of accounting in the department of political economy, University of Toronto.

Ralston D. Scott has resigned from Atterbein College, Ohio, to become head of the department of business administration, Rochester Institute of Technology.

Lloyd V. Seawell has been promoted to assistant professor of accounting, School of Business, Indiana University.

Robert J. Senkier has been appointed assistant dean of the Graduate School of Business, Columbia University.

Ezzedin M. Shamsedin, formerly of Queen's College, Charlotte, N. C., has been appointed assistant professor of economics in the School of Business Administration, University of South Carolina.

Thomas W. Sharkey has been appointed faculty lecturer in management, School of Business, Indana University.

Robert P. Shay has been appointed director of the School of Business Administration, University of Maine.

Francis Shieh has been appointed instructor in economics in Immaculate Heart College, Los Angeles.

George B. Simmons has been appointed faculty lecturer in business administration, School of Business, Indiana University.

Harold E. Simmons has been appointed instructor in the department of economics, Michigan State University.

Gerald A. Simon has been appointed lecturer in marketing, School of Business, Northwestern University.

Joseph Skehan has been appointed instructor in the department of economics, Georgetown University.

Charles W. Skinner has been appointed research associate in business administration in the Harvard Graduate School of Business Administration.

G. Slasor has been appointed lecturer in the department of political economy, University of Toronto.

Alfred G. Smith, Jr. has been named head of the department of economics, University of South Carolina.

Irvin Sobel, on leave from Washington University, is visiting professor of economics at the University of Bologna, Italy.

Marie W. Spencer has resigned as assistant professor in the department of business and economics, Illinois Institute of Technology.

Richard L. A. Sterba is on leave from the department of economics to participate in the research program in nuclear energy in the College of Engineering at the University of Florida.

Ben S. Stevenson, formerly of Ohio State University, has been appointed instructor in finance, School of Business, Northwestern University.

John B. Stewart has been appointed assistant professor of business administration in the Harvard Graduate School of Business Administration.

James H. Street of Rutgers-The State University lectured in the Foundation for Higher Studies in Management in Buenos Aires as well as in various other Argentine universities during the past year.

Robert H. Strotz has been promoted to professor of economics at Northwestern University. He is visiting professor of economics at Massachusetts Institute of Technology in the current academic year.

Robert H. Stroup has been named assistant director of the Bureau of Business Research, University of Kentucky.

Richard K. Stuart is on leave from the University of Maine to serve as visiting lecturer at Pomona College, California.

Frank M. Tamagna, consultant on savings statistics at the Board of Governors, Federal Reserve System, has been appointed fulltime professor of economics at The American University.

George C. Thompson has been promoted to professor of accounting and business law in the Graduate School of Business, Columbia University.

Richard S. Thorn has been transferred from Washington to the Paris Office of the International Monetary Fund.

Charles M. Tiebout has been appointed assistant professor of economics at the University of California, Los Angeles.

Richard Timberlake has been appointed assistant professor in the department of economics, Florida State University.

C. F. Joseph Tom has been appointed acting chairman of the department of economics and business administration at Lebanon Valley College, Annville, Pa.

Ralph N. Traxler, Jr. has been promoted to associate professor of business administration in the School of Business Administration, Emory University.

Orba F. Traylor has been appointed Commissioner of Finance, Commonwealth of

Kentucky. He was formerly executive director of the Kentucky Legislative Research Commission.

Joseph Tryon has been appointed instructor in the department of economics, Georgetown University.

Sho-Chieh Tsiang has been appointed visiting lecturer in political economy at the Johns Hopkins University for the current academic year.

William C. Tuthill, University of Illinois, has been appointed professor of accounting in the School of Business Administration, University of South Carolina.

Allan J. Twark has been appointed assistant professor of economics at St. Joseph's College, Collegeville, Indiana.

Francis D. Tyson of the School of Business Administration, University of Pittsburgh, retired July 1, 1958.

Daniel C. Vandermeulen has been promoted to professor of economics at Claremont Men's College and Claremont Graduate School.

Manuel A. Velez-Montes has been appointed instructor in economics in the University College of Rutgers—the State University.

Robert F. Voertman has been promoted to associate professor of economics and business at Grinnell College.

George A. von Peterffy has been appointed instructor in business administration in the Harvard Graduate School of Business Administration.

Maneck S. Wadia has been appointed assistant professor of business administration, School of Business, Indiana University.

Norman Waks has been appointed research associate in business administration in the Harvard Graduate School of Business Administration.

Dilworth Walker has retired as head of the department of economics and dean of the College of Business, University of Utah. He will continue as professor of economics.

Donald A. Walker has been appointed instructor in economics and finance at Boston University.

Henry C. Wallich of Yale University is on leave for 1958-59 to serve as assistant to the Secretary of the Treasury.

Clarence C. Walton, formerly of Duquesne University, has been appointed dean of Columbia University Graduate School of Business.

M. H. Watkins has been appointed lecturer in the department of political economy at the University of Toronto.

Harold L. Wattel has been promoted to associate professor of economics at Hofstra College. He is also director of the Bureau of Business and Community Research.

Murray L. Weidenbaum has been appointed economist in the headquarters office of Boeing Airplane Company, Seattle, Washington.

Robert M. Weidenhammer is on leave from the University of Pittsburgh to lecture on a Fulbright fellowship at the University of Munich in the current academic year.

Burton A. Weisbrod has beeen promoted to assistant professor of economics at Washington University.

Clifton R. Wharton Jr. has been named field associate in agricultural economics for Malaya and nearby Southeast Asian countries by the Council on Economic and Cultural Affairs, Inc.

Arthur M. Whitehill, Jr. has returned to the University of North Carolina from a year lecturing at Keio University in Japan on a Fulbright fellowship.

Ronald P. Willett has been appointed faculty lecturer in marketing in the School of Business, Indiana University.

J. Earl Williams, formerly of the University of Wisconsin, is assistant professor of economics at the University of Tennessee.

James W. Wilson, formerly of the University of British Columbia, is associate professor of business administration and sales management in the School of Business Administration, The American University.

Whitten P. Windham has been appointed instructor in accounting at the University of North Carolina.

Tadeusz Z. Wojcik has been appointed lecturer in business and economics at the University of Maine.

Ronald H. Wolf, formerly of Vanderbilt University, is assistant professor of economics at the University of Tennessee.

J. N. Wolfe has returned to the department of political economy at the University of Toronto after a year spent at the University of Rochester as visiting professor.

Robert T. Woodworth has been appointed instructor in business economics, School of Business, Northwestern University.

Cyril A. Zebot has been appointed associate professor of economics at Georgetown University.

Fred Zeller has been appointed instructor in economics at the University of North Dakota.

VACANCIES AND APPLICATIONS

The Association is glad to render service to applicants who wish to make known their availability for positions in the field of economics and to administrative officers of colleges and universities and to others who are seeking to fill vacancies.

The officers of the Association take no responsibility for making a selection among the applicants or following up the results. The Secretary's Office will merely afford a central point for clearing inquiries; and the Review will publish in this section brief description of vacancies announced and of applications submitted (with necessary editorial changes). Since the Association has no other way of knowing whether or not this section is performing a real service, the Secretary would appreciate receiving notification of appointments made as a result of these announcements. It is optional with those submitting such announcements to publish name and address or to use a key number. Deadlines for the four issues of the Review are February 1, May 1, August 1, and November 1.

Communications should be addressed to: The Secretary, American Economic Association, Northwestern University, Evanston, Illinois.

Vacancies

International Cooperation Administration: This Administration has several openings at the moment and anticipates additional vacancies from time to time for well-qualified economists interested in positions overseas. The positions are of two types. Some are for advisers in special areas, such as finance, taxation and budget. Others are for generalists to make continuing analyses of a country's economy to help guide the planning and execution of the foreign aid program for the country. Both types of positions require economists with sound theoretical background and experience in research or applied practice.

The minimum tour is for two years at the foreign post, but employees who make good are encouraged to make a career of the service. Candidates must have been citizens of the United States for at least five years. The candidate and all dependents • who will reside at the foreign post must pass complete physical examinations. Preferred ages are between 30 and 55. Base salaries for the vacancies that most frequently arise range from about $8,000 to $14,000. There are also a number of fringe benefits, including housing, medical care, educational allowances for children, differentials ranging to 25 per cent for some hardship posts, and home leave between tours. If interested—whether or not immediately available—send a résumé or preferably the Standard U.S. Government Employment Application (Form 57) to: Office of Personnel, International Cooperation Administration Box ER 1, Washington 25, D.C. Applications will be held confidential.

Business law and accounting: School of business administration in the nation's capital has opening for instructor or assistant professor, LL.B. and accounting B.S. required minimum. For 1959-60 academic year. P207

Production management: Instructor wanted in growing Rocky Mountain business administration department. Should have sound economic background and be able to teach wage and salary administration, job evaluation, middle management, etc. $5,000. P207

Undergraduate economics: Needed January, 1959. Instructor or assistant professor for fully accredited northern university, enrollment over 6,000. General sophomore economics and perhaps one or two advanced courses; 15 hour load; some evening school. Salary $4,500 to $6,500 for 9 months, depending upon education and stage of career. Should be Ph.D. or near, but experience or maturity are not required. Specialization or research not as important as good teaching ability. Permanent vacancy. P208

Economics: Young Ph.D., man or woman; opportunity to develop an undergraduate program; advancement based primarily on teaching ability. Midwestern liberal arts college for women; metropolitan area. P209